AN ANATOMY OF STORM SURGE SCIENCE AT LIVERPOOL TIDAL INSTITUTE 1919-1959: FORECASTING, PRACTICES OF CALCULATION AND PATRONAGE

A thesis submitted to the University of Manchester for the degree of

Doctor of Philosophy

in the Faculty of Life Sciences

2010

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[Final word count: 84,978 words in main text excluding footnotes, 109,784 words including footnotes]

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List of abbreviations

AAES	Anti-Aircraft Experimental Section
BAAS	British Association for the Advancement of Science
DSIR	Department of Scientific and Industrial Research
Hydro	UK Hydrographic Department (now UK Hydrographic Office or UKHO)
LCC	London County Council
LOTI	Liverpool Observatory and Tidal Institute
LSOA	Liverpool Steamship Owners Association
MAF	The Ministry of Agriculture and Fisheries
MAFF	The Ministry of Agriculture, Fisheries and Food
MDHB	Mersey Docks and Harbour Board
MHLG	Ministry of Housing and Local Government
NPL	National Physical Laboratory
PLA	Port of London Authority
TI	Tidal Institute

Archival material abbreviations

I have used Whitaker's Almanack for the relevant year to identify full names and titles for officers within Whitehall departments.

BA: Bidston Archive, Liverpool World Museum, National Museums and Galleries on Merseyside, Liverpool

Doodson papers: Liverpool World Museum, National Museums and Galleries on Merseyside, Liverpool

NA: The National Archive, Kew, London

LMA: London Metropolitan Archive, London

LUA: Liverpool University Archive, Liverpool University Sidney Jones Library, Liverpool

MMM: Merseyside Maritime Museum, National Museums and Galleries on Merseyside, Liverpool. The addition of 'North Street' signifies that the material is held in their reserve store.

RSA: Royal Society Archive, London

UKHO: United Kingdom Hydrographic Office Archive, Taunton

LIST OF SPECIALISED TERMS

Baroclinic vs barotropic conditions: "[B]arotropic conditions exist when density is a function of pressure alone so that lines of equal pressure (isobars) and of equal density (isopycnals) are parallel. Under baroclinic conditions, variation in properties (in the ocean) such as temperature or salinity causes isobars and isopycnals to be inclined to each other, resulting in dynamic instability and the possibility of motion, that is, current flow."[1]

Chains of documents: Sheets of papers with inscriptions, often taking specific forms or shapes such as graphs or tables but also freeform notes, that are linked to other such sheets and other things through transformations and re-inscriptions. For example, one document in such a chain could be a roll of paper from a tidal gauge, which connects to the sea level through inscriptions. The graphs inscribed on these rolls can then be re-inscribed as tables on another document. The tidal gauge roll and the table would form one link in a chain of documents. These chains of documents are similar to Latour's chains of elements or his description of n-th order forms,[2] but with more emphasis on the contingency of their construction and the limitations imposed by the chains.

Coriolis force: Also known as the geostrophic force, it is a name for the effect of the rotation of the earth on the apparent direction of the wind. From the point of view of a stationary observer on the rotating earth, this effect deflects the wind towards the right in the northern hemisphere and the left in the southern hemisphere.

Correlation: The extent to which variables vary together. Correlation does not necessarily imply causation.

Geostrophic wind: The geostrophic wind is an approximation of the wind high up in the atmosphere, say at 1000m height, as it assumes negligible friction. However, it is calculated using ground-level pressure measurements. It also assumes a balance between the Coriolis force and the pressure gradient. The approximation only applies when the isobars are straight, but if this holds the geostrophic wind can be directly calculated

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[1] Eric L Mills, *The Fluid Envelope of Our Planet : How the Study of Ocean Currents Became a Science* (Toronto: University of Toronto Press, 2009), 311 n382.
[2] Bruno Latour, *Pandora's Hope: Essays on the Reality of Science Studies* (Cambridge, Massachusetts: Harvard University Press, 1999), 70; Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, Massachusetts: Harvard University Press, 1987), 232-247.

from pressure gradients on meteorological charts and is of constant speed and parallel to the isobars on such charts.[3]

Gradient wind: Like geostrophic wind but taking into account the curvature of the isobars, such as around the centre of a low.

Harmonic analysis: "[T]he representation of tidal variations as the sum of several harmonics, each of different period, amplitude and phase. The periods fall into three tidal species, long-period, diurnal, semidiurnal. Each tidal species contains groups of harmonics, which can be separated by analysis of a month of observation. In turn, each group contains constituents, which can be separated by analysis of a year of observations. In shallow water, harmonics are also generated in the third-diurnal, fourth-diurnal and higher species. These **[harmonic] constituents** can be used for harmonic prediction of tides."[4]

Isobars: Lines of equal barometric pressure, as drawn on meteorological charts.

Multiple regression: A statistical technique to analyse correlation between several variables. In particular, it can be used to produce a formula to predict the value of one variable from other variables.

Practices of calculation: Methods, technologies and management practices used in the performance of calculations and other mathematical work; the embodied and material aspects that make up computational, statistical and mathematical work.

Tides, diurnal (semi-diurnal): Tides with one (two) high tide(s) per lunar day.[5]

Tide, high (low), or high (low) water: The highest (lowest) tidal level in a cycle.[6]

Tides, spring (neap): "The tides of greatest (least) amplitude in a 15-day cycle".[7] Less formally, the phrase is sometimes used to refer to the time of the month when periodic tides are at their highest (lowest).

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[3] Ray Sanderson, *Meteorology at Sea* (London: Stanford Maritime, 1982), 32-37; Jerome Williams, John J. Higginson, and John D. Rohrburgh, *Sea & Air : The Marine Environment* (Annapolis, Maryland: Naval Institute Press, 1973), 142-145.

[4] David Pugh, *Changing Sea Levels: Effects of Tides, Weather and Climate* (Cambridge: Cambridge University Press, 2004), 244. Some formatting amended.

[5] David Edgar Cartwright, *Tides: A Scientific History* (Cambridge: Cambridge University Press, 1999), 274.

[6] Pugh, *Changing Sea Levels*, 244-245.

[7] Cartwright, *Tides*, 275.

ABSTRACT

Abstract for a PhD thesis submitted in December 2010 at the University of Manchester by Anna Elisabeth Carlsson-Hyslop titled "An anatomy of storm surge science at Liverpool Tidal Institute 1919-1959: Forecasting, practices of calculation and patronage"

When the effects of wind and air pressure combine with a high tide to give unusually high water levels this can lead to severe coastal flooding. This happened in England in early 1953 when 307 people died in the East Coast Flood. In Britain today such events, now called storm surges, are forecast daily using computer models from the National Oceanographic Centre in Liverpool, formerly the Liverpool Tidal Institute (TI). In 1919, when TI was established, such events were considered unpredictable. TI's researchers, Joseph Proudman (1888-1975), Arthur Doodson (1890-1968), Robert Henry Corkan (1906-1952) and Jack Rossiter (1919-1972), did much mathematical work to attempt to change this. In 1959 Rossiter published a set of statistical formulae to forecast storm surges on the East Coast and a national warning system was predicting such events using these formulae. At this point TI believed they had made surges at least as predictable as they could with their existing methods. This thesis provides a narrative of how this perceived rise in the predictability of surges happened, analysing how TI worked to achieve it between 1919 and 1959 by following two interwoven, contingent and contested threads: practices of calculation and patronage.

A key aspect of this thesis is the attention I pay to material practices of calculation: the methods, technologies and management practices TI's researchers used in their mathematical work on storm surge forecasting. This is the first study by historians of oceanography or meteorology that pays this detailed level of attention to such practices in the construction of forecasting formulae. As well as using published accounts, I analyse statistical research in the making, through notes, calculations, graphs and tables produced by TI's researchers. They used particular practices of calculation to construct storm surges as calculable and predictable scientific objects of a specific kind. First they defined storm surges as the residuals derived from subtracting tidal predictions from observations. They then decided to use multiple regression, correlating their residuals with pressure gradients, to make surges predictable. By considering TI's practices of calculation the thesis adds to the literature on mathematical research as embodied and material, showing how particular practices were used to make a specific phenomenon predictable.

I combine this attention to mathematical practice with analysis of why TI's researchers did this work. US historians have emphasised naval patronage of physical oceanography in this period but there is very little secondary literature for the British case. The thesis provides a British case study of patronage of physical oceanography, emphasising the influence on TI's work not only of naval patronage but also of local government, civil state and industrial patronage. Before TI's establishment Proudman argued that it should research storm surges to improve the Laplacian theory of tides. However, when the new Institute received patronage from the local shipping industry this changed and the work on forecasting surges was initially done as part of a project to improve the accuracy of tidal predictions, earning TI further patronage from the local shipping industry. After a flooding event in 1928 the reasons for the work and the patronage again shifted. Between then and 1959 TI did this work on commission from various patrons, including local government, civil state and military actors, which connected their patronage to national debates about state involvement in flood defence. To understand why TI's researchers worked on forecasting surges I analyse this complex mix of patrons and motivations. I argue that such complex patronage patterns could be fruitfully explored by other historians to further existing debates on the patronage of oceanography.

DECLARATION

No portion of the work referred to in the thesis has been submitted in support of an application for another degree or qualification of this or any other university or other institute of learning.

Differently edited versions of some of the work in chapter 5 and 6 appears in two publications by me:

Anna Carlsson-Hyslop, "Storm surge science – the London connection 1928-1953", *Tides and floods: New research on London and the tidal Thames from the middle ages to the twentieth century,* Ed. J.A. Galloway, Centre for Metropolitan History Working Paper Series no. 4, (London: Institute of Historical Research, 2010)

and

Anna Carlsson, "What is a storm: severe weather and public life in Britain in January 1928", *Weather, Local Knowledge and Everyday Life: Issues in Integrated Climate Studies*, Eds. Vladimir Jankovic and Christina Barboza, (Rio de Janeiro: MAST, 2009)

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Acknowledgments

First and foremost, many thanks to my supervisory team: Jeff Hughes, Vladimir Jankovic and James Sumner. Thanks also to others such as my advisors Lyn Schumaker and Mick Worboys, Mark Elliot, Jay Kennedy, Bill Luckin, Simone Turchetti, Mary Walsh and many others who have answered questions, provided support and comments. In particular I would like to thank various friends for reading through and commenting on chapters, especially Cavus Batki, Jasmin Blanchette, Dan Carlsson-Hyslop, Raphael Hyslop and Sofia Larsson. More generally, but no less important, thanks also to my fellow PhD students and the other staff at CHSTM for providing a supportive atmosphere and pleasant company!

I also owe many thanks to staff at archives and libraries, such as the library at the National Oceanographic Centre (Liverpool), John Rylands Library at the University of Manchester, the Churchill Archives Centre at Churchill College, the archive at UKHO (the Hydrographic Office), the National Archive, the Royal Society Archive, the Merseyside Maritime Museum Archive, the Liverpool University Archive and the London Metropolitan Archive. Without doubt my greatest archival debt lies with Marianne Elliott and Alan Bowden at the Liverpool World Museum, for allowing me access to the Bidston Archive and the Doodson papers.

I would also like to thank members of the National Oceanography Centre (Liverpool) for talking to me at various points during my project, especially Kevin Horsburgh, John Eric Jones and Philip Woodworth. Others who have been similarly enthusiastic and supportive include Greg Bankoff, Hugh Deeming, David Demeritt, James Fleming, James Galloway, Paul Hughes, Mike Hulme, Eric Mills, James Porter, Samuel Randalls and Michael Reidy, and participants and organisers of various conferences, especially the British Commission for Maritime History New Researchers Conference 2010, the Weather, Climate and Everyday Life Conference, and the conference London, the Thames and Water: new historical perspectives – thank you all!

In addition, I would like to thank the Economic and Social Research Council for providing me with funding and Mick Worboys for providing further assistance with financial matters.

Last, but certainly not least, much deserved thanks go to Dan for untold number of dinners, jokes and other unquantifiable support.

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	2009	"What is a storm: severe weather and public life in Britain in January 1928", *Weather, Local Knowledge and Everyday Life: Issues in Integrated Climate Studies*, Eds. Vladimir Jankovic and Christina Barboza, (Rio de Janeiro: MAST, 2009)
	2007	"The geography of scientific culture in early nineteenth-century Britain: the case of Bamburgh Castle Library", *Library History*, Vol. 23, Sep 2007

CHAPTER 1, INTRODUCTION

If the effects of wind and air pressure combine with a high tide to give unusually high water levels this can lead to severe coastal flooding. This happened during a flooding event in early 1953 when more than 300 people died on the East Coast of England. Today these 'storm surges' are seen as predictable flows of sea water that are forecast daily and linked to a nationwide warning system. The forecasts are run by the Met Office for the Environment Agency and use complex computer models produced at the National Oceanographic Centre – Liverpool, the successor of the Liverpool Tidal Institute (TI). In early November 2007, when the models predicted a sea level of a similar height to that of 1953 on the East Coast of England, the government's emergency committee COBRA met. In the end only a little flooding took place, as the wind and thus the height of the surge was lower than the worst predictions, but the event exemplifies the high levels of government concern about, and investment in, storm surge forecasting today.

In 1919, when TI was established, 'meteorological effects on sea level' as they were then known were not forecast on a regular basis. Indeed many, such as the late George Howard Darwin, deemed such effects to be unpredictable. These potentially catastrophic flows of sudden and unpredictable sea water were recurrent, yet out-of-the-ordinary and irregular events. TI's researchers set out to change this. In 1959 a set of statistical forecasting formulae for storm surges was published by one of them and a warning system, using these formulae amongst other things, was forecasting such events on the East Coast of Britain. At this point TI believed they could predict surges, or at least as well as possible with current technology and methods. This thesis looks at how this rise in perceived predictability happened; something which has not previously been studied by historians. How were surges made (more) predictable by TI between 1919 and 1959?

One key aspect of how TI's researchers made surges more predictable was by introducing new practices of calculation, by which I mean that they introduced new methods, technologies and management practices to perform calculations. These practices of calculation produced particular chains of documents, i.e. sheets of papers with inscriptions on them linked to other such documents – an example of one link in such a chain could be the re-inscription of tidal gauge graphs into a table. TI's researchers attempted to make disorderly sea water into predictable storm surges by first defining surges as the residual – the difference between observed and predicted sea level – and then constructing formulae to forecast such residuals. To produce the forecasting formulae they used particular mathematical, often statistical, practices. Their particular practices of calculation constituted surges as calculable and, eventually, predictable phenomena, in a contested and contingent process of constructing chains of documents.

The construction of surge forecasting formulae by TI's researchers was closely related to the resource gathering and institution building they took part in, as well as the patronage given to them by those who wanted predictions of surges (and more generally increased control over nature). Debates over patronage were a key source of contingency in TI's work on storm surges which at times impacted on their practices. Another obvious contingency was the influence of major surge events in promoting work in the area, particularly in 1928 and 1953. TI also made choices regarding which practices of calculation to use and how to organise the documents and formulae they worked with. These choices were contingent on a range of issues such as the training of their workers, earlier work and practice, computational limits and specific demands from patrons.

This thesis analyses the work TI did on understanding and forecasting meteorological effects between 1919 and 1959. Initially this work was done not to forecast flooding but to attempt to provide 'corrections' to the published tidal predictions for the benefit of the Liverpool shipping industry, which was the patron of the work providing funding and other support. The primary concern of TI's patrons then was not with *higher* than expected water levels but with *lower* than expected water levels, which could lead to ships stranding. The work on meteorological effects was part of a programme of increasing the accuracy of tidal predictions using new practices of calculation, which was linked not only to the researchers' earlier work but also to the increasing size of ships and the impact of the First World War. As part of this work storm surges were defined as residuals, the difference between observed and predicted sea level, and statistical methods were used to try to make them predictable in Liverpool's port.

The narrative traces the work done on storm surges at TI from its establishment to the late 1950s and how this was affected by various events and decisions. When fourteen people died in a flood event in London in 1928 the early programme of work changed, shifting towards forecasting flooding. The patron of the work also shifted, towards local government, and in particular local authorities in London. During the Second World War a request from the Hydrographic Department of the Admiralty (Hydro) for a storm surge forecasting formula led to renewed emphasis on statistical practices in TI's surge work. After the war, the work for the local authorities continued and, in an odd twist, the report that resulted from this work was printed in a very limited edition by the US Navy in the late 1940s.

The 1953 flood event was another turning point for TI's work on surges. After this event central government became the largest patron of storm surge science and with this new patronage TI constructed statistical forecasting formulae that were taken up by the newly established warning system. In 1957 TI declared they had produced as good as possible forecasting formulae of the statistical form they had been honing for many years, and, after a couple of years testing, those in charge of the warning system at Hydro agreed. However, TI had already begun doubting the validity of their own practices and the formulae they produced a couple years earlier. At the same time a dispute with the Met Office regarding TI's methods for calculating the effect of the wind had broken out. After these two simultaneous events TI began arguing that if more 'accurate' formulae were wanted for the warning system, shifts in their practices of calculation were needed, away from their previous statistical methods. They began one such shift in 1959, when digital computers were introduced into their storm surge work for the first time, which marks the end of the thesis.

This thesis does two things. Firstly, it looks at the work done at TI, concentrating on how it performed its mathematical calculations. The researchers at the Institute were trained as mathematicians and one of them, Arthur Thomas Doodson (1890-1968), had worked with Karl Pearson, the statistician, and with anti-aircraft ballistics computations during the First World War. The other, Joseph Proudman (1888-1975), had studied mathematics at Liverpool and Cambridge universities. TI's disciplinary links were thus initially primarily to mathematics and mathematical physics. The thesis concentrates on one recurrent aspect of TI's work: mathematical research on forecasting storm surges. It also discusses some of their work on periodic tides. In particular, the thesis focuses on the practices of calculation used in the storm surge work, which were frequently statistical and computational. Much of TI's work was set within a particular tradition of mathematical analysis fostered at Cambridge and in particular a tradition of tidal analysis with links to Pierre-Simon Laplace's work. At TI this tradition was developed together with new ways of organising and performing precision calculations, brought from Doodson's wartime work. The storm surge work depended on the use of computational aids, such as tables, calculating machines and tidal predictors, and particular management methods to produce chains of documents linked to other documents. TI's work to predict tides and storm surges provides a case study of the use of particular practices of calculation on a particular phenomenon to produce particular chains of documents. As such, this study adds to the small but growing literature on the practices involved in mathematical work, emphasising the physical materiality of such research, for example the use of technologies of calculation. In particular I analyse TI's work as that of constructing documents in linked chains, emphasising the importance of contingent choices during the construction process.

Secondly, the thesis provides a case study of an institute and a research topic that today would be considered oceanographic, concentrating on the patronage structure of the two. Most work on the history of physical oceanography has emphasised the importance of naval patronage and rarely focused on direct industrial or civil state patronage. While TI was dependent for its income on sales of tidal predictions to the Navy, in the shape of Hydro, it was equally dependent on industrial patronage from the local shipping industry for further financial support and a building. The third element of TI's patronage structure was Liverpool University, providing connections to academia. Like a three-legged stool, TI relied on all three legs for patronage – if one leg had broken, the whole structure is likely to have fallen down. For example, while most of TI's funding was provided by the shipping industry in the early 1920's, Hydro then commissioned TI to do tidal predictions because of its connections with the university and because TI was neither a state organisation nor a private business. The three elements of TI's patronage structure were interconnected and interdependent. From the standpoint of most history of physical oceanography the patronage of storm surge science is equally unusual: first shipping industry, then local government and only after 1953 civil state patronage, with some, but only some, naval involvement throughout. The thesis thus provides a case study of research into physical oceanography for which naval patronage was only one part of the story, arguing for increased attention to non-naval patronage in the history of this field.

Throughout the thesis I emphasise the contested and contingent nature of patronage TI received from different actors. TI did not receive 'naval' or 'state' patronage; they received patronage from one particular part of one department of the Admiralty, the tidal branch of Hydro, and later one particular department of civil central government, the Ministry of Agriculture and Food. In addition, the patronage they received was frequently contested. Securing patronage involved TI's researchers and supporters in complex negotiations and intense arguments about a wide range of issues, such as the accuracy of tidal predictions, the type of oceanographic science state actors should support and, of course, much institutional politics. Such politics was not only an issue within individual institutions and departments but also between local and central government, between Liverpool University and the local shipping industry, and between different groups of scientists. While TI's work on predicting tides and surges was aimed at increasing the predictability of nature, different actors disagreed not only on whether this aim was necessary but also on how to achieve it.

The two themes of patronage, especially the gradual increase of state patronage, and TI's use of statistical practices of calculation to make surges calculable, tell a story of how a particular phenomena was 'tamed' by scientists for their patrons using statistics.[1] It also provides yet another example of the gradual rise of technological and scientific governance of nature during the twentieth century, but focuses on the details of how this rise happened rather than on the governance that eventually came out of the work.[2] This is partly because the formulae that resulted from TI's work on constructing surges as potentially predictable were not put into use until after the flooding in 1953. Before then TI primarily used their forecasting formulae to promise increased predictability, or governance, of water; promises that only some actors were interested in. While the thesis discusses the contested increase in the number of actors interested in governing the sea, the primary focus of it is *how* storm surges were made calculable and predictable, or governable. This was done through the use of particular practices of calculation funded by particular actors for particular reasons. To study why and how surges were made calculable is a necessary foundation to understand the increase in

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[1] Compare Ian Hacking, *The Taming of Chance* (Cambridge: Cambridge University Press, 1990), 22.
[2] W Harry G Armytage, *The Rise of the Technocrats* (London: Routledge and Kegan Paul, 1965); David Edgerton, *Warfare State: Britain, 1920-1970* (Cambridge: Cambridge University Press, 2006). For a recent example of historical work describing the rise of governance of nature, see Mark Whitehead, *State, Science, and the Skies: Governmentalities of the British Atmosphere* (Chichester: John Wiley & Sons, 2009).

governance of the sea. Without understanding how TI tried to make surges predictable before 1953 we cannot understand how particular state actors attempted to govern it using TI's formulae for the warning system after 1953. However, for a full picture of this increase in governance this study should be complemented by wider studies of how different parts of society have attempted to govern the sea, e.g. by building coastal defences.

This introduction will now situate the thesis within the contexts of the literature on the history of oceanography, meteorology and on practices of calculation. First, however, it will introduce storm surges in a little more detail. I then review the literature after which I provide an overview of the thesis.

1.1 Present-day context: what are 'storm surges' and why do they matter today?

What were the events that the workers at TI tried to forecast? Today they are called 'storm surges'. In 1919 they were called meteorological effects, which is a more descriptive term. Scientists today define storm surges as relatively rapid changes in sea level due to wind and atmospheric pressure that affect the regular periodic tidal pattern.[3] This thesis focuses on storm surges as they affect England, i.e. extra-tropical storm surges.[4]

According to recent descriptions by scientists, surges form when "the atmosphere forces the water body, which responds by generating oscillations of the water level with various frequencies and amplitudes".[5] Such an oscillation is then transported by the wind until it comes into contact with a coast either nearby (leading to a 'local' surge) or far away (an 'external' surge). A complex combination of causes decide the period and level of storm surges, such as the direction, speed and duration of the wind, the change

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[3] Two introductory texts to tides, prediction and surges are John D. Boon, *Secrets of the Tide : Tide and Tidal Current Analysis and Applications, Storm Surges and Sea Level Trends* (Chichester: Horwood Publishing, 2004); Pugh, *Changing Sea Levels*.

[4] More extreme storm surges are formed in tropical waters. These can be extremely deadly and/or destructive, for example during the 1991 Bangladesh cyclone, the 1970 Bhola cyclone or the 2005 hurricane Katrina.

[5] Gabriele Gönnert *et al.*, *Global Storm Surges*, ed. German Coastal Engineering Research Council, Archive for Research and Technology on the North Sea and Baltic Coast (Holstein: Westholsteinische Verlagsanstatl Boyens & Co., 2001), 7.

in atmospheric pressure, the layout of the coastline and seabed, the track and intensity of the storm, and the rotation of the earth.[6] The southern North Sea, including England's East Coast, is sensitive to storm surges due to its semi-enclosed funnel shape which intensifies the height of the surge. Like the ordinary tide, surges travel southwards along England's east coast. As they also both travel at about the same speed, due to both being similar kinds of travelling waves, a surge that coincides with high tide will affect most of this coast, as it did in 1953. Storm surge flooding has been recorded throughout European history, often with severe consequences in terms of loss of land or lives.[7]

One reason to study the history of storm surge science is the context of climate change. According to the most recent research storm surges themselves are not predicted to become statistically significantly more frequent or severe due to climate change, but the Intergovernmental Panel on Climate Change expects sea level to rise.[8] This increase in sea level means that the impacts of storm surge events are likely to become more pronounced, leading to increased risk of flooding and erosion. Any climate related sea level changes will come on top of the gradual geological changes that have affected Britain since the last glaciation, with some areas, notably London and the South, sinking, while others rise. Such sea level changes, and the North Sea's susceptibility to meteorological effects, mean the British have a long history of dealing with coastal flooding and erosion. Their responses to surges have varied across time and circumstances. For example, the number of storm surges in the Thames Estuary increased in the Middle Ages, causing loss of land. Considerable resources were initially spent to combat this, but as labour costs increased after the Black Death some retreat from the threatened land took place instead.[9] While today there is much policy interest

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[6] The rotation of the earth matters as the highest storm surges do not necessarily result from winds perpendicular to the coast, as the coriolis effect leads to what is known as Ekman transport when wind-drifted water is deflected up to 45° away from the wind direction (to the right in the Northern hemisphere). Ibid., 2-4; Keith Smith and Roy Ward, *Floods: Physical Processes and Human Impacts* (Chichester: John Wiley & Sons, 1998), 151; Pugh, *Changing Sea Levels*, 139-141.

[7] Smith and Ward, *Floods: Physical Processes and Human Impacts*.

[8] IPCC, "Climate Change 2007: Synthesis Report, Summary for Policymakers", 2007 p. 8, http://www.ipcc.ch/pdf/assessment-report/ar4/syr/ar4_syr_spm.pdf accessed 30th November 2009. Jason Lowe *et al.*, *UK Climate Projections Science Report: Marine and Coastal Projections* (Exeter: Met Office Hadley Centre, 2009).

[9] James A Galloway and Jonathan S Potts, "Marine Flooding in the Thames Estuary and Tidal River C.1250-1450: Impact and Response," *Area* 39, no. 3 (2007).

in coastal flooding,[10] and much scientific work on sea level change and storm surges,[11] there is as yet little historical work on how the British have previously dealt with coastal flooding, or with other types of extreme weather.[12] This thesis looks closely at how a particular understanding of meteorological effects, as storm surges that could be forecast using statistics, developed, which was one key way in which British society dealt with storm surges in the period 1919-1959. It thus aims to contribute towards providing a historical analysis of adaptation to extreme weather and climate change, particularly the use of science in twentieth century adaptations.

I will now discuss literature relevant to the thesis, covering in turn literatures related to patronage of oceanography, forecasting of extreme meteorological events, differences in the type of mathematics used by different oceanographers and, finally, practices of mathematics, including a discussion of accuracy.

1.2.1 PATRONAGE OF OCEANOGRAPHIC SCIENCE

How and why TI attempted to make surges predictable was closely linked to who their patrons were. Patronage is a major topic of interest in history of science as a lens through which to analyse a range of issues. Ronald Doel discusses a range of questions that studies of patronage can be concerned with: What was the consequence of patronage for research? What was its influence on institutions? What research did it NOT allow? How did it link to politics? How did it influence the community and culture?[13] In short, patronage is not just about money, but also about values, research and identity. As Mario Biagioli puts it, analysing patronage "is the key to understanding processes of identity and status formation that are the keys to understanding *both* the scientists' cognitive attitudes *and* career strategies".[14] The funding of science is involved

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[10] For example the Pitt Review after the summer 2007 floods and DEFRA's recent publication of both "Adapting to Coastal Change: Developing a Policy Framework" and the "UK Marine Science Strategy"
[11] Specific programmes of work include Coastal Flooding by Extreme Events (CoFEE), Oceans 2025 (especially the Sea Level and Vertical Land Movement work package 1.9) as well as continuing work on the storm tide warning system.
[12] See also Roderick J. McIntosh, Joseph A. Tainter, and Susan Keech McIntosh, "Climate, History and Human Action," in *The Way the Wind Blows : Climate, History and Human Action*, ed. Roderick J. McIntosh, Joseph A. Tainter, and Susan Keech McIntosh (New York: Columbia University Press, 2000).
[13] Ronald E. Doel, "Constituting the Postwar Earth Sciences: The Military's Influence on the Environmental Sciences in the USA after 1945," *Social Studies of Science* 33, no. 5 (2003).
[14] Emphasis in original. Mario Biagioli, *Galileo, Courtier: The Practice of Science in the Culture of Absolutism* (London: The University of Chicago Press, 1993), 14.

in the careers, identities and work of scientists, and this was certainly the case with storm surge science at TI.

This section introduces literature on the history of oceanography, and to a lesser extent allied sciences, concentrating on patronage issues in the period from the First World War to the late 1950s. It argues that US historians of physical oceanography have concentrated heavily on military patronage, de-emphasising industrial patronage which was very important at TI. It also argues that what little work has been done in regards to twentieth century history of UK oceanography has focused more on its deficiencies than its actual work. By oceanography I mean research concerned with the sea.

1.2.2 Military patronage of US earth science

Analysts of the patronage pattern of earth sciences in the US have argued that military patronage has been very important for physical earth sciences, such as physical oceanography and meteorology, especially in the post-war period.[15] Chandra Mukerji has identified oceanography as an extreme case within the sciences with an unusually high dependency on government funding. She argues that in the post war period oceanographers were given funding so that they would be a reserve labour force to support policy or in case of need, e.g. during war.[16] Naval patronage of physical oceanography has often been linked to the two world wars and the Cold War, though different authors identify different trigger points. For example, Gary Weir has studied the relations between the US Navy and oceanographers from the First World War to the Second World War and beyond. He argues that in the US the First World War led to military interest in physical oceanography, especially for detection of submarines. After the First World War lack of funding for both the Navy and academic oceanography led to a partnership between the Navy, especially the US Hydrographic Office, and academic oceanographers, with the Navy offering practical support such as

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15 Doel, "Constituting the Postwar Earth Sciences."; Kristine C. Harper, *Weather by the Numbers : The Genesis of Modern Meteorology* (London: The MIT Press, 2008).
16 Chandra Mukerji, *A Fragile Power: Scientists and the State* (Princeton: Princeton University Press, 1989).

space on ships and also some monetary support. He argues that this relationship continued and grew in strength during the Second World War and beyond.[17]

Others have argued that the Second World War was an important trigger in the rise of naval patronage of science. Ronald Rainger, for example, has argued that the patronage pattern of the Scripps Institute of Oceanography shifted from a mixed pattern of patronage and oceanographic research in the interwar period, towards a strong focus on military patronage and physical oceanography after the Second World War.[18] Jacob Darwin Hamblin in turn identifies a different trigger point. In his view naval patronage of oceanographic research had only a limited start during the war when an "awakening" of the mutual benefits of co-operation between Navy and oceanographers took place.[19] Instead he argues that the Navy was not thoroughly supportive until the first half of the 1950s, when they on the suggestion of scientists through the Hartwell report began to develop atomic submarine warfare and defence. This was done to ensure the Navy's role within the larger US military and administration, which he argues was pro-air force at this time. This, according to Hamblin, led to a much higher level of naval patronage of oceanography during the 1950s.[20] Despite the identification of different trigger points, these US historians of oceanography agree that naval patronage was very important for oceanography, especially physical, in the period covered by this thesis.

1.2.3 British oceanography and state patronage of science

Little historical work has been done on UK twentieth century oceanography and what has been done has emphasised its supposed deficiencies and communication problems with the Navy. I will question both these assumptions in this thesis. For example, Hamblin mentions Edward Crisp Bullard as a British case study of somebody doing

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[17] Gary E. Weir, *An Ocean in Common: American Naval Officers, Scientists, and the Ocean Environment* (College Station, Texas: Texas A&M University Press, 2001).
[18] Ronald Rainger, "Constructing a Landscape for Postwar Science: Roger Revelle, the Scripps Institution and the University of California, San Diego," *Minerva* 39, no. 3 (2001); Ronald Rainger, "Adaptation and the Importance of Local Culture: Creating a Research School at the Scripps Institution of Oceanography," *Journal of the History of Biology* 36, no. 3 (2003). See also Naomi Oreskes and Ronald Rainger, "Science and Security before the Atomic Bomb: The Loyalty Case of Harald U. Sverdrup," *Studies In History and Philosophy of Science Part B: Studies In History and Philosophy of Modern Physics* 31, no. 3 (2000).
[19] Jacob Darwin Hamblin, *Oceanographers and the Cold War : Disciples of Marine Science* (London: University of Washington Press, 2005), 9-10.
[20] Ibid., ch 2.

oceanography in interwar Britain, but takes Bullard's words at face value when the latter claimed that British oceanographers were worse off than the Scandinavians in terms of support for their deep-sea work, with Hamblin seemingly arguing that not much 'good' oceanography was done in the interwar period in Britain due to lack of state support.[21] TI provides a case study of an oceanographic institution in Britain that found enough patronage to establish itself in this period. In addition the so-called Discovery Committee was doing considerable amounts of oceanographic research in the Southern Oceans in the interwar period. Its work, which was run by the Colonial Office, focused on research related to the economic resources of the Antarctic, especially whales.[22] Bullard's comment was made in the context of discussions regarding what geophysical – not oceanographic – research should be emphasised in a Royal Society report on the 'needs' of geophysics after the Second World War.[23] Bullard's claim may thus be better seen as that of a public scientist arguing for further financial support from the state for his geophysical science.[24]

Margaret Deacon's work on twentieth century oceanography provides another example of how historians have argued that oceanography was weak in the UK in the interwar period and also that communications problems between the Navy and civilian scientists limited the work.[25] Recently she stated that "the nation that sent out the *Challenger* Expedition seemed largely to have lost interest in oceanic research, especially physical

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21 Ibid., 8-11. Bullard makes the relevant statement in Bullard to Darwin, 1st Mar 1946, BLRD F.85, Churchill Archives Centre, Churchill College, Cambridge

22 Margaret Deacon, "Marine Science in the UK before World War II," in *Of Seas and Ships and Scientists: The Remarkable Story of the UK's National Institute of Oceanography 1949-1973*, ed. Anthony Laughton*, et al.* (Cambridge: The Lutterworth Press, 2010). This contradicts Clarke's statement that the Colonial Office had not provided significant funds to fisheries research before the 1940s, see Sabine Clarke, "A Technocratic Imperial State? The Colonial Office and Scientific Research, 1940 - 1960," *Twentieth Century British History* 18, no. 4 (2007): 474.

23 "List of projects" and various draft minutes and reports re Post-War needs in Geophysics Committee from 1944, BLRD F.86, and Bullard to Darwin, 1st Mar 1946, BLRD F.85, Churchill Archives Centre, Churchill College, Cambridge

24 CMB 101, AE/1/2/3 & AE/1/9/4, Royal Society, shows Bullard's involvement in various Royal Society Committees on Geodesy and Geophysics. For more on public scientists see Edgerton, *Warfare State*; Andrew Hull, "Passwords to Power - a Public Rationale for Expert Influence on Central Government Policy-Making: British Scientists and Economists, c. 1900 - c. 1925" (PhD, University of Glasgow, 1994); Andrew Hull, "War of Words: The Public Science of the British Scientific Community and the Origins of the Department of Scientific and Industrial Research, 1914-1916," *The British Journal for the History of Science* 32, no. 4 (1999).

25 However, Deacon's main work on the history of oceanography has concentrated on developments before the twentieth century, see Margaret Deacon, *Scientists and the Sea 1650-1900: A Study of Marine Science* (London: Academic Press, 1971).

oceanography, in the first part of the twentieth century",[26] blaming this on lack of money. Despite stating this lack of interest in and support for oceanographic science, she then goes on to list work in biological oceanography and naval science, providing examples of work actually done. She briefly mentions TI as a "notable exception" to what she says is the otherwise poor record of UK oceanography in the first half of the twentieth century.[27] Earlier work of hers provides further examples of oceanographic work done in co-operation between the Navy and oceanography in the UK. She describes how Hydro survey ships were used by oceanographers for field work before the First World War, and how during the war this co-operation increased. At the end of the war plans were made to permanently incorporate oceanographic work into the Admiralty. This was said to be "in the national interest" due to the increasing importance of submarines. While these plans came to little, Deacon discusses how other oceanographic research, e.g. on tidal streams, was carried out by the Navy in the early 1920s.[28] Willem Hackmann's book on the history of sonar provides further examples of such naval oceanographic work.[29] Despite detailing quite substantial amounts of oceanographic work being done, both Hackmann and Deacon argue that such work was limited, in part by problems of communication with traditional Navy personnel struggling to accept civilian advice.[30]

David Edgerton has argued against such views in his analysis of the relations between military, state and science in twentieth century Britain. He focuses on military-related state-supported research in Britain from 1920 to 1970, arguing that the British state spent large sums of money on military-related research and development in this period. He has adopted the concept 'warfare state' to summarise his views. For the interwar period he argues that Britain's military remained well funded, and that, for example, the Navy was at least as strong, if not stronger, than other countries' navies, including in the area of R&D.[31] While much literature on military patronage of science focuses on the Cold War period, Edgerton argues that there were close links between the military, state, industry and science in Britain well before the Second World War, and that much

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[26] Italics as in original. Deacon, "Of Seas and Ships and Scientists," 19.
[27] Ibid., 22.
[28] Margaret B. Deacon, "G. Herbert Fowler (1861-1940): The Forgotten Oceanographer," *Notes and Records of the Royal Society of London (1938-1996)* 38, no. 2 (1984): 273.
[29] Willem Hackmann, *Seek & Strike : Sonar, Anti-Submarine Warfare and the Royal Navy, 1914-54* (1984), 11-43.
[30] Ibid; Deacon, "G. Herbert Fowler."
[31] Edgerton, *Warfare State*, 21-33.

new technology and innovations came from state/military/industrial research establishments in the interwar period.[32]

What I take from Edgerton is an emphasis on tracing the actual extent of military and other state patronage of British science, such as TI's, in this period. As historians we should go beyond scientists' statements that the military and state did not provide sufficient support and that British science thus suffered. Like Weir has argued for the US, and Deacon and Hackmann show for Britain, there were at least some connections between the academic and naval oceanographic communities from the time of the First World War. However, unlike Edgerton, my focus is not on the state's relationship to military and science, but on a particular and rather peculiar institute which had links not only to specific departments of the military and civil state, including local government but also to industrial and academic actors. Edgerton argues that oceanography was a state science, but the work TI did was more than this.[33]

1.2.4 NON-NAVAL PATRONAGE

The concentration on naval patronage in much of the history of oceanography is useful and will be followed up in this thesis; however TI's patronage was often not military but instead industrial. Close links between industry and science have been described in many other branches of science and this source of patronage for oceanography should no less be ignored than military patronage.[34] While there are some exceptions to the heavy emphasis on naval patronage, particularly of biological oceanography, historians of physical oceanography have until recently rarely concentrated on inter-linkages with industry in the discipline, and instead concentrated on state patronage.[35]

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[32] Ibid., 122.
[33] Ibid., 112.
[34] See for example Jeff Hughes, "Plasticine and Valves: Industry, Instrumentation and the Emergence of Nuclear Physics," in *The Invisible Industrialist*, ed. Gaudillere and Lowy (1998); Robert Fox and Anna Guagnini, "Introduction," in *Education, Technology and Industrial Performance in Europe, 1850-1939*, ed. Robert Fox and Anna Guagnini (Cambridge: Cambridge University Press, 1993), 7-8; Karl Grandin, Nina Wormbs, and Sven Widmalm, eds., *The Science-Industry Nexus : History, Policy, Implications*, Nobel Symposium 123rd (2002) (Stockholm: Science History Publications/USA 2004); Jean-Paul Gaudillière, "The Invisible Industrialist : The Technological Dynamics of 20th-Century Biological Research," in *The Science-Industry Nexus : History, Policy, Implications; Nobel Symposium 123rd (2002)*, ed. Karl Grandin, Nina Wormbs, and Sven Widmalm (Stockholm: Science History Publications/USA, 2004).
[35] For examples focused on biological oceanography, see Helen Rozwadowski, *The Sea Knows No Boundaries : A Century of Marine Science under ICES* (London: International Council for the Exploration of

However, lately more attention has been paid to industrial and other non-naval patronage of physical oceanography in various guises. For example, David Van Keuren has discussed the use of deep sea drilling technology developed by the oil industry in oceanographic and geological research.[36] Ronald Doel *et al* also discuss industrial patronage when writing about work creating the Heezen-Tharp physiographic map of the ocean floor in the 1950s. One of the researchers involved in this map simultaneously did contract work for Bell Labs, which provided him with extra data and funding for the mapping work, while he helped them find a low-maintenance route for their new trans-Atlantic phone cable. Doel *et al* emphasise that the links to the industrial Bell Labs, as well as to the military, were very important for this key oceanographic research project. This shows that industrial patronage could be important even in American physical oceanography in the 1950s, often identified as a point when military patronage dominated oceanography.[37] Eric Mills has recently argued that concerns regarding fisheries, agriculture and other industrial issues with links to weather and the sea had a key role in stimulating research in marine sciences generally in Scandinavia in the late nineteenth century. He has also downplayed the role of the Second World War as a trigger in the development of modern-day oceanography.[38] Hamblin has recently written in more depth on the links between oceanographers, radioactive waste and atomic energy establishments of the US, Britain and France in the late 1950s. He turns the traditional patronage question of the influence of patronage on scientists' work on its head, arguing that the scientists' patronage strategies influenced the patrons' policies and status.[39] In addition, while his book *Oceanographers and the Cold War* concentrates on military patronage of oceanography in the 1950s he also mentions many other sources

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the Sea in association with University of Washington Press, 2002); Vera Schwach, "An Eye into the Sea: The Early Development of Fisheries Acoustics in Norway, 1935-1960," in *The Machine in Neptune's Garden: Historical Perspectives on Technology and the Marine Environment*, ed. Helen Rozwadowski and David K. van Keuren (Sagamore Beach, MA: Science History Publications, 2004). See also Susan Schlee, *A History of Oceanography: The Edge of an Unfamiliar World* (London: Robert Hale & Company, 1973).

36 David K. van Keuren, "Breaking New Ground: The Origins of Scientific Ocean Drilling," in *The Machine in Neptune's Garden: Historical Perspectives on Technology and the Marine Environment*, ed. Helen Rozwadowski and David K. van Keuren (Sagamore Beach, MA: Science History Publications, 2004).

37 Ronald E. Doel, Tanya J. Levin, and Mason K. Marker, "Extending Modern Cartography to the Ocean Depths: Military Patronage, Cold War Priorities, and the Heezen-Tharp Mapping Project, 1952-1959," *Journal of Historical Geography* 32, no. 3 (2006).

38 Mills, *The Fluid Envelope of Our Planet*, ch 3 & 9, esp p. 260.

39 Jacob Darwin Hamblin, "Hallowed Lords of the Sea: Scientific Authority and Radioactive Waste in the United States, Britain, and France," *Osiris* 21, no. 1 (2006).

of patronage, arguing that scientists played a strong role in defining their own part in the Cold War.[40]

Despite these recent examples of attention to non-military patronage, many historians of physical oceanography seem to almost assume a dominance of military patronage in the discipline. Doel *et al* are quick to point out that Bell Labs were also secretly laying cables for military use, arguing there were close links to military needs even in the industrial patronage. However, the examples above demonstrate that the patronage patterns of oceanographic science have often been complicated, involving a range of patrons and interests with influences going in both directions between scientists and patrons.[41] Atsushi Akera has argued historians should attend to what he calls fundamental pluralism in terms of institutions, interests, opportunities and priorities, in his case research into computers during the Cold War.[42] While attention to military patronage is important because of its special political implications, the historians' attention to it should not necessarily be all-consuming, as all patronage has political implications which should be attended to.

From the above it is clear that military patronage has been crucial for the work of American oceanographers both before and, especially, after the Second World War. However, there is not enough secondary literature to establish whether this pattern applies to the UK too. In addition, the emphasis by historians on naval patronage may neither take full account of other sources of patronage nor of the scientists' work in forming their own roles and relationships with patrons. This thesis will look closely at the connections between the Admiralty and TI's work but it must also take account of TI's other sources of patronage. By doing so this thesis provides an example of a different kind of patronage structure compared to that outlined by most historians of oceanography, in which industrial, local government and civil state patronage was also important. In addition it traces the connections between different parts of the state and TI, analysing how discussions and disputes between different state actors related to TI's patronage and its research. By attending both to such disputes and to the institution

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[40] Hamblin, *Oceanographers and the Cold War*, conclusion.
[41] See also Angela NH Creager, "The Industrialization of Radioisotopes by the U.S. Atomic Energy Commission," in *The Science-Industry Nexus : History, Policy, Implications; Nobel Symposium 123rd (2002)*, ed. Karl Grandin, Nina Wormbs, and Sven Widmalm (Stockholm: Science History Publications/USA, 2004).
[42] Atsushi Akera, *Calculating a Natural World : Scientists, Engineers and Computers During the Rise of U.S. Cold War Research* (London: The MIT Press, 2007).

building strategies used by TI's researchers I attempt to show the contestation and contingency involved in TI's patronage.

1.3 Practices, especially of forecasting

During the twentieth century forecasting of extreme events, for example of storm surges, by experts has increased. Recently Gary Alan Fine wrote, in relation to weather forecasting, that "[s]ociety depends on prognostication, allowing social systems to prepare for whatever shocks may transpire".[43] The rise in forecasting of extreme events underlying this quote is a topic which has primarily been analysed within the history of meteorology but rarely with attention to the researchers' practices in developing forecasting methods. Before discussing that literature I will here define my usage of the term 'practice'.

As Joseph Rouse makes clear 'practice' is a term with many meanings and many varied theoretical attachments in social science and history.[44] To me paying attention to practices is to pay attention to bodily skills and discipline. In addition, and following Rouse, I see language as part and parcel of the practices, skills and disciplines I am interested in. In particular, I am interested in how TI developed forecasting formulae in a practical manner – for example, how they used pen, paper, tidal gauge records and calculating machines to construct such formulae. Attention to such practices of calculation provides a deeper understanding of how TI's workers made surges predictable than is possible to achieve by attending only to published accounts of their work. Overall, my attention to practice places me within the wider project in history of science that attempts to show scientific and mathematical work as embodied *work*, that makes 'things' in particular ways with particular effects, and that does not effortlessly travel.

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[43] Gary Alan Fine, *Authors of the Storm* (Chicago: The University of Chicago Press, 2007), 245.
[44] Joseph Rouse, "Practice Theory," in *Handbook of the Philosphy of Science*, ed. Dov M Gabbay, Paul Thagard, and John Woods (Elsevier, 2006). Volume 15: Philosophy of Anthropology and Sociology, edited by Stephen Turner and Mark Risjord.

Several historians of meteorology have studied historical attempts at making storms, or extreme flows of air, into more predictable or governable events.[45] A key study is Robert Friedman's *Appropriating the Weather* on Vilhelm Bjerknes. By appropriating the weather, Friedman means that Bjerknes redefined, reclassified and restructured weather to make it more predictable by redefining it as a hydrodynamical problem, reclassifying the atmosphere into new three-dimensional concepts and restructuring weather forecasting to make it useful to flight, farming and fishing, and also to achieve professional support and recognition.[46]

Overall such work on the construction of methods to predict extreme events is more limited in the history of oceanography. However, recently Eric Mills has recast Friedman's story about Bjerknes in oceanographic terms, describing how Bjerknes developed his hydrodynamical theorems. These were applicable to 'baroclinic' conditions when lines of equal pressure and equal density are inclined due to properties of the oceans, like temperature, creating the possibility of currents. He then discusses how these theorems were taken up and further developed by oceanographers such as Bjørn Helland-Hansen and Johan Sandström, making Bjerknes's theorems into simplified equations that could be more easily calculated. In particular, he claims that once Helland-Hansen had reformulated the theory "it was a routine matter to calculate the difference of current speed between surface and the depths across the plane joining the two stations".[47] However, neither Mills nor Friedman go into detail of the practices of calculations used by Bjerknes, Helland-Hansen or Sandström. What were the routines used in these calculations and how were they made into routines by the researchers? What did Bjerknes and the others do in terms of practices to make the weather or ocean currents calculable and predictable?

This question is not answered by history of meteorology, which has instead either focused on the use of forecasting techniques or the development of numerical weather prediction using digital computers. Anthropologists and historians of twentieth century

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[45] Vladimir Jankovic, "The Politics of Sky Battles in Early Hanoverian Britain," *Journal of British Studies* 41(2002); Katherine Anderson, *Predicting the Weather* (Chicago: The University of Chicago Press, 2005); Mark S. Monmonier, *Air Apparent : How Meteorologists Learned to Map, Predict, and Dramatize Weather* (London: University of Chicago Press, 1999); James Rodger Fleming, *Meteorology in America, 1800-1870* (London: Johns Hopkins Press, 1990); Vladimir Jankovic, *Reading the Skies* (Manchester: Manchester University Press, 2000).

[46] Robert Friedman, *Appropriating the Weather* (Ithaca: Cornell University Press, 1989).

[47] Mills, *The Fluid Envelope of Our Planet*, 110.

meteorology have often concentrated on day-to-day forecasting of extreme weather events. For example, Fine has studied how weather forecasting is done through everyday practice in a particular institutional settings, the importance of workplace culture in this and how changes in forecasting technology led to changes in forecasting practice.[48] While his emphasis on practice is useful, he concentrates on the practices involved in everyday prediction, downplaying other aspects of importance for a full understanding of the prediction process, such as the reception of forecasts or the construction of forecasting formulae.[49] Other historians of twentieth century meteorology have focused on the role of digital computers.[50] Kristine Harper is a recent example of this, combining discussions of the development of numerical weather forecasting and digital computers with debates about the role of military and other patronage. Her work indicates the importance of patronage and institutional politics in earth sciences.[51] Paul Edwards similarly combines an account of the history of computing and meteorology. He interestingly pays some attention to practices of calculation including those used before digital computers were introduced.[52]

While researchers like Friedman and Mills concentrate on the work done on the science of forecasting, both they and for example Harper pay less attention to unpublished accounts of how the technical content was constructed than this study does. This thesis adds to the literature of history of meteorology and oceanography by studying another set of practices of calculation in depth – statistical ones as opposed to digital computing – in an under-studied time period. From the history of meteorology I take the attention to the history of forecasting, transferring this interest into the history of oceanography where forecasting has been emphasised much less.

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[48] Fine, *Authors of the Storm*, 111. See also e.g. Steve Rayner, "Domesticating Nature: Commentary on the Anthropological Study of Weather and Climate Discourse," in *Weather, Climate, Culture*, ed. Sarah Strauss and Benjamin S Orlove (Oxford: Berg, 2003); Roger A. Pielke Jr., Daniel Sarewitz, and Radford Byerly Jr., "Decision Making and the Future of Nature: Understanding and Using Predictions," in *Prediction : Science, Decision Making, and the Future of Nature*, ed. Daniel Sarewitz, Roger A. Pielke Jr., and Radford Byerly Jr. (Washington, DC: Island Press, 2000); Trevor A. Harley, "Nice Weather for the Time of Year: The British Obsession with the Weather," in *Weather, Climate, Culture*, ed. Sarah Strauss and Benjamin S Orlove (Oxford: Berg, 2003); Monmonier, *Air Apparent*.

[49] Compare William H. Hooke and Roger A. Pielke Jr., "Short-Term Weather Prediction: An Orchestra in Need of a Conductor," in *Prediction : Science, Decision Making, and the Future of Nature*, ed. Daniel Sarewitz, Roger A. Pielke Jr., and Radford Byerly Jr. (Washington, DC: Island Press, 2000), 75.

[50] Frederick Nebeker, *Calculating the Weather : Meteorology in the 20th Century* (London: Academic Press, 1995).

[51] Harper, *Weather by the Numbers*.

[52] Paul N Edwards, *A Vast Machine : Computer Models, Climate Data, and the Politics of Global Warming* (London: The MIT press, 2010).

1.4 DIFFERENT MATHEMATICAL APPROACHES TO OCEANOGRAPHY AND STORM SURGES

The work TI did to develop surge forecasting formulae primarily used statistics. However, this statistical work linked to Proudman's and Doodson's wider tidal and oceanographic research as well as debates regarding the definition and institutionalisation of oceanography. This section sets out a number of different mathematical approaches to physical oceanography, contrasting the Laplacian tradition to dynamical oceanography, and also discussing work on the history of storm surge science in the Netherlands. Here and later in the thesis I will argue that the formation of contemporary oceanography as a discipline needs to be seen as contested definitional work in which debates between proponents of different approaches played a key role.

Before returning to review the literature I will briefly describe TI's links to the Laplacian tradition. Doodson's wider work on the predictions of tides, within which the work on surges was set, came from the tradition of harmonic analysis of tides. Proudman also worked in this tradition of "classical hydrodynamics and analytical mathematics".[53] Much of his work consisted of extension and application of Laplace's tidal equations to theoretical (as opposed to actual) bodies of water.[54] Both Proudman and Doodson thus belonged to the Laplacian tradition of work on the hydrodynamic theory of long waves. This tradition was strong in Britain (and at Cambridge especially), including researchers such as George Biddell Airy (1801-1892), William Thomson/Lord Kelvin (1824-1907), Horace Lamb (1849-1934) and George Howard Darwin (1845-1912).[55] As Eric Mills has put it recently, those who worked in this tradition were "resolutely theoretical", did not work with actual geophysical fluids (they did not for example take account of temperature differences) and do not seem "to have had the slightest interest in applying a significant body of theoretical fluid mechanics to the dynamics of the ocean".[56] However, tidal work was a key application of the work in this tradition, as described from different historical angles by Paul Hughes, Michael Reidy and David Edgard Cartwright. They do not consider storm surges in any great detail. Cartwright

[53] David Edgar Cartwright and F. Ursell, "Joseph Proudman. 30 December 1888 - 26 June 1975," *Biographical Memoirs of Fellows of the Royal Society* 22(1976).
[54] Cartwright, *Tides*, 164-165.
[55] Ibid., ch 7.
[56] Mills, *The Fluid Envelope of Our Planet*, 80.

concentrate on the technical development of tidal science and predictions.[57] Hughes does the same, but also discusses some of the technologies of calculation used by researchers.[58] Reidy concentrates on developments in tidal science in the first half of the nineteenth century, usefully emphasising links with industrial and military concerns.[59]

An alternative approach to studies of the physical properties of oceans was being developed in Scandinavia around the turn of the century. The development of this 'dynamical oceanography' has been analysed by Mills. He argues that much of physical oceanography went down the route of a particular kind of dynamical mathematical analysis, following the development of Bjerknes's methods as discussed above. He argues that this dynamical analysis came to dominate oceanography. As an example of the difference between the two approaches he mentions that while Lord Kelvin had analysed barotropic flow, Bjerknes dealt with baroclinic flow, during which variation in ocean properties, such as temperature, creates conditions for currents that were not analysed in the Laplacian tradition.[60]

While Mills is at pains to emphasise contingencies in how the dynamical method was adopted and adapted in different circumstances, he none the less portrays Bjerknes's hydrodynamics as the key part of the route that led to modern day oceanography, as dominated by what he calls its foundation text, *The Oceans*. This book, published in 1941, was written by Harald Sverdrup (geophysicist and physical oceanographer), Martin Johnson (zoologist) and Richard Fleming (chemist). Mills argues that post war oceanography was shaped not so much by the war as by Sverdrup's vision of an integrated oceanography, incorporating physical, geological, biological and chemical sciences, and with dynamical oceanography as a crucial organising framework, as expressed in *The Oceans*.[61] Mills's emphasis on dynamic oceanography as the key part of the eventually successful approach to oceanography is particularly clear in how Mills portrays George McEwen's alternative approach, describing it as mathematical physics. He writes that "McEwen's method used a mathematics that was patently too complex

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[57] Cartwright, *Tides*.
[58] Paul Hughes, "A Study in the Development of Primitive and Modern Tide Tables" (PhD, Liverpool John Moores University, 2005).
[59] Michael S. Reidy, *Tides of History: Ocean Science and Her Majesty's Navy* (London: The University of Chicago Press, 2008).
[60] Mills, *The Fluid Envelope of Our Planet*, 102 and note 182.
[61] Ibid., ch 9, esp. p 260-264.

and inappropriate to the quality of his data".[62] He argues that when McEwen entered oceanography, in the US in 1912, the future of the field was not determined and thus McEwen could not have foreseen that his methods would not become mainstream oceanography, but his emphasis is on explaining McEwen's failures. Mills discusses developments in Scandinavia, Canada, France, Germany and the US, but only mentions Britain in passing. According to him this is because little work was done in Britain using dynamic oceanography in the period he focuses on, before the Second World War.[63]

The emphasis on Laplacian tidal equations in TI's theoretical work means that this thesis complements Mills's study, as his work concentrates on one particular strand of physical oceanography which TI's work did not fall into. While Proudman interacted closely with dynamical oceanography, for example writing a textbook on it towards the end of his career, his own research was focused on Laplacian tidal equations. He also defended this approach from accusations by Bjerknes and others that its neglect of baroclinic conditions, leading to vertical acceleration such as temperature-induced currents, could cause serious error, by arguing that the conditions under which this became a problem did not occur in actual tides in the oceans.[64] While thorough analysis of this dispute is outside the scope of this thesis,[65] it shows that Proudman's primary loyalty was towards theoretical work on tides in the Laplacian tradition. Proudman's *Dynamical Oceanography* references some of TI's work on storm surges and discusses some of the causes involved in surge generation (e.g. the effects of atmospheric pressure on the sea and the travel of such effects), but it does not name the phenomenon and does not discuss it in a focused section, demonstrating the distance between TI's work on forecasting storm surges and dynamical oceanography.[66] While TI's researchers were aware of and interacted with dynamical oceanography, their own work was different from this approach.

Some work on the history of storm surge science has been done in the Netherlands, but this has focused on tidal modelling research done for hydraulic coastal engineering

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62 Ibid., 210.

63 Eric L. Mills, personal communication, emails 17 & 18 June 2010

64 Joseph Proudman, "On Laplace's Differential Equations for the Tides," *Proceedings of the Royal Society of London. Series A, Mathematical and Physical Sciences* 179, no. 978 (1942). See also Cartwright, *Tides*, 165-166.

65 It could provide a potential window into further analysis of different types of physical oceanography and how they interacted.

66 Joseph Proudman, *Dynamical Oceanography* (London: Methuen and Co, 1953, reprinted 1963). The original book was probably in print before the 1953 storm surge.

projects. Cornelis Disco and Jan van den Ende covers the period before 1953 in most depth, while Wiebe Bijker provides more cover for the period after 1953.[67] They all identify 1953 as a key event, giving storm surge science impetus and funding. As I will do, they all also emphasise the interconnections between politics at many different scales and the techno-scientific work, as Bijker has expressed in the title of one of his articles: "Dikes and dams, thick with politics".[68] Disco and van den Ende have made the more specific claim that mathematical models of tides and storm surges were used as socio-political management tools by engineers to provide compelling, trusthworthy evidence in support of various engineering schemes for dams. They also discuss a range of different types of models, including electric and scale models, and differentiate between different types of mathematical models, using different equations, often based in hydrodynamic theory but also employing ideas from the harmonic methods of predicting tides.[69]

Unlike TI's work with statistical forecasting formulae, the aim in the Dutch work analysed by historians was to predict the response of the sea water to new dams, dikes and other flood defences. The work described by these authors was linked to the planning of engineering projects instead of real time warning systems, but it used some of the same practices of calculation: human computers, slide rulers and desk calculators, careful checking of results, and lots of time and paper.[70] While Disco and van den Ende's emphasis on this is useful, I will discuss TI's use of such practices in more depth. The practices were used for different purposes: TI used these practices for statistical calculations to forecast surges, whereas the work described by Disco, van den Ende and to some extent Bijker involved different kinds of mathematics based in fluid dynamics.

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[67] Jan van den Ende, *The Turn of the Tide: Computerization in Dutch Society, 1900-1965* (Delft: Delft University Press, 1994); Wiebe E. Bijker, "The Oosterschelde Storm Surge Barrier," *Technology and Culture* 43(2002); Wiebe E. Bijker, "American and Dutch Coastal Engineering: Differences in Risk Conception and Differences in Technological Culture," *Social Studies of Science* 37, no. 1 (2002); Wiebe E. Bijker, "Dikes and Dams, Thick with Politics," *ISIS* 98, no. 1 (2007); Cornelis Disco, "Delta Blues," *Technology and Culture* 47, no. 2 (2006); Cornelis Disco and Jan van den Ende, "'Strong, Invincible Arguments'? Tidal Models as Management Instruments in Twentieth-Century Dutch Coastal Engineering," *Technology and Culture* 44, no. 3 (2003).
[68] Bijker, "Dikes and Dams, Thick with Politics."
[69] Disco and Ende, "'Strong, Invincible Arguments'? Tidal Models as Management Instruments in Twentieth-Century Dutch Coastal Engineering."
[70] Ibid.

Within oceanography generally and tidal and storm surge science in particular there were thus a number of different mathematical approaches available. This thesis discusses how TI went down one particular route. While concentrating on TI's approach, it also pays some attention to different approaches to oceanography, especially regarding the institutional landscape of UK oceanography after the Second World War when debates about different types of oceanography impacted on TI. This problematises Mills's argument regarding the origin of contemporary oceanography, at least in Britain. In the case of Britain I believe this argument needs to be further contextualised in contested debates about the nature of oceanographic science at different times.

1.5.1 Practices of mathematics

While the perspective of history of oceanography is highly relevant to understand TI's work on predicting tides and surges, both Doodson and Proudman were trained as mathematicians and the work they did was mathematical, often statistical in relation to storm surges, and involved a large numbers of calculations. The overview of the existing literature above has identified a fascination with naval patronage within the history of oceanography, and a fascination with forecasting within the history of meteorology. While recent work by Mills has looked at the use of a particular type of mathematics within oceanography and Reidy pays some attention to practices of calculation regarding early nineteenth century tidal predictions, the literature review has also found a lack of attention to the construction of formulae used in forecasting and to the practices of theoretical work in both meteorology and oceanography. This lack, and Doodson's and Proudman's disciplinary affiliation to mathematics, brings us to this section on the history of mathematics and practices of calculation. How exactly did TI make surges calculable and predictable? The short answer is that they used multiple regression statistics to construct correlations that were then used to construct a forecasting formulae, but how did they do this practically? What was involved in this work?

This section will first look at how historians have argued that mathematics can be studied through practices, but have rarely done so, before turning to examples of those who have in fact done this. In this area I concentrate on the work of Andrew Warwick, Bruno Latour, Michael Lynch and Herbert Kalthoff, developing concepts and critiques I will then use to analyse TI's attempts to make surges predictable in the rest of the thesis.

1.5.2 Mathematics as amenable to sociological and historical study

Various historians of science have argued that mathematics is as amenable to sociological analysis as other scientific work. They have also argued that the universalisation of mathematics takes just as much work as the universalisation of experimental findings, and relies just as much on tacit knowledge and on networks of correspondence and data as experimental science.[71] For example, Warwick has argued that there are a number of similarities between the theoretical and laboratory sciences, such as that the travel of both theoretical and experimental knowledge is difficult and dependent on tacit skills.[72] David Bloor has described mathematics as a "body of skills, beliefs and thought processes into which individuals must be initiated".[73]

Despite this, historians of science have paid considerably less attention to the practices involved in mathematical work than they have to the practices involved in laboratory science. Historical work on statistics could be an obvious comparison for TI's use of statistics in forecasting storm surges. However, work on the history of statistics has concentrated on the development of statistics to around the First World War and its use in social science for the purposes of the state. When there has been attention to the use of twentieth century statistics in natural sciences, this has concentrated on biology

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[71] Simon Schaffer, "Newton on the Beach: The Information Order of Principia Mathematica," *History of Science* 47(2009); David Bloor, *Knowledge and Social Imagery*, 2nd ed. (London: The University of Chicago Press, 1991); Andrew Warwick, "The Laboratory of Theory or What's Exact About the Exact Sciences?," in *The Values of Precision*, ed. M. Norton Wise (Chichester: Princeton University Press, 1997); Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (London: University of Chicago Press, 2003); Andrew Pickering and Adam Stephanides, "Constructing Quaternions: On the Analysis of Conceptual Practice," in *Science as Practice and Culture* ed. Andrew Pickering (London: University of Chicago Press, 1992); Eric Livingston, *The Ethnomethodological Foundations of Mathematics* (London: Routledge and Kegan Paul, 1986); Donald MacKenzie, "Slaying the Kraken: The Sociohistory of a Mathematical Proof," *Social Studies of Science* 29, no. 1 (1999).

[72] Warwick, *Masters of Theory*, 10-18.

[73] Bloor, *Knowledge and Social Imagery*, 87. He proceeded to attempt to portray mathematics in such a way, discussing its basis in empirical practices such as moving pebbles, showing that alternative mathematics, which appears illogical to us, are possible and have been in existence historically and by discussing the role of 'negotiation' and moral obligations in logical necessity. Bloor, *Knowledge and Social Imagery*, ch 5-7.

(especially inferential statistics) and physics (e.g. probability and quantum physics), and does not appear to have concentrated on practices. I will refer to some of this work when discussing aspects of TI's work, but the history of statistics has not emphasised twentieth century applications and practices of calculation in sciences like oceanography or geophysics so comparison case studies are limited. What this literature has done is to make very clear that statistics is amenable to sociological and historical studies, showing many links between the particular social and scientific concerns of the practitioners of the work.[74]

More generally, work on the practices used in mathematical work in specific physical sciences in the twentieth century is somewhat limited. While there has been some work done on the history of computing before digital computers, work analysing practices of calculation in relation to specific applications in more depth is still limited. However, the literature on computing often usefully describes machines and methods of work.[75] Some attention has been paid to the role of practices of teaching mathematics and theoretical physics and to the influence of this on research.[76] When attention has focused on professional mathematical research, this has often concentrated on the process of constructing proofs in 'pure' mathematics.[77] However, I am interested not in

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[74] Stephen M. Stigler, *The History of Statistics: The Measurement of Uncertainty before 1900* (London: The Belknap Press of Harvard University Press, 1986); Donald MacKenzie, *Statistics in Britain 1865-1930: The Social Construction of Scientific Knowledge* (Edinburgh: Edinburgh University Press, 1981); Theodore M. Porter, *The Rise of Statistical Thinking, 1820-1900* (Chichester: Princeton University Press, 1986); Alain Desrosières, *The Politics of Large Numbers : A History of Statistical Reasoning*, trans. Camille Nash (London: Harvard University Press, 1998); Gerd Gigerenzer *et al.*, *The Empire of Chance : How Probability Changed Science and Everyday Life* (Cambridge: Cambridge University Press, 1989); Hacking, *The Taming of Chance*; M. Eileen Magnello, "Karl Pearson and the Establishment of Mathematical Statistics," *International Statistical Review* 77, no. 1 (2009); David Howie, *Interpreting Probability: Controversies in the Early Twentieth Century* (Cambridge: Cambridge University Press, 2002), section 6.4 & 6.5; M. Eileen Magnello, "The Non-Correlation of Biometrics and Eugenics: Rival Forms of Laboratory Work in Karl Pearson's Career at University College London, Parts 1 and 2," *History of science* 37(1999).

[75] Martin Campbell-Kelly *et al.*, *The History of Mathematical Tables from Sumer to Spreadsheets* (Oxford: Oxford University Press, 2003); Martin Campbell-Kelly and William Aspray, *Computer: A History of the Information Machine* (Oxford: Westview Press, 2004); Mary Croarken, *Early Scientific Computing in Britain* (Oxford: Oxford University Press, 1990); Warwick, "The Laboratory of Theory."; Mary Croarken, "Astronomical Labourers: Maskelyne's Assistants at the Royal Observatory, Greenwich, 1765-1811," *Notes and Records of the Royal Society of London* 57, no. 3 (2003); Jon Agar, *The Government Machine: A Revolutionary History of the Computer* (London: MIT Press, 2003).

[76] Kathryn M. Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics* (London: Cornell University Press, 1991); Warwick, *Masters of Theory*; Moritz Epple, "Knot Invariants in Vienna and Princeton During the 1920s: Epistemic Configurations of Mathematical Research," *Science in Context* 17, no. 1-2 (2004).

[77] Christian Greiffenhagen, "Video Analysis of Mathematical Practice? Different Attempts to "Open up" Mathematics for Sociological Investigation," *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research* (2008); Livingston, *The Ethnomethodological Foundations of Mathematics*; MacKenzie, "Slaying the Kraken: The Sociohistory of a Mathematical Proof."

the practices of 'pure' mathematics but in the practices involved in using statistics while trying to make a particular phenomenon predictable. Eric Livingston points out that the mathematical practice of theoretical physicists, which is closer to what I am interested in, is not necessarily the same as that of professional mathematicians, even if they are working on proofs of the 'same' phenomena – his example is the divergence theorem – but that both can be studied through attention to practice.[78] That is also the key message I take from this body of literature: mathematical work, even pure mathematics, can be studied as a historically contingent material practice.

1.5.3 Work on practices of mathematics

I now turn to literature that has discussed the use of mathematical practices in scientific work, such as the use of statistics and computational techniques in particular disciplines, in more detail. One example is Edwards's recent book on computing and meteorology. He describes the use of statistics in climatology before digital computers and discusses practices of calculation, such as the use of analogue computing, other computing aids and punch cards, in meteorology in the first half of the twentieth century.[79] He pays particular attention to the limits introduced by computational practices, introducing two concepts to analyse this: data and computational friction. He defines data friction as "the costs in time, energy, and attention required simply to collect, check, store, move, receive, and access data," whereas computational friction is the "expenditures of energy and limited resources in the processing of numbers [or, differently expressed,] the struggle involved in transforming data into information and knowledge".[80] Edwards's attention to the impact and use of computing technology in a specific scientific discipline, meteorology, is interesting and useful. However, he relies on secondary sources which limit his attention to practices of calculation as they are written up. In addition, his focus is on the limits set by computing, not on the particular practices actually used in specific case studies. Another example is Mary Croarken's work on LJ Comrie at the Nautical Almanac Office, describing how he introduced computing there, but Croarken's work concentrates on computing per se instead of its use in other

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[78] Livingston, *The Ethnomethodological Foundations of Mathematics*, Appendix.
[79] Edwards, *A Vast Machine*, ch 4-5.
[80] Ibid., 83-84.

aspects of science.[81] Jon Agar's work on the links between computing and government ties together practices of calculation with an emphasis on the rise of the state and statistics, but in this work he pays less attention to scientific/mathematical research.[82]

1.5.4 WARWICK: THE MATERIAL CULTURE OF MATHEMATICS

An important exception to the lack of attention to the practices of mathematics is Warwick's work. He has argued for the study of theoretical works as skilled and practice-laden work, into which practitioners are encultured and trained so that many techniques and much "theoretical technology" becomes tacit and taken for granted: "the products of theoretical work can be viewed as the cultural artifacts of the theoretical practices learned and articulated by theoreticians".[83] His primary case study of how these theoretical practices were learnt and used is the development of mathematical physics at Cambridge University, which he approaches from the view of how students were trained, examined and worked between the eighteenth century and the early twentieth century. He emphasises the development of a particular material culture of mathematics at Cambridge, for which written exams, coaching as well as indvidual practice, and pen and paper were important. Through their training in these material practices mathematical physicists from Cambridge developed particular skills and sensibilities. Warwick also emphasises how mathematical physics travelled, discussing how the Cambridge teaching methods as well as theories travelled (or not as the case may be), how theories from elsewhere, such as Albert Einstein's work on relativity, were received at Cambridge and how the skills and theories taught there interacted with such 'received' theories in research work.[84] To him, the cultural history of mathematical physics is a necessary explanation for how the "technical history" of this discipline was made possible.[85]

Warwick has also linked the history of technologies of calculation to physics, arguing that precision measurements and precision calculations were closely linked. Both

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[81] Warwick, "The Laboratory of Theory."; Croarken, *Early Scientific Computing in Britain*.
[82] Agar, *The Government Machine*.
[83] Andrew Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905-1911 Part I: The Uses of Theory," *Studies In History and Philosophy of Science Part A* 23, no. 4 (1992): 633-634.
[84] Warwick, *Masters of Theory*.
[85] Ibid., xi.

increased in Britain at the same time from the mid to late nineteenth century, and both were dependent on sets of new practices and technologies as well as on a specific industrial and governmental setting. New technologies of calculation, such as mathematical tables, calculating machines and new management practices regarding human computers, enabled the application of the new nineteenth century mathematical physics and analysis not only to the needs of bureaucracy, business and military but also to science and the creation of empirical laws – for example the kind of forecasting formulae TI constructed. Practices and technologies of calculation connected the style of mathematics taught for the Cambridge Mathematical Tripos, using pure algebraic analysis, to the needs of physics. He argues that new technologies of calculation led to new practices, which together enabled workers to calculate more and for longer with fewer mistakes (i.e. with higher accuracy and precision).[86]

I take my attention to the material culture of mathematics and technologies of calculation from Warwick, but look at mathematical practice in a very different setting from Cambridge: that of provincial Merseyside. While Proudman and Doodson had links to the world of Cambridge mathematics, for example through training, they operated in a different world in which industrial and naval patronage was crucial. I will be combining attention to their practices of calculation with their attempts to enlist patrons. I also concentrate on the construction of statistical formulae in relation to a specific physical phenomenon of direct interest to TI's patrons, not the development of theoretical mathematical physics – I concentrate on TI's work on statistical forecasting formulae and harmonic tidal predictions, not their work on the hydrodynamic theory of surges or tides. In particular, I analyse the use of the theoretical technology of statistics as well as technologies of calculation in TI's work on this.

1.5.5 LATOUR: CHAINS OF DOCUMENTS THAT CONNECT

The practices of calculation TI used to construct forecasting formulae for surges produced reams of documents connected in chains; pieces of paper with numbers, graphs and calculations on them which linked to other such documents. To analyse how TI used new practices of calculations to produce and connect such documents I use

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[86] Warwick, "The Laboratory of Theory."

Bruno Latour's work on chains of inscriptions as a framework. This is partly as he has emphasised such documents, which he calls inscriptions or forms, and partly as he is one of the researchers who have encouraged studies of mathematical work from the same standpoint as other scientific work, emphasising that it is not abstract or transcendental but accessible to analysis through the study of documents and practices.[87] I will focus on one particular aspect of Latour's work, extending his idea of chains of inscriptions to pay more attention to contingencies and less attention to the power-seeking behaviour of scientists.

According to Latour one important aspect of scientific work is to create and combine inscriptions to find patterns. These inscriptions are transformed from one element/form to another in a never-ending chain of transformations producing "chains of elements", with each element being a 're-presentation' of another. Each transformation leads both to amplifications and reductions, for example in terms of materiality or compatibility.[88] In TI's case they received inscriptions from tidal gauges which they, through a number of transformations, turned into other inscriptions or documents 're-presenting' the surge as numbers extracted out of the tidal gauge record. Such documents, or n-th order forms, could be more easily stored (due to being compact) and also compared with other such surges.

Latour argues that the creation of n-th order forms gives rise to unexpected 'supplements' for the scientists, i.e. results that increases their ability to intervene or convince.[89] Such forms/inscriptions mobilise and stabilise the world, making aspects of it moveable and intelligible to the scientist, and by being such 'immutable and combinable mobiles' they allow scientists to "speak more authoritatively and with more assurance" about the world.[90] For Latour scientists become master of the phenomena they are studying through work creating chains of documents, by studying such documents and finding patterns in the traces on them.[91] Such mastery is particularly pronounced in 'centres of calculation', such as TI, that bring together inscriptions and do additional work with them, creating further inscriptions, especially using calculations

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[87] Latour, *Science in Action*, 237-247; Bruno Latour, "A Relativistic Account of Einstein's Relativity," *Social Studies of Science* 18, no. 1 (1988): 25.
[88] Latour, *Pandora's Hope*, 70.
[89] Latour, *Science in Action*, 246.
[90] Latour, *Pandora's Hope*, 99-102, quote from 101.
[91] Ibid., 53.

and mathematical formalism.[92] TI used statistical calculations on their inscriptions of surges to produce further documents, attempting to create forecasting formulae for surges to make this phenomenon predictable. Latour sees mathematical formalisms or theories as just another step in the creation of more and more mobile yet immutable inscriptions, arguing that they allow scientists to "assemble many allies in one place".[93] For example, using Einstein's relativity theory scientists could do more than without it: "more frames of reference with less privilege can be accessed, reduced, accumulated and combined, observers can be delegated to a few more places in the infinitely large (the cosmos) and the infinitely small (electrons), and the readings they send will be understandable."[94]

Latour is emphatic that the increase in power scientists gets from equations come from them increasing connections, not from some miracle of immaterial thinking. For example, he argues that the Lorentz transformation, as used by Einstein, increased connections by "defin[ing] the paperwork necessary to move documents from one frame to the other and still maintain superimposition of traces at the end."[95] He emphasises the paper*work* involved in constructing Einstein's formulae: "the word relativity refers to this lowly work of building and relating frames to another in such a way that some kind of stable form can be maintained" through various transformations.[96] Latour's emphasis on such logistical work in the construction of formulae, such as Reynolds's formula,[97] Mendeleev's table,[98] Galileo's law of falling bodies[99] or a diagram produced after fieldwork in Brazil,[100] is productive for me. Together with the analytical tools I take from Warwick and others it provides an approach to analyse TI's mathematical work on surge forecasting as practice. However, Latour's main focus when it comes to theory, or formalism as he often calls it, is on the links between language and nature, with him concentrating on denying the "canonical view" that the two are separate and need mysterious bridges to connect up. Despite his

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92 Latour, *Science in Action*, 232-247.
93 Bruno Latour, "Drawing Things Together," in *Representation in Scientific Practice*, ed. Michael Lynch and Steve Woolgar (London: The MIT Press, 1990), 50.
94 Latour, "A Relativistic Account of Einstein's Relativity," 22.
95 Ibid.: 18.
96 Ibid.: 20.
97 Latour, *Science in Action*, 237-238, 243.
98 Ibid., 235-236.
99 Latour, "Drawing Things Together," 47-52.
100 Latour, *Pandora's Hope*, ch 2, esp 46-47 and 54-46.

emphasis on paperwork, he is usually less concerned with how chains of inscriptions or formulae are constructed in practice by scientists, instead focusing on debating various philosophical and sociological issues.[101]

My focus is instead on how TI constructed their particular chains of documents and forecasting formulae. Why does a scientist create one document and not another? Why use one formula or mathematical technique over another? Latour does not much discuss the choices or work involved in creating these particular relationships. For example, his narrative of pedological research in Brazil analyses the scientists' "unbroken series of well-nested elements", e.g. of earth, pedocomparator and diagrams, but spends less time analysing how the scientists had chosen to construct each element or the particular links between them.[102] How did these elements come to take the form of a wellnested unbroken series? While I agree that scientists construct chains of documents, I will add to this picture by further emphasising the work, choices and contingencies involved in constructing such chains of documents.

My emphasis on the choices made when constructing chains of documents is closely linked to how I de-emphasise the power-seeking behaviour of TI's scientists. As is clear from the description of his view of the construction by scientists of chains of documents above, Latour explicitly argues that such work gives power to scientists. His concentration on the power of scientists has been criticised as assuming that scientists seek power and control, and gain this through their work, while in fact there are often strict limitations to the extent of their power.[103] In particular Latour claims that mathematicians are in a strategic position to increase their powers as he sees mathematics and formulae as concentrating connections. To him, an increase of formalism will lead to an increase in connections in the network the formalism/equation is at the heart of, and thus to an increased ability for scientists to intervene, i.e. more equations equals more power. As he puts it in reference to theories: "Inscriptions allow *conscription!*"[104]

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101 Ibid., 71-74; Latour, "A Relativistic Account of Einstein's Relativity," 20-36.
102 Latour, *Pandora's Hope*, 56.
103 Graeme Gooday, *The Morals of Measurement: Accuracy, Irony, and Trust in Late Victorian Electrical Practice* (Cambridge: Cambridge University Press, 2004), ch 1; Olga Amsterdamska, "Review: Surely You Are Joking, Monsieur Latour!," *Science, Technology, & Human Values* 15, no. 4 (1990).
104 Emphasis in original. Latour, "Drawing Things Together," 50.

While TI's researchers of course sought a certain amount of dominance in order to ensure their institute's continued survival and patronage, I will downplay the extent to which TI's researchers sought power as such. The choices they made when constructing chains of documents or choosing formulae cannot necessarily be explained by reference to them seeking power or domination – it was as much about immediate reactions as about long-term planning. Latour instead writes: "All innovations in picture making, equations, communications, archives, documentation, instrumentation, argumentation, will be selected for or against depending on how they simultaneously affect either inscription or mobilization".[105] I cannot see how a scientist can necessarily know *in advance* how a new 'innovation' will affect inscription (i.e. the process of creating chains of documents) or mobilisation (i.e. the process of enlisting allies), and thus how they can select for or against it on this ground – it has to be tried out first. If scientists cannot know in advance if their experimental work will produce one of Latour's 'supplements,' they cannot select the 'innovation' that will produce one, as they do not necessarily know which one will. Instead the shape of the chain of documents and the choices made in its construction must depend on other factors.

Latour's work on chains of inscription provides a framework for my attention to documents and formulae. I will add to this framework by attending to the contingencies, choices and work involved in TI's construction of chains of documents. This is inspired not only by Warwick's work on the practices of mathematics but also by work done by Kalthoff and Lynch on practices of calculation.

1.5.6 Practices of calculation constituting calculable entities

How are events, such as storm surges, made into entities onto which mathematical practices, such as statistics, can be used to, say, predict future surges? This has been discussed from different angles but with similar results by Michael Lynch and Herbert Kalthoff, both arguing that things are constituted as calculable entities through practices of calculation, or mathematisation in Lynch's terminology.

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105 Ibid., 52.

Lynch argues that different renderings of a phenomenon – in his case visual representations of biological specimens – select and, crucially, add visual features,[106] and that such representations "are essential to how scientific objects and orderly relationships are revealed and made analyzable".[107] In particular, practices of selection and visualisation, such as "clearly marking the outlines to distinguish one case from another",[108] produce coded and aggregated objects onto which "[a]rithmetic and graphic representational operations can then be performed on the basis of the enhanced identities and differences".[109] Lynch's basic argument is that scientists constitute and frame phenomena through visual and mathematical practices that make them calculable. I agree with this and will give a detailed example of such a process operating in TI's mathematical work in chapter five. Lynch however argues that such practices operate "in the direction of generic pedagogy and abstract theorizing", i.e. he argues that scientists are able to in advance select how to constitute objects with a goal of specific desired results in mind. The same criticism as against Latour's selection of innovations to produce supplements applies: how can the scientist know in advance what practice will lead to a specific result in terms of pedagogy or theorising?[110] I will instead argue that while TI's practices of calculations made surges into calculable objects, they did so in particular contingent ways that in unpredictable ways limited future calculations.

Kalthoff has paid more attention to how practices of calculation produce chains of documents that bind as well as connect. Whereas he comes from the field of social studies of finance and concentrates on how banks make risk calculable, he specifically discusses how this was done through practices of calculation, using particular computer programmes containing particular formulae producing particular written documents from particular data put together in particular ways.[111] He argues that such practices, and in particular the formulae used, bring into being entities that are structured in

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106 Michael Lynch, "The Externalized Retina: Selection and Mathematization in the Visual Documentation of Objects in the Life Sciences," in *Representation in Scientific Practice*, ed. Michael Lynch and Steve Woolgar (London: The MIT Press, 1990), 181.
107 Ibid., 154.
108 Ibid., 181.
109 Ibid., 182.
110 Ibid., 181.
111 For other works from the social studies of finance, see e.g. Donald MacKenzie, *Material Markets : How Economic Agents Are Constructed* (Oxford: Oxford University Press, 2009); Donald MacKenzie, Fabian Muniesa, and Lucia Siu, *Do Economists Make Markets? : On the Performativity of Economics* (Woodstock: Princeton University Press, 2007).

specific ways, for example by the rules of mathematics.[112] Following Heidegger, Kalthoff argues that calculation technology is part of a modern system of ordering of resources: "modern technology transforms the objects it [calculation] reveals into uniform, materialized things that can be measured and compared through computing, balancing and calculating".[113] However, the use of such technologies of calculation "means being set in a 'chain' … of calculation: one level of calculation sets or place another level of calculation … one interpretation sets another interpretation".[114] He argues that in modern society these set chains limit our understanding of phenomena, making technological understanding seem the only legitimate form.

Kalthoff's and Lynch's approaches provide a way of analysing how TI made surges into calculable scientific objects.[115] I will describe in detail how TI's practices of calculation constituted theoretical entities they called surges. These entities were linked to changes in sea level through chains of documents, in Latourian fashion. Surges were made calculable entities, and indeed constructed, through practices of calculation. Kalthoff differentiates between calculating something and calculating *with* that something and Lynch makes a similar differentiation between constructing a picture with selected attributes and mathematically analysing those 'residues': "Constructing a 'good' picture of a laboratory specimen's residues is [a] prerequisite for mathematically analyzing those residues".[116] I will explore this differentiation further in chapter five by discussing how the process of making surges into calculable entities was not sufficient to also make them predictable. In addition I explore the contingent process of establishing particular practices of calculation, which in turn contingently produce and structure the entities the practices constitute. Studying how chains of documents come into being provides a way to understand the historical contingencies involved in scientists' use of such chains.

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112 He argues that the bank's practices of calculation make both risk and companies anew through writing and calculation. Entities, such as the credit proposal the bank prepares that suggests the bank should lend to a particular company or the company as it is seen by the bank, are constituted by writing and calculation, producing documents: "Theoretical entities are … brought into existence and made calculable by operative writing" (p.82). He also emphasises that writing and calculations are performative: "the performative force of formulae consists in bringing (economic) reality into being by connecting different entities" (p.83). Herbert Kalthoff, "Practices of Calculation: Economic Representations and Risk Management," *Theory, Culture & Society* 22, no. 2 (2005).
113 Ibid.: 73.
114 Ibid.
115 See also Theodore M. Porter, "Making Things Quantitative," *Science in Context* 7, no. 03 (1994).
116 Lynch, "The Externalized Retina," 182.

1.5.7 *ACCURACY, TRUST AND PRACTICES OF CALCULATION*

A key theme in this thesis is that TI attempted to increase the 'accuracy' of their calculations or predictions of tides and storm surges. By 'accuracy' they meant the closeness between observed and predicted sea level. How this closeness was judged will be further discussed in various places of the thesis. Much work has been done on accuracy in history of science, though it has concentrated on precision measurements.[117] I discussed one exception to this emphasis on precision measurements above in the context of Warwick's work on the links between technologies of calculation and accuracy. His work in this area shows that arguments about accuracy developed in the wider literature on precision measurements can also be adapted for other areas of science, such as computational and theoretical work.

The wider work on precision measurements has shown that demands for increased accuracy of scientific measurements or calculations often have been linked to the needs of state or industry.[118] For example, early nineteenth century German states wanted more precisely defined standards of weight and measure, which led to physicists working on precision measurements.[119] Industry, both precision instrument engineering and the telegraph industry, was heavily involved in debates on precision measurements of resistance standards.[120] I will argue that this was also the case at TI, discussing a number of cases where TI's work on improving the accuracy of tidal and surge predictions was linked to industrial, military or state demands. Graeme Gooday argues that trust was another important issue in relation to late nineteenth century electrical measurements. Trust in these measurements was a complex web of trust in people and their morality, materials, reading practices, machines, mathematical theories and the level of care taken.[121] Rhetoric about precision measurements could also be important in

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[117] E.g. Gooday, *The Morals of Measurement*; M. Norton Wise, "Introduction," in *The Values of Precision*, ed. M. Norton Wise (Chichester: Princeton University Press, 1997); Bruce J. Hunt, "The Ohm Is Where the Art Is: British Telegraph Engineers and the Development of Electrical Standards," *Osiris* 9(1994); Simon Schaffer, "Late Victorian Metrology and Its Instrumentation: A Manufactury of Ohms," in *The Science Studies Reader*, ed. Mario Biagioli (London: Routledge, 1999).

[118] Wise, "Introduction."; Warwick, "The Laboratory of Theory."; Simon Schaffer, "Accurate Measurement Is an English Science," in *The Values of Precision*, ed. M. Norton Wise (Chichester: Princeton University Press, 1997); Hunt, "The Ohm Is Where the Art Is."

[119] Kathryn M. Olesko, "The Meaning of Precision: The Exact Sensibility in Early Nineteenth-Century Germany," in *The Values of Precision*, ed. M. Norton Wise (Chichester: Princeton University Press, 1997).

[120] Schaffer, "Accurate Measurement Is an English Science."; Gooday, *The Morals of Measurement*; Hunt, "The Ohm Is Where the Art Is."

[121] Gooday, *The Morals of Measurement*, 263-267.

institution building. As Gooday has argued, physicists from the 1860s to 1880s appealed for resources for teaching laboratories by rhetorically using recent dramatic developments in research on precision measurements to justify the funding. Such appeals were made within a setting where there was both supply of laboratory training, through the expansion of physics in terms of personnel and resources, and demand for such training due to debates on scientific education and the expansion of the telegraph industry.[122] I discuss how TI used similar arguments about accuracy to argue for funding in the 1920s, linking accuracy to trustworthiness as well as to funding, but in the context of precision calculations instead of precision measurements.

1.5.8 Practices of calculation and storm surges

In order to analyse how TI attempted to make storm surges more predictable I use a range of analytical tools and approaches. From Warwick I take my attention to the material culture and practices of mathematics – how the seemingly universal language and results of mathematics were constructed in a particular place and in particular ways, using particular technologies. From Latour I take my attention to networks of inscriptions, centres of calculation and enrolment of patrons – chains that connect. From Lynch and especially Kalthoff I take the idea that particular entities are made calculable through practices in a particular way on particular documents – chains that bind. From the literature on precision measurement I take my attention to the rhetoric and practices involved in scientific work on accuracy and how this linked both to demands from state and industry and to arguments for patronage by scientists – TI's work was linked to its patronage.

This thesis thus provides a case study of how a group of mathematicians attempted to make a particular phenomenon, storm surges, (more) predictable through the use of particular practices of calculations, creating documents and formulae. Such work on the accuracy of tidal predictions was linked to demands from industry and state actors. TI's work on the predictions of tides and surges was intensely dependent upon specialised practices of calculation, equipment (e.g. tide predictors) and networks of

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[122] Graeme Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain," *The British Journal for the History of Science* 23, no. 1 (1990); Gooday, *The Morals of Measurement*, 63, note 77.

correspondence, including the provision of data from many sources. These practices, technologies and data were used to construct documents and formulae in specific ways, using statistical practices of calculation to constitute storm surges as predictable events. Only by concentrating on the details of TI's mathematical practice is it possible to understand what TI did to make surges predictable.

1.7 Chapter overview

Chapter two describes how TI, the institute where the storm surge work discussed in the rest of the thesis was done, was established. This is set into the context of the First World War and debates regarding the effect of this war on science and the involvement of the state in scientific research. I also discuss aspects of tidal research before TI's establishment, focusing on changes in the level of accuracy in predictions deemed necessary by different actors. TI originated during the First World War as part of a British Association for the Advancement of Science (BAAS) proposal to set up a Geodetic Institute to conduct academic research and give advice to the government. However, only the tidal part of it was developed while the rest of the institute was dropped, when Joseph Proudman via existing university-based networks managed to convince two Liverpool shipping brothers, Charles and Alfred Booth, to provide funding for five years.

The ability of Proudman to convince businessmen of the need for TI was grounded in the increased size of ships and in the recent war during which the Navy had needed tidal predictions for mining operations and to ship troops to the continent. Together these two factors, war and larger ships, had changed the level of accuracy state and industrial actors wanted in tidal predictions, but this was not reflected in the initial BAAS proposal for which Proudman emphasised research on tidal theory. During the process of establishing TI, its research programme, as put forward by Proudman and Doodson, changed towards emphasising predictions over tidal theory. This change depended on audience, the work already done and on information received, crucially from the Hydrographic Department of the Admiralty (Hydro). I argue that shipping men provided funding as they wanted more 'accurate' tidal predictions that were closer to observations, and that because of this patronage increasing such accuracy became one of key aims of the Institute. Within this research programme meteorological effects was one aspect that was said to need work.

The third chapter then looks at how TI tried to implement this research programme during its first five years. The focus of the chapter is TI's work to improve the accuracy of tidal predictions in comparison with observations, and to predict meteorological effects. In both cases I argue that they introduced new practices of calculation, such as management routines and ideas from statistics. The chapter therefore also discusses TI's calculating machines and the human computers who used these, as well as the person who introduced them to TI, Arthur Doodson, and his work experiences during the First World War. These experiences shaped his practices of calculation.

The chapter argues that TI's research workers linked accuracy and trustworthy predictions to practices of calculation. It provides a case study of the links outlined by Warwick between demands from different actors, developments of technologies of calculation and mathematical work, both theoretical and computational, aimed at improving the accuracy of the numbers generated by a specific theory.[123] Doodson used new technologies and practices of calculation to redevelop the harmonic theory of tidal prediction as well as analysing tidal data to find further constituents by analysing residuals. He argued that because of these new practices of calculation he was able to include more of the constituents of tides in predictions, and that therefore these predictions would be of higher accuracy, i.e. closer to observations of tides. His arguments were however not only linked to developments in demands for accuracy and practices of calculation, but also closely linked to TI's institution building activities. I argue that the way Doodson and Proudman talked about improving accuracy was steeped in the need to justify both their funding and the work they did – talk about the need for accurate tidal predictions was a key part of their institution building strategy. As part of their work on the accuracy of tidal predictions, TI's researchers defined surges as residuals, a definition which I argue was also linked to Doodson's earlier work.

Chapter four analyses how TI used the results of the work discussed in chapter three both to do predictions and to argue for patronage in the period up to 1929. The chapter also discusses how the Navy increased its patronage of TI, first by proxy via

[123] Warwick, "The Laboratory of Theory."; Warwick, *Masters of Theory*.

Department of Scientific and Industrial Research (DSIR) grants between 1921 and 1923. In 1923 the funding from the Booth brothers and from DSIR ended, at which point TI used the results of their earlier work to argue for further patronage from the shipping industry and the Navy. TI's work on tidal predictions had been influenced by Hydro but not directly paid for by them until 1923 when an agreement was signed after which TI provided Hydro with an increasing amount of tidal predictions and analysis. This provided a measure of long-term funding and security for TI. The chapter looks at how this agreement followed not only TI's research on tidal predictions but also network building with Hydro and internal concerns within Hydro regarding the quality of predictions from their other supplier.

With the agreement in 1923 the naval 'leg' in TI's triangular patronage structure had appeared on a permanent basis. The other two legs, the university and the shipping industry, repeatedly renegotiated their relationship during the first decade of TI's existence, but at the end of it they had agreed a patronage and governance structure that would remain in place until the end of the period covered by the thesis. Under this structure the Mersey Docks and Harbour Board (MDHB) paid a key role as patron of TI, housing it and providing funds, while the University had an important but smaller role, providing little monetary assistance but access to academic networks. One important industrial patron in this period was the Liverpool Steamship Owners Association (LSOA). They also became involved in 1923, following an appeal to them via personal contacts. A key part of this appeal from TI was a promise to provide forecasts of meteorological effects for Liverpool port. The main argument of this chapter is that TI's researchers were able to use their earlier work on predictions and storm surges to gain patronage and generate income by producing tidal predictions and forecasts of meteorological effects. They also built networks with their patrons, which by the mid 1920s were sufficiently strong that even when the meteorological forecasts were twice deemed unsuccessful this had no major effect on TI's patronage.

While the first three empirical chapters covered TI and its work fairly widely, chapter five focuses on storm surge work. It concentrates on one flooding event in 1928, when fourteen people died in central London. The chapter analyses what TI and others did in response to this chance event, especially concentrating on TI's practices in their construction of surges. The chapter also looks at how the 1928 event led to changes in the patronage of TI's storm surge science, with a move towards local government, away from shipping industry patronage. The change of patronage was linked to a change of focus towards the forecasting of floods. TI however retained the idea of forecasts as corrections to tidal predictions and an emphasis on both increases and decreases in the water level, demonstrating the links between their work and the needs of their earlier industrial patrons.

The chapter analyses how TI's definition and construction of surges came to dominate the scientific report into the event. TI used their work on surges, first to get another contract to do further research into them, and then to fulfil their promise to their new clients, in part by convincing others to use their definition of surges. This convincing was done partly through rhetorical displays of expertise and partly by using established and new practices to make a number of surges into calculable objects for themselves and the Met Office. A key section of the chapter analyses how TI did this and how they constructed (as opposed to defined) surges as residuals. It looks in detail at the practices of calculation involved: how tidal gauge records and tidal predictions were made into numbers on documents which were in turn transformed into more numbers on other documents and eventually into graphs of 'surges' as residuals. This provides a detailed example of how TI's practices of calculation operated and how they constituted surges as calculable in a particular way, in a similar way to how Kalthoff's bank constituted risk or Lynch's scientists made specimens calculable.[124] However, I argue that in order to make storm surges predictable it was not enough to make them calculable, as exemplified by how despite work by both TI and the Met Office the events were still deemed unpredictable.

Chapter six analyses a number of events involving TI's storm surge work, especially concentrating on discussions regarding patronage from state actors for this work. The contested nature of patronage for research into storm surges was further emphasised in the decade after the 1928 event, through a dispute between London County Council (LCC), the Ministry of Health and the Treasury regarding who should fund proposed further research by TI into storm surges. In the end LCC gave in, nearly a decade after the event in 1937, and funded the work together with some other local authorities. With this the civil state had rejected becoming patron of storm surge science, but almost immediately after this, during the early part of the Second World War, the Navy became

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[124] Kalthoff, "Practices of Calculation."; Lynch, "The Externalized Retina."

interested in TI's work. In response to a request from Hydro in 1940 TI, who had already begun the research using one particular method, changed tack and produced a forecasting formula for the German coast using a different method involving statistics. I argue that the Navy's demands, together with problems with an earlier method TI had experimented with, impacted on the specific practices TI used and the type of formulae they constructed. After the war one of TI's research workers constructed a forecasting formula for Southend outside London, using similar methods to those used on the German coast during the war, i.e. statistical, and wrote up the work. To do this TI's researchers had to make choices regarding how to organise the data – for example which typology, theory and formulae to use. Such choices were contingent on many things such as the amount of work involved in calculations, the researcher's training and Hydro's request, and impacted on the results TI's work produced. I here develop my argument that chains of documents did not only connect but also bind.

In a curious incident, the report from the LCC work was not printed in the UK, but instead appropriated by the US Navy who had a very limited edition printed in the US, indicating further naval patronage of TI's storm surge work. More generally however, TI did not see the dramatic increase in naval patronage following the war that other historians of oceanography have reported. While the Navy and other state actors increased their patronage of oceanography in the UK too, this increase went to the new National Institute of Oceanography (NIO), an organisation led by George Deacon (1906-1984) who had worked on wave and swell forecasting for the Navy during the war. While Proudman and Doodson had been closely involved in its establishment, I argue that NIO represented a different kind of oceanography to that done at TI, which fits better into Mills's analysis of the development of dynamical physical oceanography. Following the contested establishment of NIO TI's patronage structure was re-affirmed, with an increased emphasis on research agreed with MDHB and the University. This meant that during the 1950s TI continued more or less as before in terms of overall patronage and governance structure. One exception to the general continuation of the existing patronage structures was for storm surge work, which saw a dramatic increase in state patronage following the major storm surge in 1953.

Chapter seven, the final empirical chapter, discusses this chance event, the 1953 East Coast flood, and the impact it had on storm surge science. The event led to a shift towards demand for, and patronage from, state actors for surge science, with generous central government funding given to TI's work on forecasting surges. The chapter first discusses how and why state actors became patrons, emphasising the contested nature of this process. In comparison to most secondary literature on the event I argue that there was substantial political pressure on the government from the opposition to provide funding and that this, together with the existence of research-supportive officers within the land drainage division of the Ministry of Agriculture and Fisheries, is why (parts of) central government became a patron of storm surge science. This patronage was in fact seen as a potential long-term saving by Cabinet ministers.

The new patronage changed the framework within which TI did their work, leading to a coming together – or even collision – of different practices, which in turn led to questioning of TI's practices both by others and themselves. As part of their work, they had made particular choices in terms of practices of calculation, in particular how they calculated winds. These choices were criticised during a debate in the mid 1950s but when TI responded by suggesting they change from statistical practices towards more theoretical work they were asked to continue with statistics. After another couple of years' work TI declared the statistical formulae as good as they could get. Using statistics, storm surges were now as predictable as TI thought they would get and TI wanted to develop other practices. Through material produced as part of the questioning of TI's wind calculations I take a last look at TI's practices. This pulls together various strands of my discussion about TI's practices of calculation, again emphasising how they through work and choices made chains of documents that were shaped in particular ways, and both connected and bound them. When they suggested a change away from statistical practices TI was suggesting that new chains of documents needed to be constructed to gain further decreases in the difference between predicted and observed residuals. They wanted to part-break their earlier chains, changing away from statistics but retaining the image of surges as residuals, but this did not happen in the 1950s.

As TI no longer wanted to construct surge forecasting formulae using the statistical practices of calculation they had honed since 1919, the end of the 1950s marks the end of the thesis. An additional push towards new practices of calculation was Doodson's retirement, aged 70. That, and the take-over of TI by the University in 1960, ending the patronage structure that had been in place since 1929, forms a suitable endpoint to the thesis.

CHAPTER 2 (1919-20), THE ESTABLISHMENT OF TI: SHIP SIZES, WAR AND TIDES

This chapter outlines the establishment of the Tidal Institute (TI), in particular who became a patron of the Institute and why. It sets this in the context both of previous work on tides, especially regarding the accuracy of predictions, and debates regarding the involvement of state actors in science at the end of the First World War. While the chapter concentrates on the establishment of TI, it also gives an understanding of the reasons for TI's initial work on storm surges. Today concerns with storm surges are set within the context of coastal flooding and climate change, but the initial work on meteorological effects on tides at TI was set within wider arguments for increasing the 'accuracy' of tidal predictions. The chapter also introduces actors that will recur throughout the thesis. How, and to what extent, did these different actors – Liverpool University, BAAS, Hydro and the shipping industry – become involved with TI?

In particular, what was the role of state actors in TI's establishment? There is debate in the literature regarding the links between the state, military and science at the time of the First World War. Some writers have argued that the state and military were slow to take up and support science, while others argue that it in fact got strongly involved at this time. For example, Andrew Hull argues a number of new scientific bodies were established with the involvement of the state and the military at the same time that TI was set up. In the UK the Department of Scientific and Industrial Research (DSIR) was key in this. As well as itself being one of the scientific bodies that were established during the war, it set up seven new research stations, such as the Radio Research Board, in the period 1917-1920. DSIR also took over the National Physical Laboratory (NPL) from the Royal Society and the Geological Survey from the Board of Education, and created Co-operative Trade Research Associations.[1] Hull sees the creation of these new organisations, and the establishment of various military-scientific bodies, such as the Board of Invention and Research at the Admiralty, the Munitions Invention Department for the Army and the Air Inventions Committee, as evidence for "a

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[1] Hilary Rose and Steven P. R. Rose, *Science and Society* (London: Allen Lane, 1969), 45; Ian Varcoe, *Organizing for Science in Britain* (Oxford: Oxford University Press, 1974); Sabine Clarke, "Pure Science with a Practical Aim: The Meanings of Fundamental Research in Britain, Circa 1916—1950," *ISIS* 101, no. 2 (2010).

dramatic wartime conversion of the British government to belief in the worth of scientific research in war and peace, and a major financial commitment".[2] Hull is arguing against authors such as Guy Hartcup who has claimed that stories of scientific and technological success during the First World War were mainly "relatively simple pieces of engineering". Hartcup further argued that chemical gas warfare and tanks failed to take off because of a "failure on the part of the '*user*' to appreciate the capabilities and limitations of these new weapons", not because of "technological failings"[3]. According to Hartcup, the military authorities had been complacent regarding R&D pre-war and during the war they were slow to realise the possible contributions of science.[4]

One reason behind TI was that the First World War had increased the interest in tidal matters, as its Secretary, Arthur Doodson (1890-1968), later noted.[5] In the introduction I discussed how historians of oceanography have found that the First World War led to an increased military interest in physical oceanography and hydrography.[6] For example, the UK Hydrographic Department (Hydro) organised an international conference in 1919 that led to the creation of the International Hydrographic Bureau in 1921. The Bureau was aimed at increasing exchange and standardisation of hydrographic information, including tidal data.[7] This increase of naval interest in oceanography may have provided a supportive environment for something like TI, but does not in itself explain its establishment. This chapter will analyse the establishment of TI as a specific case study of the debates regarding the role of state patronage of science at the end of the First World War. I argue that the question may need to be reformulated. Instead of asking if the war led to increased state and military funding of science generally, it may

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[2] Hull, "Passwords to Power", 79. Varcoe provides further support for the idea of a sudden increase in activity creating scientific organisations following the First World War. Varcoe, *Organizing for Science in Britain*, 44-46.
[3] Emphasis in original. Guy Hartcup, *The War of Invention: Scientific Developments, 1914-18* (London: Brassey's, 1988), 193. Other authors who have argued similarly include Jon Tetsuro Sumida, *In Defence of Naval Supremacy: Finance, Technology and British Naval Policy, 1889-1914* (London: Unwin Hyman, 1989); Hackmann, *Seek & Strike : Sonar, Anti-Submarine Warfare and the Royal Navy, 1914-54*; Roy M. MacLeod and E. Kay Andrews, "The Committee of Civil Research: Scientific Advice for Economic Development 1925–30," *Minerva* 7, no. 4 (1969).
[4] Hartcup, *The War of Invention*, 6, 161.
[5] Arthur Doodson, Personal Information File, Royal Society. Doodson will be further introduced in the next chapter.
[6] Weir, *Ocean in Common*; Deacon, "G. Herbert Fowler," 284.
[7] Weir, *Ocean in Common*; Archibald Day, *The Admiralty Hydrographic Service, 1795-1919* (London: Her Majesty's Stationery Office, 1967), 311-312.

be more useful to ask whether the war influenced what *kinds* of scientific work were done by different actors.

On a related note, I also discuss what kind of arguments garnered TI patronage. TI's founders were able to argue there was a need to increase the accuracy of tidal predictions following the war and the recent increase in the size of ships. It was by appealing to the needs of state and industry for better tidal predictions that TI received patronage, but who exactly was prepared to pay for their work? While state actors already had a long history of heavy involvement in tidal science they were not directly involved in TI's initial establishment and did not provide funding, but did influence TI's research programme. Instead financial patronage from industry, often not emphasised by historians of physical oceanography, was important for TI.

2.1.1 TIDAL WORK BEFORE TI

The Hydrographic Department had a deep interest in tidal science at the time of TI's establishment, as they had had at least since the early nineteenth century when Michael Reidy describes them as "the research and development wing of the British Admiralty". Reidy emphasises the close links between tidal research, the demands of merchant shipping and the military demands of the Admiralty in the early nineteenth century, arguing that tidal research enabled the British commercial and military empire to control the seas.[8] From 1833 onwards the Admiralty organised the collection of tidal gauge data and published tide tables, but the calculation of the tide tables were done by outside contractors.[9] The Admiralty continued to publish tide tables and be involved with tidal science throughout the rest of the nineteenth century and into the twentieth.[10]

Those who calculated the tide tables Hydro and others published used different methods. One method, the synthetic one, had been developed with support from BAAS in the 1820s and 30s.[11] William Thomson (Lord Kelvin, 1824-1907) and the American William Ferrel (1817-1891) then independently developed the harmonic

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[8] Reidy, *Tides of History*, 140. See also Hughes, "A Study in the Development of Primitive and Modern Tide Tables".
[9] Reidy, *Tides of History*, 116; Day, *The Admiralty Hydrographic Service, 1795-1919* 47.
[10] Day, *The Admiralty Hydrographic Service, 1795-1919*
[11] Reidy, *Tides of History*; Hughes, "A Study in the Development of Primitive and Modern Tide Tables".

method of tidal analysis and prediction in the 1860s. Thomson's work was done as part of a BAAS committee and expenses were paid by BAAS. The harmonic method was further developed by George Howard Darwin (1845-1912) in the 1880s, again with support from BAAS.[12] The BAAS thus had a tradition of supporting research into tidal predictions.

Darwin's version of the harmonic method of tidal analysis and prediction was critically revised by TI's researchers and it will therefore be described in a little more detail. In a book which was based on his Lowell Lectures in Boston in 1897,[13] he explained the idea behind the harmonic method of tidal predictions: "The analysis of tidal observations consists in the dissection of the aggregate tide-wave into its constituent partial waves, and prediction involves the recomposition or synthesis of those waves".[14] This synthesis could either be done by hand or by using a mechanical computer called a tidal predictor.

By analysing the tide-creating forces, or potential, of the moon and the sun, Darwin produced lists of astronomical constituents for a BAAS report in 1883. Such astronomical constituents represent the influence of different aspects of lunar and solar motion on the tides. Each constituent has an angular speed (related to its period and frequency) and an amplitude. Darwin also listed a number of other tidal constituents, related to non-astronomical influences on the tides, such as the effects on the tidal wave of shallow water. The depth of the water impacts the height of tides in many places including on the German coast and in estuaries such as the Thames or Mersey. Howevever, while he detailed these he also stated that he did not consider it necessary to analyse for other than the astronomical constituents and a few non-astronomical constituents of the particular type called compound tides to account for the periodic tides. His summary schedule of constituents included six compound tides out of 36 possible.[15]

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12 Hughes, "A Study in the Development of Primitive and Modern Tide Tables"; Cartwright, *Tides*.
13 Kushner claims the book was a "scientific bestseller" translated into several languages, see David Kushner, "Sir George Darwin and a British School of Geophysics," *Osiris* 8(1993): 206. The Lowell Institute was a well-endowed educational institute which ran a long series of both advanced and popular lectures, see http://en.wikisource.org/wiki/1911_Encyclopædia_Britannica/Lowell_Institute
14 George Howard Darwin, *The Tides and Kindred Phenomena in the Solar System: The Substance of Lectures Delivered in 1897 at the Lowell Institute, Boston, Massachusetts*, 2nd ed. (London: John Murray, Albemarle Street, 1901), 208.
15 George Howard Darwin, "Report of a Committee for the Harmonic Analysis of Tidal Observations," in *Report of the Fifty-Third Meeting of the British Association for the Advancement of Science, Southport - 1833, September* (London: John Murray, Albemarle Street, 1884), esp p 52-53, 65-78 & 99.

Darwin discussed the 'correctness' of such harmonic tidal predictions, admitting that the weather often caused problems:

> The utmost that can be expected of a tide-table is that it shall be correct in calm weather and with a steady barometer. But such conditions are practically non-existent, and in the North Atlantic the great variability in the meteorological elements renders tidal prediction somewhat uncertain.[16]

Though he said the effect of barometric pressure was well known, with an inch change in mercury being equal to a change of sea level of just more than a foot, and winds known to be a major cause of errors to tidal predictions, Darwin did not think meteorological effects could be predicted. He did not believe forecasting formulae for meteorological effects could be developed.[17]

Despite these problems Darwin believed harmonic theory produced successful, 'good enough', predictions and that this was evidence it was a true theory for the tides: "Prediction must inevitably fail, unless we have lighted on the true causes of the phenomena; success is therefore a guarantee of the truth of the theory".[18] He claimed that the set of constituents his schedule for harmonic analysis produced "contain a *complete* record of the behaviour of the sea at the place in question".[19] To Darwin, wind and barometric pressure was a source of error to otherwise trustworthy predictions. He drew a sharp line between these meteorological effects and ordinary 'true' periodic tides, which he thought his theory and methods adequately captured and could predict accurately.

Methods of predicting tides were further developed by staff at Hydro before and during the First World War. In 1912 a full-time tidal officer, Commander Harold Dreyer Warburg (1878-1947), was appointed by Hydro. Warburg had entered the Navy in 1894, concentrating on surveying from ships from 1899 until his eyesight stopped him doing this work in 1910, when he began office-based work, soon concentrating on tides.[20] Following his work on prediction methods, computation of some non-harmonic tide

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[16] Darwin, *The Tides and Kindred Phenomena in the Solar System*, 219.
[17] Ibid., 220.
[18] Ibid., 225-226.
[19] My emphasis. Ibid., 199.
[20] J. F. Parry, "Meeting for the Discussion of Geophysical Subjects, Wednesday, 1918 June 12," *The Observatory* 41(1918); Day, *The Admiralty Hydrographic Service, 1795-1919* 325-326.

tables was taken in-house, with an additional computer hired, though Hydro continued to have harmonic predictions for waters further afield, especially diurnal (once-daily) tides, done by outside contractors.[21] In 1917 Warburg was appointed Superintendent of Tidal Work.[22]

Hydro outsourced harmonic predictions as these were done using special tidal predictor machines and the department did not have one. Together with others William Thomson had developed these predictor machines, see figure 2.1 for an example. They are a form of mechanical analogue computers, or a type of automatic integrating machine.[23] The machines added together the previously found harmonic constituents for a particular port to produce a prediction.[24] Thomson had a bitter dispute with one of his collaborators, Mr E Roberts, regarding who should be considered the inventor of tidal predictors.[25] In the end Mr Roberts built his own tidal predictor machine, and established a business in Broadstairs producing tidal predictions. This business was later taken over by his son and continued to provide the Admiralty with predictions. At the end of the First World War the two outside contractors Hydro used was this company, Messrs. Roberts & Son, and the NPL which had another machine.[26]

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[21] Day, *The Admiralty Hydrographic Service, 1795-1919* 257.

[22] Ibid., 325-326.

[23] James S. Small, *The Analogue Alternative: The Electronic Analogue Computer in Britain and the USA, 1930-1975* (2001); Michael R. Williams, *A History of Computing Technology* (London: Prentice-Hall, 1985); Crosbie Smith and M. Norton Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989), 370-371; Arthur Thomas Doodson, "Tide-Predicting Machines," *Nature* 118, no. 27 November (1926).

[24] For more on the operation and invention of tidal predictor machines, see Hughes, "A Study in the Development of Primitive and Modern Tide Tables"; Cartwright, *Tides*.

[25] Described in detail in Hughes, "A Study in the Development of Primitive and Modern Tide Tables", ch 9.

[26] Day, *The Admiralty Hydrographic Service, 1795-1919* 257.

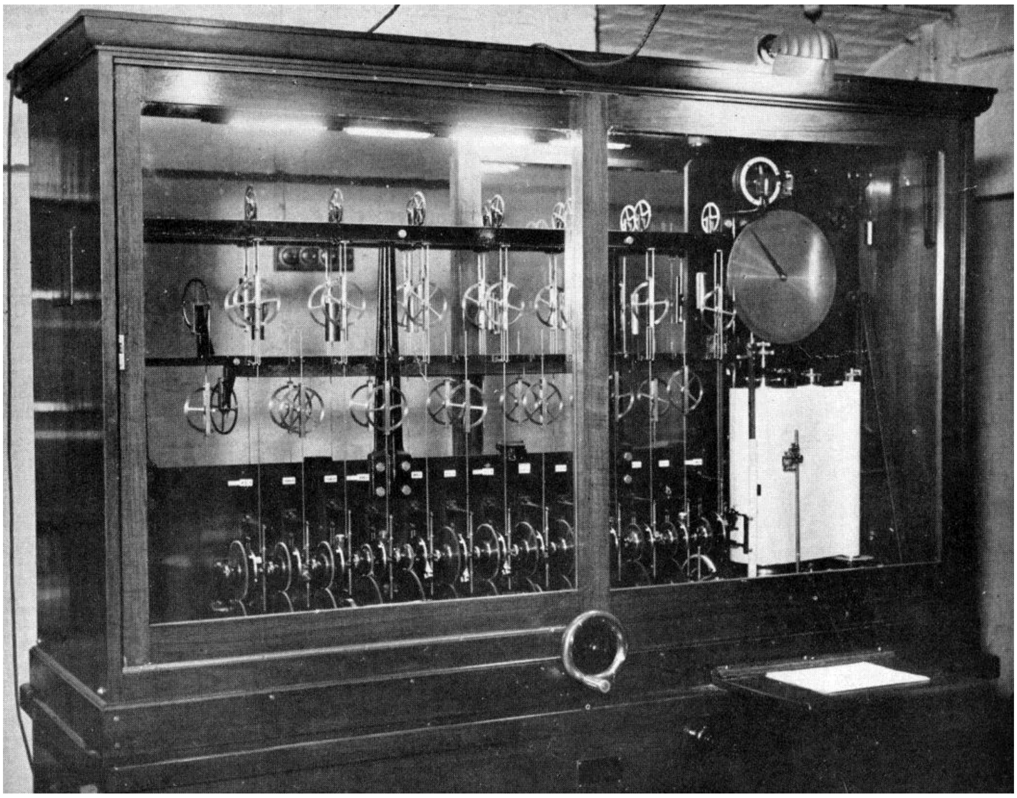

Figure 2.1: A Kelvin tidal predictor, used by TI from 1924[27]

The following, written by Warburg in 1921, describes in a somewhat idealised manner how Messrs. Roberts & Son went about producing predictions:

> A staff of two is permanently employed: (a) a man of great age (Mr. Roberts says 80) who is the computer, that is to say he carries out all the hack work of computing harmonic constants, and (b) a girl who reads times and heights from the machine curves. Mr. Roberts states that he always works the machine himself, checks both computations and curve readings personally, and personally calculates the harmonic constants from the computer's results. ....
>
> The staff employed are certainly highly expert and efficient and the general methods leave but a few loopholes for errors. Each machine curve is checked by means of 5 computed heights (the National Physical Laboratory checked each by 2 only); the times and heights are written on the curves themselves and

[27] Permission to reproduce in unpublished material kindly given by National Oceanographic Centre, Liverpool. I thank J Eric Jones there for providing me with a copy of the picture and allowing me to use it.

> printed proofs checked from the curves, and in fact it is difficult to see how methods could be improved, but the work proceeds with such rapidity that errors undoubtedly do occur.
>
> Mr. Roberts has reduced actual work to the lowest possible limit; for instance in the computation of constants much division is required – he has his own M.S. Tables for dividing any numbers with any other; these are complete tables, i.e. they give the required result without interpolation of any sort; it is only by shortening work in this way he can get on with so small a staff.
>
> He employs no calculating machines, stating that an expert computer with the necessary tables is both quicker and more accurate than any machine.[28]

In this description Warburg emphasised the practices of calculation used at Messrs. Roberts, such as how they checked and simplified the work to increase accuracy and efficiency. While Warburg was positive regarding these practices, he also claimed that the speed with which the work was done led to errors. This high speed was necessary for the company to make a profit.

Hydro's interest in tides and tidal research further increased during the First World War, as the war stopped the earlier exchange of tidal predictions between different countries, including between Germany and England. To substitute for these Warburg had first tried what he called the "scientific" harmonic method, but had found it "unsatisfactory", giving large differences between predicted and observed tides for the North Sea ports that were crucial for the war. Instead he had invented a "rough unscientific method" to predict these tides which he claimed "gave good results".[29] War-induced needs had led Warburg to identify problems with the harmonic method, arguing that it was not producing 'good enough' predictions, in opposition to what Darwin had claimed at the end of the nineteenth century. He argued further work in this area was necessary, implicitly arguing for further funding.

Since the first half of the nineteenth century there had been close links between military, industry and science regarding tidal work. In the early twentieth century such links

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[28] "Methods, staff, etc.", part of Minute from Warburg to Hydrographer, 28 Sep 1921, in HYD 587/1921, within H 4434.23, UKHO

[29] Parry, "Discussion of Geophysical Subjects," 286.

existed between Hydro, NPL (run by the Royal Society for much of this period), Messrs. Roberts & Son and also the users of predictions, both naval and merchant shipping. The state, in the form of Hydro, was heavily involved in the calculation of tidal predictions and had been so since the nineteenth century, both doing 'routine' work and research, but so were industry and scientists. In addition, regarding tidal predictions Hydro had its own R&D programme in the form of Warburg's work, which forms a case study against to those who argue the military did not take scientific research seriously before and during the First World War.

2.1.2 HOW TO JUDGE THE ACCURACY OF A TIDAL PREDICTION?

The (insufficient) accuracy of tidal predictions was identified as an important issue at the end of the First World War by Hydro. In February 1919 Warburg gave a detailed technical presentation of his new methods for predicting tides to the Royal Geographical Society. In this he emphasised that while the number of tidal predictions published by the Admiralty had increased radically between 1833 and 1912 "there had been no corresponding increase in accuracy" though "the increase in size, speed and draft of vessels" had made a higher accuracy desirable well before the war, which he claimed was why he had begun work on it.[30] During the discussion of Warburg's paper, Hydrographer Parry emphasised the importance of accurate tidal predictions to seamen. He stated that during the recent war the laying of mines, the avoiding of torpedoes and the rapid movement of ships had relied on tidal predictions: "if a vessel is being chased, it may be a question of life or death to her to be able to decide whether she may cross over a shoal or not [i.e. decide what the depth of the water is, influenced by the tide], and the disadvantage of not being able to cross and having to go round the shoal is obvious to us all".[31]

Earlier Darwin had judged harmonic tidal predictions to be accurate, but now Hydro said they were not sufficiently accurate. In one sense judging the accuracy of tidal predictions was a 'simple' comparison between the predicted and observed tide in a

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[30] Harold Dreyer Warburg, "The Admiralty Tide Tables and North Sea Tidal Predictions," *The Geographical Journal* 53, no. 5 (1919): 308-309.
[31] J. F. Parry *et al.*, "The Admiralty Tide Tables and North Sea Tidal Predictions: Discussion," *The Geographical Journal* 53, no. 5 (1919): 327.

particular location at a particular time. However, this comparison was of course far from simple. While tides are continuous, tidal predictions of times and height were usually given only for high and possibly low water.[32] These times and heights were thus the numbers whose 'accuracy' was at stake when talking about the accuracy of tidal predictions,[33] but Warburg pointed out that for seamen it was more important that the time of high water was accurate than the height.[34] In his paper Warburg spent considerable time defining what he meant by accuracy as "[t]he proper judging of the accuracy of tidal predictions is not a simple matter".[35] In the end he developed point-scoring system for predictions, so that those closest to measured records got the highest scores, and produced percentage measures of the degree of accuracy of tidal predictions. He claimed this showed that his new equation method of predicting tides produced more accurate predictions than the method usually used by the Hydrographic Department.[36]

To increase accuracy was to reduce the difference between observed and predicted times and heights of high and low waters. However, judging what this difference was involved complex comparisons and judgements of tidal gauge records and predictions. Firstly, the accuracy of tidal gauge records compared to actual sea levels was by no means assured, as there could be measurement errors, for example if the gauge's clock was wrong, or issues with where and how the gauge had been placed – one part of a port might have a different tidal pattern to another – or there might be problems with deciding the datum.[37] Even if the tidal gauge record was deemed to represent sea levels at a chosen place closely enough, these records included meteorological effects, which tidal predictions made no pretence of including, so to judge the accuracy of the predictions in comparisons with the records it was necessary to take meteorological

32 See e.g. *Harbour Regulations and Tide Tables from Januarly to December, 1911, for the Port of Lagos, Southern Nigeria*, (Lagos: Government Printer, 1910).
33 Arthur Thomas Doodson, "Report on Harmonic Prediction of Tides," in *Report of the Eighty-Eighth Meeting of the British Association for the Advancement of Science, Cardiff - 1920, August 24-28* (London: John Murray, Albemarle Street, 1920), 321-322.
34 Warburg, "The Admiralty Tide Tables and North Sea Tidal Predictions," 310.
35 Ibid.: 311.
36 Warburg 1919. Warburg seems to use the term 'degree of accuracy' as a quantitative term for the accuracy of a large number of predictions whereas he uses the term 'accuracy' in general discussion and when talking about one specific prediction, compare Gooday, *The Morals of Measurement*, 57.
37 For an example of such problems, see Shankland to Doodson, 22nd May 1928, Box 16, BA

effects out of the records.[38] However, the difficulties in calculating what the meteorological effect were made it difficult to take them out of the records. Warburg had identified this as a key problem, but had not had time to work on it, so had instead chosen to assess the accuracy of the predictions during the summer, when meteorological effects should be smaller.[39]

Secondly, the accuracy of predictions by themselves was another issue, both analysing for constants and calculating predictions using such constants. Predictions were connected to observations through the process of analysing tidal gauge records for the constituents that were used in the predictions – if the observations were poor the results of the analysis would also be poor. In addition, choices during the process of analysis, such as how many or which constituents to analyse for, could impact on the predictions. Another issue was that only a limited number of the potentially very large number of harmonic constituents found could be included in machine calculations, as tidal predictors were physically limited in terms of the number of constituents they could be set to include. Even if the fullest practicable harmonic development of tides was used, there were non-harmonic aspects of the tides that a standard harmonic prediction did not necessarily include, such as further shallow water effects.[40] The analysis and predictions also relied on complicated calculations that needed not only to follow the latest theory of how to calculate tidal predictions but also to follow mathematical rules without mistakes.[41] The use of tidal predictor machines introduced potential calculation errors which needed to be accounted for when gauging the accuracy of predictions.[42] Warburg pointed out that harmonic predictions produced by different people using the same constants differed substantially, linking this to such machine errors.[43]

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[38] Emphasis added. Liverpool Tidal Institute, *Tidal Institute: First Annual Report, 1920* (Liverpool: University of Liverpool, 1920), 7-8.
[39] Warburg, "The Admiralty Tide Tables and North Sea Tidal Predictions."
[40] Emphasis added. Liverpool Tidal Institute, *Tidal Institute: First Annual Report, 1920*, 7-8.
[41] This immediately raises issues of rule-following, which is philosophically problematic – how to know that the rules have been applied correctly is not necessarily obvious, even if we can decide what the rules are. See Warwick, "The Laboratory of Theory," 313-316; Harry M. Collins, *Changing Order : Replication and Induction in Scientific Practice*, 2nd ed. (London: The University of Chicago Press, 1992).
[42] See folder "Machine tests", Box 126, BA and Arthur Thomas Doodson, "To Assist Works on the Tides," in *Report of the Eighty-Ninth Meeting of the British Association for the Advancement of Science, Edinburgh - 1921 September 7-14* (London: John Murray, Albemarle Street, 1922), 243.
[43] Warburg, "The Admiralty Tide Tables and North Sea Tidal Predictions," 310.

As Graeme Gooday has emphasised, judgements of measurement accuracy depended on many different issues and varied over time and situation.[44] The same held for judgements of the accuracy of tidal predictions. While Darwin considered that his harmonic method produced accurate predictions at the end of the nineteenth century, Hydro did not think so at the end of the First World War. In addition there were multiple judgements and issues, such as regarding the accuracy of observations including meteorological effects, involved in this matter. The accuracy of measurements, calculations and predictions were all connected together and what the necessary accuracy was or what accuracy meant was often left vague. This meant that talk of and work on the accuracy of predictions could easily be used for rhetorical purposes, such as when Warburg argued for further work in this area during his presentations.

2.1.3 PROUDMAN'S CAREER BEFORE TI

Warburg was not the only new tidal researcher in the 1910s. Another was Joseph Proudman (1888-1975), who later became TI's Director. His father was a farm bailiff and tenant farmer in Bold, between Widnes and St Helens (in Merseyside), and he himself was a pupil teacher between 1902 and 1907 in nearby schools. As he was not from a privileged background he needed to earn a living through his scientific work. In 1907 he became a scholarship student of Mathematics at the University of Liverpool where he gained the B.Sc. in 1909 and B.Sc. Honours in 1910. Again as a scholarship student he then went to Trinity College, Cambridge, to study pure and applied mathematics. After passing the Mathematical Tripos in 1912, with distinction and first class grades in the final exam, he spent a third year at Cambridge doing research. This is when he became interested in tides.[45] Proudman later described how he went to Cambridge with a view to do research on electricity and chose courses accordingly. However, after he had sat his exams he claims he could not, despite talking to a number of mathematicians in Cambridge, including Joseph Larmor, find anyone who could give him the "definite problem" he felt he needed. His Director of Studies eventually

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[44] Gooday, *The Morals of Measurement*, 268.

[45] This was a couple of years after the nineteenth-century Wrangler system had been abandoned in 1909. Warwick, *Masters of Theory*, 284-285.

recommended that Proudman write to Horace Lamb (1849-1934), Professor of Mathematics at Victoria University of Manchester, who "[b]y return of post" sent him a problem in the theory of tides Proudman deemed suitable.[46] As part of his work on hydrodynamics Lamb had worked on the mathematical theory of tides and other types of oceanic waves such as tsunamis.[47] After this Proudman's research career centred on the dynamical theory of tides.

After his student years in Cambridge, in 1913, Proudman took up a post as lecturer in mathematics at Liverpool University. He continued to teach at Liverpool when in 1915 he became a Fellow of Trinity College, spending summers in Cambridge. During most of the First World War Proudman remained at Liverpool due to "being placed in a low medical category" but, on top of the tidal work, he also worked on ballistics at the Research Department of Woolwich Arsenal for the last half of 1918.[48] His health problems may have been due to recurrent psychological issues rather than physical problems, as two later episodes of what was called exhaustion, nervousness and insomnia have been recorded.[49] At Woolwich he produced a paper on "the gyroscopic dirft [sic] of a shell". He had also worked with Thomas Bertrand Abell (Professor of Naval Architecture), who in turn worked with the Admiralty.[50] Proudman thus had some personal networks with the military world.

Much of TI's mathematical research was based in a tradition of mathematical physics rooted at Cambridge,[51] in particular the development there of Laplace's work on the hydrodynamic theory of long waves. TI's statistical work on forecasting storm surges was complemented by work in this tradition, for example developing solutions to related dynamical problems for simplified basins.[52] Proudman was TI's main practitioner of this hydrodynamical work and their link to the Laplacian tradition. How did he acquire the skills to operate in this tradition? He claims to have "felt no need"

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[46] Joseph Proudman Biographical lecture D 212/2, LUA
[47] A. E. H. Love and R. T. Glazebrook, "Sir Horace Lamb. 1849-1934," *Obituary Notices of Fellows of the Royal Society* 1, no. 4 (1935).
[48] *Joseph Proudman,* Personal Records of Fellows of the Royal Society, Royal Society Archive, 8.2
[49] In summer 1932 he was given leave for exhaustion and nervousness, see documents in D/BO 1/5/1, MMM – North Street. In 1951 Doodson mentioned to Hydrographer Day that Proudman "is troubled with insomnia, is not allowed to see anyone and has just received a term's leave of absence." Doodson to Day, 2nd Feb 1951, D/BO 1/4/17, MMM – North Street.
[50] University Council Report book, 1918, S2466, LUA
[51] Warwick, *Masters of Theory*.
[52] See e.g. Arthur Thomas Doodson, "Meteorological Perturbations of Sea-Level and Tides," *Geophysical Journal International* 1, no. s4 (1924).

for coaching, nor for tutorial classes, once at Cambridge, instead claiming lectures and working out of examples were "completely satisfactory" for him and his teachers.[53] This partly reflects the changes the Cambridge mathematics course was undergoing at the time of Proudman's attendance. Following the reforms to the Mathematical Tripos in 1909 fewer used private coaches and the subjects students were choosing were changing, away from the earlier emphasis on the solution of physical problem.[54] However, it also indicates that he had already acquired some of the skills necessary to pass the Cambridge mathematics exams successfully before arriving at the university.

In addition, Proudman seems to have been torn between the 'old' and 'new' Cambridge styles, claiming to have learnt most from RA Herman, whom he described as "of the old Cambridge school" and whom Andrew Warwick calls a leading coach.[55] However, he also claims that he was "much attracted" to the kind of mathematics practiced by Littlewood and Hardy, whom he called "pure mathematicians of a new type in England" whose work Warwick suggests attracted many students under the new examination system.[56] Yet, when Proudman failed to find an attractive problem in electricity, he turned back to physically related problems solving in the tradition of mathematical physics.

While Warwick discusses how former wranglers turned public school teachers were important in preparing students for the Cambridge Tripos in the nineteenth century, Proudman did not go to public school.[57] However, Warwick also mentions that Ebenezer Cunningham and Harry Bateman, both Cambridge-trained mathematicians, were holding junior lectureships in mathematics at Liverpool University at about the same time Proudman was a student there.[58] Proudman describes his undergraduate teaching at Liverpool as including "work[ing] out vast quantities of examples" set,

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[53] Joseph Proudman Biographical lecture D 212/2, LUA
[54] Warwick, *Masters of Theory*, 280-285; Andrew Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905-1911 Part II: Comparing Traditions in Cambridge Physics," *Studies In History and Philosophy of Science Part A* 24, no. 1 (1993): 2-3. Proudman claimed many were still using coaches when he attended Cambridge.
[55] Warwick, *Masters of Theory*, 283. Joseph Proudman Biographical lecture D 212/2, LUA
[56] Ibid., 434. Joseph Proudman Biographical lecture D 212/2, LUA
[57] Ibid., ch 5.
[58] Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905-1911 Part I: The Uses of Theory," 639 & 644.

corrected and criticised by his lecturers, all of which had been wranglers.[59] This indicates that he was developing some of the specific problem solving and learning skills used by Cambridge mathematicians while he was at Liverpool University, taught by former wranglers, much like the public school students discussed by Warwick. Further investigations of how, and the extent to which, teaching methods first developed at Cambridge spread to mathematics departments at other universities in Britain when wranglers lectured there is something that could extend Warwick's thesis regarding how mathematical teaching methods have travelled.

2.2.1 The Liverpool setting: The shipping industry and the university

Both Liverpool University and the Liverpool shipping industry were involved in the establishment of TI. Their interests in TI and tidal work are introduced here, beginning with the shipping industry. When TI was established in 1919 the shipping man Alfred Booth gave £350 per year towards payment of the salary for the Secretary of TI while his brother Charles Booth, also a shipping man, gave £50 per year towards working expenses, both for five years.[60] This was the core of the early TI's funding, but why did these shipping men provide funding for tidal science? No direct evidence of why the Booths decided to fund TI appears to exist, so this section attempts to find a more contextual answer.[61] I argue that the Booths were part of a modernising faction of the Liverpool shipping community who owned and managed ever-larger ocean liners, had links with university science as well as the state and were keen on increasing the throughput of the port, spurred on by the First World War.

The Booth brothers, Charles (1868-1938) and Alfred (1872-1948), had obvious and direct interests in the success of shipping, particularly in Liverpool, with Charles being chairman of the Booth Steamship Company and Alfred chairman of the Cunard Steamship Company, but also more generally. Alfred Booth had studied Mathematics at King's College, Cambridge, graduating in 1894. He did not come very high in the order

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[59] Joseph Proudman Biographical lecture D 212/2, LUA. The Professor, FS Carey, had been third wrangler about 1880 and another lecturer, James Mercer, was senior wrangler in 1905, according to Proudman.
[60] Gift book 1 Oct 1903-Sep 1912, Feb 1919, S81, LUA
[61] See note 97, this chapter, for references regarding TI's establishment

of merit at shared 29th position in Part I, but his mathematical training gave him a chance of understanding what Proudman's work was about and the proposed research TI would do.[62] He had been Chairman of the Board of Trade Committee on the Shipping and Shipbuilding Industries and member of Lord Balfour's Committee on Commercial and Industrial Policy in 1916, so was involved in national politics.[63] Charles Booth was similarly involved in a range of organisations and companies. Crucially he was on the University Council for many years, forming a link between the shipping world and the university. He was also involved in port-related organisations, as Chairman of the Employers Association of the Port of Liverpool (1919-1938) and later a member of the Mersey Docks and Harbour Board (1924-1938).[64]

Another of TI's industrial patrons was the Mersey Docks and Harbour Board (MDHB), a large organisation heavily dominated by shipping interests that had run Liverpool port since 1858.[65] From 1920 MDHB were represented on the governing committee of TI, and thus another of TI's patrons which became gradually more and more important during the first decade of its existence. MDHB had had a long-standing interest in tides and their prediction, as evidenced by the existence of committee papers on tidal matters, for example discussing several investigations into differences in tide tables produced by different organisations.[66] MDHB was interested in tides as the height of the tides determined when ships of different sizes could access the port.[67] As the size of ships increased with the introduction of ever-bigger ocean liners before the war this became more and more of an issue. Table 2.1 shows the increase in tonnage per vessel from 375 tonnes per vessel in 1880 to 996 tonnes in 1919, indicating that ships became larger. New docks, able to cope with ships with 34 foots draught, were said to be needed by companies running passenger and cargo ocean liners, as the current docks

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[62] Cambridge University Reporter, 29th May 1894, p 846, and 12th Jun 1894, p 904. He does not appear to have sat the Part II exam, which only the more advanced students sat after another years work, see Warwick, *Masters of Theory*, 267-268.
[63] REGISS database, "Who's Who - 1897-1998."
[64] Ibid.
[65] The Board had 28 members, 24 of whom were elected by users of the Docks (so primarily shipping men) and 4 were nominated by the Mersey Conservancy Commissioners. Stuart Mountfield, *Western Gateway : A History of the Mersey Docks and Harbour Board* (Liverpool: Liverpool University Press, 1965).
[66] MDHB WUP T111 "Liverpool Tide Tables," Liverpool Maritime Museum Archive
[67] James N. Shoolbred, "The Tidal Regime of the River Mersey, as Affected by the Recent Dredgings at the Bar in Liverpool Bay," *Proceedings of the Royal Society of London. Series A, Containing Papers of a Mathematical and Physical Character* 78, no. 523 (1906).

could not deal with ships with such draughts.[68] In response to such demands for larger and deeper docks with wider entrances and other facilities, the Docks Board organised new dock facilities and 'improvements' to the Mersey, including 'training banks' to control the channels of the river and dredging in the Edwardian period.[69] Another concern was that such work could lead to changes in the tides.[70]

A key factor behind industrial demands for increasing accuracy of tidal predictions was thus the increasing size of ships with the introduction of ocean liners. Another was the First World War, as it led to a large increase in ships using Liverpool docks which created congestion, labour shortages and raised costs. In 1915 the Liverpool Committee for the Co-ordination of the Naval, Civil & Military Requirements of the Port was established by central government in 1915, to reduce the congestion of the port as this was said to be important for the war effort. It had representation from a range of interests, including not only the ship-owners of MDHB but also the Navy, labour, railways and others, and was chaired by Alfred Booth.[71] Initially the MDHB criticised this new committee,[72] but according to Francis Hyde, an economic historian of the port, "measures were taken as a result of the war to improve efficiency [i.e. increase the port's throughput]", so the MDHB's initial resistance was overcome.[73] At the time of TI's establishment the Liverpool shipping industry was divided into factions. MDHB has been characterised by Adrian Jarvis as having a "cosy environment, high average age and a low turnover of members" at this time, which he claims led to poor policy making and poor performance monitoring, including some spectacular overspends on poorly managed projects.[74]

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68 Mountfield, *Western Gateway*, 140.
69 Ibid., 105-109,115-125.
70 Shoolbred, "The Tidal Regime of the River Mersey, as Affected by the Recent Dredgings at the Bar in Liverpool Bay."
71 Tidal predictions does not seem to have been considered by the committee. D42/C1/1/18, Liverpool Committee for the Co-ordination of the Naval, Military and Civil Requirements of the Port, LUA. Mountfield, *Western Gateway*, 126-134.
72 Ibid.
73 Francis Edwin Hyde, *Liverpool and the Mersey : An Economic History of a Port 1700-1970* (Newton Abbot: David & Charles, 1971).
74 Adrian Jarvis, *In Troubled Times : The Port of Liverpool, 1905-1938* (St John's, Newfoundland: International Maritime Economic History Association, 2003), 91-112.

Year	Total number of vessels	Total tonnes net registered tonnage	Tonnage per vessel
1880	20,070	7,524,533	375
1881	20,249	7,893,948	390
1882	20,966	8,104,136	387
1883	21,315	8,527,531	400
1884	23,940	8,800,362	368
1885	21,529	8,571,454	398
1886	20,598	8,370,723	406
1887	21,884	8,797,783	402
1888	22,241	9,017,935	405
1889	22,662	9,291,964	410
1890	23,633	9,654,006	408
1891	22,775	9,772,506	429
1892	22,304	9,968,697	447
1893	21,206	9,468,539	447
1894	21,170	9,960,902	471
1895	23,943	10,777,146	450
1896	23,695	11,046,459	466
1897	23,640	11,473,421	485
1898	24,664	11,815,376	479
1899	25,522	12,534,116	491
1900	24,870	12,380,917	498
1901	24,334	12,648,539	520
1902	24,214	13,308,305	550
1903	24,827	14,537,751	586
1904	25,400	15,626,241	615
1905	26,065	15,996,387	614
1906	25,773	16,147,856	627
1907	25,635	17,064,211	666
1908	25,739	17,111,814	665
1909	24,799	16,747,479	675
1910	24,961	16,654,071	667
1911	25,377	17,600,888	694
1912	23,483	17,327,415	738
1913	24,982	18,433,269	738
1914	24,756	19,086,672	771
1915	22,562	18,980,913	841
1916	18,742	15,679,943	837
1917	16,747	14,018,652	837
1918	11,855	11,687,204	986
1919	12,372	12,324,010	996
1920	17,115	16,521,373	965

Table 2.1: The total number of vessels and the total tonnage for which MDHB were paid rates, the ratio of which shows the increase in tonnage by vessel between 1880 and 1920[75]

[75] Mountfield, *Western Gateway*, Appendix IV.

There was conflict between the traditionalist MDHB and the modernising war committee, led by Alfred Booth, during the First World War. The modernisers appear to have won out, leading to the 'efficiency drive' identified by Hyde, which was continued after the war. It led not only to further attempts to cater for larger ships, e.g. through building works to widen the entrance to the Alfred Docks, but also to changes in the management of MDHB, such as its financial routines.[76] This efficiency drive coupled with the longer term trend of increasingly larger ships and a tradition of donations to the university seems the most likely reason why the Booths funded TI. Within such an efficiency drive funding research on improving tidal predictions would have made sense as it was supposed to help reduce accidents, such as ships grounding due to lower than expected tides, and also increase throughput, as water depths would be known – supposedly – with more certainty, so shipping could be allowed to enter and leave for longer. As we will see below and in the next two chapters, in funding applications TI repeatedly argued that 'inaccurate' tidal predictions caused dangers, delays and expense for shipping, which their work could fix.[77]

2.2.2 Liverpool University

The University formally established the new TI, and is thus an important patron for the Institute, but provided very little funding. TI was not unusual in being hosted by the University but funded by local shipping industry, as the University's own history of patronage shows. Throughout the University's history it had relied heavily on local industry, especially shipping, for funding; a common pattern for English provincial universities at this time. When Liverpool University College was set up in 1881 much of the funding came from local subscribers, including £6230 by ship owners towards a chair of Mathematics.[78] As funding from other sources increased, the proportion from industry decreased, but the shipping industry and traders continued to contribute significant sums, for example to establish the School of Tropical Medicine in 1899 (supported by amongst others the Booth Line, which traded with Brazil), and a chair in

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[76] Ibid., 136-146.
[77] LSOA General Minutes, part 2: 1920-1964, (illegible date) April 1923, Vol 29, D/SS/2/4, MMM
[78] Thomas Kelly, *For Advancement of Learning : The University of Liverpool, 1881-1981* (Liverpool: Liverpool University Press, 1981), 48-49. In 1884 the college joined Victoria University and then in 1903 Liverpool became an independent University.

Naval Architecture in 1909.[79] Much research work was done for the shipping industry, e.g. on using cement in waterwork construction, cold storage machinery and marine propulsion using internal combustion engines.[80]

Donations from the shipping industry to the university continued during and after the First World War. For example, in 1920 ship-owners and industrialists endowed four chairs in engineering.[81] Though not directly from the shipping industry, one of the war-time donations is of particular interest due to its links to TI's work. In 1919 a Department of Oceanography was created, endowed by Natural History Professor Herdman and his wife. Biological oceanography had existed at the University since the 1880s with a marine biology station established in 1887 and moved to Port Erin, Isle of Man, in 1892, when a Fisheries Laboratory also became part of the Natural History department.[82] Herdman had successfully argued that this research was important for the fishing industry and got monetary support from local industry as well as government for this "applied biology".[83] The Department of Oceanography initially concentrated on biological oceanography, so was not in direct competition with TI's mathematically-based physical oceanography. Its establishment was mainly Herdman's way of leaving a legacy but also seems to be part of the general increase in interest in oceanography following the war, which TI was similarly part of. If there was any competition between the Herdmans's Department of Oceanography and Proudman's TI this has not left any traces in the sources.

Given this context of constant and repeated donations by shipping men to the university, it is not surprising that industrial patronage was important to TI, despite the lack of comparators within history of oceanography. However, we now turn to how it came to be that TI was founded through a benefaction from shipping men such as the Booth brothers, because this was not the initial plan.

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79 Ibid.
80 June Jones, "Science, Utility and the 'Second City of the Empire': The Sciences and Especially the Medical Sciences at Liverpool University 1881-1925" (PhD, University of Manchester, 1989), 73-74.
81 Kelly, *For Advancement of Learning : The University of Liverpool, 1881-1981*, 245.
82 Ibid., 72-73.
83 Jones, "Science, Utility and the 'Second City of the Empire'", 94-100.

2.3 THE ESTABLISHMENT OF TI: THE BAAS CONNECTION

During the First World War a large BAAS committee of senior scientists under section A (Mathematics and Physics) set up a survey of the state of geodetic research, in part they said to keep alive scientific interest in geophysics following George Howard Darwin's death.[84] The committee argued that a Geodetic Institute, covering geodesy, seismology, terrestrial magnetism and tides, was necessary and would give much useful help to the state. They also argued that there existed no specialised research institution for work on these topics and, critically, no British institution that could provide scientific advice to government on these topics.[85] The arguments for the proposed Geodetic Institute are likely to have been part of the wider agenda to increase the links between science, military and government, which has been identified at BAAS and elsewhere in relation to the establishment of DSIR,[86] and the emphasis within BAAS to use science for "national efficiency".[87] These BAAS scientists were arguing for increased funding for *theoretical* geodetic research by framing their proposed institute as assisting the state.

These arguments can be exemplified by the tidal survey. As part of the wider BAAS survey Horace Lamb prepared a report on the current state of research on tides. Lamb asked Proudman, his protégé, to help with the report.[88] However, their report to the BAAS concentrated on academic aspects of tidal science. It claimed that though existing tide-tables were 'sufficiently accurate' for practical needs, 'improved' predictions of

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[84] The non-tidal members of the committee were astronomer Frank Watson Dyson, who chaired it, physicist Charles Chree (who reported on magnetic observations), Charles F Close (Director General of the Ordnance Survey), astronomer and physicist James Hopwood Jeans, mathematician Augustus Edward Hough Love, geologist Henry George Lyons, astrophysicist Hugh Frank Newall, physicist Arthur Schuster, Napier Shaw (who reported on meteorology), geologist Aubrey Strahan, astronomer and seismologist Herbert Hall Turner, George W Walker (who reported on seismology) and Major EH Hills (who reported on geodesy and surveying). Frank W Dyson, "Meeting for the Discussion of Geophysical Subjects, Wednesday, 1917 Nov 7," *The Observatory* 40, no. 520 (1917): 444.

[85] Organising Committee of Section A, "Reports on Physical Sciences for Which World-Wide Observations Are Important," in *Report of the Eighty-Seventh Meeting of the British Association for the Advancement of Science, Bournemouth: 1919, September 9-13* (London: John Murray, Albemarle Street, London, 1920), 27-31.

[86] Roy M. MacLeod, "Scientists, Government and Organised Research in Great Britain 1914-16," *Minerva* 8, no. 1 (1970); Roy M. MacLeod and E. Kay Andrews, "Scientific Advice in the War at Sea, 1915-1917: The Board of Invention and Research," *Journal of Contemporary History* 6, no. 2 (1971); Hull, "War of Words."

[87] Roy M. MacLeod, "Retrospect: The British Association and Its Historians," in *The Parliament of Science: The British Association for the Advancement of Science, 1831-1981*, ed. Roy M. MacLeod and Peter Collins (Northwood: Science Reviews, 1981), 2.

[88] Joseph Proudman, "Arthur Thomas Doodson, 1890-1968," *Biographical memoirs of the Fellows of the Royal Society* 14(1968): 193.

periodic tides were needed to further theoretical research on the dynamic theory of tides. One issue was uncertainties in the harmonic constants for long-period periodic tides which were due to “meteorological disturbances”.[89] In other words, storm surges needed to be analysed so they could be separated from periodic tides, which in turn would lead to improvements of the general (Laplacian) theory of tides. The report did not mention such improvements as being linked to improvements in predictions as published by the Admiralty. Overall this report emphasised the theoretical aspects of tidal science over the need to improve predictions for practical reasons, arguing that funding for theoretical but not practical research was necessary. There was a disconnect between the message of the main geodetic committee, claiming the proposed institute would assist the state, and the message of Lamb and Proudman, not discussing state needs but instead focusing on theoretical research.

The Hydrographer argued there was a similar disconnect between academic tidal research and the work of ‘practical men’. As part of their campaign for increased state support of geodetic research the BAAS committee organised meetings at the Royal Astronomical Society to bring together workers in the field of geophysical sciences.[90] In 1918 one of these geophysical discussions covered tides and was attended both by Proudman and key Hydro staff, including the Hydrographer Parry who chaired. At this meeting Warburg, Lamb and Proudman all presented work. The Hydrographer claimed to have only accepted to chair the discussion to emphasise the need for co-operation between “practical” men, like himself and Warburg, and “scientific” men, like Lamb and Proudman. He thought such co-ordination was lacking and pointed out that “knowledge of the tides was vital to the seaman, especially in war”.[91] Yet despite this call for increased tidal research, quoting the war as one reason it was needed, Hydro did not offer monetary assistance either to the proposed Geodetic Institute or to the Tidal Institute.

In the end the proposed Geodetic Institute petered out, as other actors including the Royal Society were working towards setting up a similar institute at Cambridge

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89 Horace Lamb and Joseph Proudman, "Preliminary Report on Tides and Tidal Currents," in *Report of the British Association for the Advancement of Science, 1918* (London: John Murray, Albemarle Street, London, 1919), 15.
90 Dyson, "Discussion of Geophysical Subjects."
91 Parry, "Discussion of Geophysical Subjects."

University funded not by the state but by private benefaction.[92] BAAS decided to let others organise – and fund – geodetic research, though they continued to argue in favour of it.[93] In relation to DSIR Hull has argued that the BAAS was more radical than the Royal Society in its demands for increased state funding of science at this time. The Geodetic Institute may be a further example of such institutional politics, and as with DSIR, the Royal Society and in this case also Cambridge University 'won out', with the result of fewer demands for state funding of science.[94]

This section has discussed some of the academic reasons for establishing TI, primarily to further research on tidal theory, and also that TI's establishment was part of a wider context of contested arguments for increasing scientific involvement with the state. However, while these arguments for theoretical research from BAAS had not been enough to get funding, the proposed tidal branch of the Geodetic Institute turned into TI.

2.4.1 The establishment of TI

How was TI established? In early 1918 Proudman had hoped to become head of the tidal department of the proposed Geodetic Institute but then later in the year, as the full Institute became less likely, he started formulating plans for setting up a separate tidal institute at Liverpool, to be funded by shipping men. By November he had formulated a memorandum to be put to these shipping men via the professor of mathematics, FS Carey.[95] While this memo appears to have been lost, at this stage Proudman clearly reformulated his proposal in a way he thought would appeal to industry, which worked. In early 1919 he had a meeting with Charles Booth, member of the Council of the University, who apparently called the proposed Tidal Institute a "capital idea" and said he would put the idea to his brother.[96] A few days later in early February Charles told Proudman that he and his brother Alfred Booth would provide funds of £400 per year

[92] Organising Committee of Section A, "Reports on Physical Sciences for Which World-Wide Observations Are Important," 27-31.
[93] EH Griffiths and EO Henrici, "The Urgent Need for the Creation within the Empire of a Central Institution for Training and Research in the Sciences of Surveying, Hydrography and Geodesy," in *Report of the Eighty-Eighth Meeting of the British Association for the Advancement of Science, Cardiff - 1920, August 24-28* (London: John Murray, Albemarle Street, 1920).
[94] Ibid.
[95] Doodson to Margaret, 19th Feb 1918, 17th Jun 1918 and 19th Nov 1918, Doodson Papers
[96] Doodson to Margaret, 4th Feb 1919, Doodson Papers

to support TI for five years.[97] The University then became involved, accepting the Booths' offer and establishing TI "as proposed in the letters" from the Booth brothers.[98] With this TI was established.

Proudman became Director, a role for which he was not paid but instead remaining employed by the University, and Arthur Doodson Secretary of the new Institute, which was housed by the University, providing it with a room in the Holt Physics Laboratory. As rooms were scarce at the University this shows it wanted to encourage the Institute,[99] but while they were clearly happy to be involved in TI they did not fund it, providing only a minimal grant (initially £10 per year).[100] When TI was established its governing committee had only six members and was dominated by the University and the Booths,[101] but by 1920 it had grown to 15 members. While there was a strong contingent of shipping men, consisting of the Booths and representatives of MDHB, scientific representatives had numerical dominance of the governing committee at this point.[102]

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[97] *Joseph Proudman,* Personal Records of Fellows of the Royal Society, Royal Society Archive, 9.1. Sources that discuss the early TI include "The Liverpool Observatory and Tidal Institute" (Draft article, Proudman to Roberts, 16th May 1935, D/BO 5/1/1, MMM – North Street), Joseph Proudman Biographical lecture (D 212/2, LUA); Cartwright and Ursell, "Joseph Proudman. 30 December 1888 - 26 June 1975."; Proudman, "Arthur Thomas Doodson, 1890-1968."; Liverpool Tidal Institute, *Tidal Institute: First Annual Report, 1920*; Joyce Scoffield, *Bidston Observatory: The Place and the People* (Birkenhead: Countyvise Ltd, 2006); J. Eric Jones, "From Astronomy to Oceanography - a Brief History of Bidston Observatory," http://www.pol.ac.uk/home/history/jejhist.pdf. A few letters from TI's secretary Arthur Doodson to his future wife mentions discussions with Proudman on the proposed TI, but they do not cover why it was proposed (Doodson to Margaret, 19th Feb 1918, 17th Jun 1918 and 19th Nov 1918, Doodson papers). No letters to or from Proudman or the Booths on this issue seem to have been preserved. The establishment of TI is recorded in the University Council Minutes, but this gives little information (University Council Minute 18th Feb 1919, S2221, LUA). The University Gifts book provides confirmation of the Booth's monetary gifts, with Alfred Booth giving £350/yr towards payment of the salary for the Secretary of TI while Charles Booth gave £50/yr towards working expenses, both for five years (Gift book 1 Oct 1903-Sep 1912, Feb 1919, S81, LUA). These official University notes are the only direct primary sources on TI's establishment. Other actor's Minutes (e.g. MDHB main Board and its Marine Committee) do not even record the establishment of TI. The main archive of TI consists of material collected after TI had been established and has nothing on this very early stage.

[98] University Council Minute 18th Feb 1919, S2221, LUA. The letters from the Booths do not seem to have been kept.

[99] Doodson to Margaret, Feb 12th 1919, Doodson papers

[100] Tidal Institute Ledger, S2147, LUA

[101] The University set up a small committee with six members (the two Booths, Proudman, the Professor of Mathematics FS Carey, the President of the University Council and the Vice-Chancellor). University Council Minute Book 14, Minutes from meeting on 18th Feb 1919, S2221, LUA

[102] By 1920 the Committee had grown to 13 members: the same two official University representatives, the two Booth brothers, five academics (Proudman; Carey; R Hargreaves, Reader in Applied Mathematics; Horace Lamb; and L R Wilberforce, Professor of Physics), three representatives from MDHB (Sir Francis C Danson, C Livingston and F W Mace, MDHB's Marine Surveyor) and finally W E Plummer, Director of Bidston Observatory (somewhere between an academic member and the shipping

Another of TI's early sources of funding was BAAS. In the wake of the proposed Geodetic Institute Section A (Mathematics and Physics) of the BAAS set up a tidal committee chaired by Doodson.[103] In the autumn of 1919, TI's first year, this committee was given the largest grant within section A of £150, which went to TI to pay for staff to do computational work. After this the BAAS funding of the committee quickly declined, e.g. to only £35 in 1920, and completely stopped in 1922.[104] The role of BAAS and this funding will be returned to in the next two chapters.

2.4.2 Proudman's motivations for establishing TI and his career after TI

Proudman was a finder of opportunities and funding for himself and his chosen field, for example when he contacted Lamb asking for a research problem or when he was able to convince the Booths to fund TI. He however had to put much effort into trying to find a niche and support, balancing his life, for example teaching at Liverpool and researching at Trinity College, and applying for funding, for example from the Booths. As we will see in the remainder of this chapter and the following two Proudman looked for funding for TI wherever he could think of, gradually adjusting how he portrayed TI's research programme to fit the audience. He invented his institution building strategies as he went along.

As the memo to the Booths have not been preserved it is unclear exactly how Proudman framed the Institute to them, but it seems likely it was as a combination of theoretical work, as in the BAAS reports, and as work aimed at improving tidal

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men of MDHB). In 1921 two more academics, W A Herdman (Emeritus Professor) and J Johnstone (Professor of Oceanography) were added to the committee. Liverpool Tidal Institute, *Tidal Institute: First Annual Report, 1920*; Liverpool Tidal Institute, *Tidal Institute: Second Annual Report, 1921* (Liverpool: University of Liverpool, 1921).

103 The other members were Horace Lamb (chair), Charles F Close (Surveyor General), Philip Herbert Cowell (Director of the Nautical Almanac Office), Horace Darwin, G Herbert Fowler (oceanographer, see article by M Deacon), Hydrographer Learmouth, Proudman, Geoffrey Ingram Taylor, D'Arcy W Thompson, Joseph John Thomson and Herbert Hall Turner. The committee was dominated by Doodson and TI's work, see folder HD 1472/1920 within HD 1401/1500/1920, UKHO.

104 *Report of the Eighty-Eight Meeting of the British Association for the Advancement of Science, Cardiff - 1920, August 24-28*, (London: John Murray, Albemarle Street, London, 1920), xx; *Report of the Eighty-Ninth Meeting of the British Association for the Advancement of Science, Edinburgh - 1921 September 7-14*, (London: John Murray, Albemarle Street, London, 1922), xxxi; *Report of the Ninetieth Meeting of the British Association for the Advancement of Science, Hull - 1922 September 6-13*, (London: John Murray, Albemarle Street, London, 1923), xxvi.

predictions, which the Institute's research programme later emphasised. For a few years starting in 1915 Proudman collaborated with Geoffrey Ingram Taylor (1886–1975), a Cambridge mathematician and physicist, on the theory of the disturbances to a rotating fluid caused by moving solid.[105] Proudman wrote to Taylor that TI was set up in order to take the suggested work from the BAAS report forward by "one man spending the whole of his time on material relating to *actual* tides".[106] As Proudman later described the rationale for TI, the BAAS study and his own experience from six years of theoretical research on tides led him to believe "there was an opportunity for an institute" in Britain researching tides.[107] While an important part of spotting this opportunity was finding gaps in academic work that could be filled by the Institute (as done in the BAAS report), another part of it was Proudman's realisation that such an Institute was potentially fundable by the shipping industry, if related to actual tides.[108] While he was still emphasising the more theoretical work suggested by the BAAS report at this point, this was now to be done on actual, as opposed theoretical, tides, though he still did not discuss tidal predictions. However, his discussions with the Booths also began a process of making him aware of the industrial need for tidal prediction in a way he claimed not to have been before establishing TI.[109]

In addition, for Proudman, establishing TI not only furthered his academic research on tides but also his own career. Following the establishment of TI, he was upgraded (without competition from other candidates) from Lecturer to Chair of Applied Mathematics at Liverpool from October 1919.[110] That establishing TI was important to his career can also be seen from his Royal Society Certificate of Election from 1925. Almost half of the "Qualifications" section on this was spent on TI and its work, with the rest concentrating on his own work on the dynamical equations of the tides for

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105 The collaboration led to the formulation of what is now called the Taylor or Taylor-Proudman column, Cartwright and Ursell, "Joseph Proudman. 30 December 1888 - 26 June 1975," 324.

106 My emphasis. Proudman to Taylor, 18th Mar 1919, GIT D66, Trinity College Library, Cambridge

107 Proudman, "Arthur Thomas Doodson, 1890-1968," 193.

108 Doodson to Margaret, 19th Nov 1918, Doodson Papers

109 Joseph Proudman, "Report on Harmonic Analysis of Tidal Observations in the British Empire," in *Report of the Eighty-Eighth Meeting of the British Association for the Advancement of Science, Cardiff - 1920, August 24-28* (London: John Murray, Albemarle Street, 1920). and Proudman to DSIR, 8th Jul 1919, DSIR 36.13.4, NA

110 University Council Minute 4th Nov 1919, S2221, LUA

which he had won the Adams Prize in 1923.[111] In 1933, when the previous professor, James Johnstone, retired, Proudman took over the Department of Oceanography, refocusing it on physical oceanography.[112]

2.5 The role of the Navy in the establishment of TI

For Proudman establishing TI was an important part both of supporting his chosen topic of research and of establishing his own career. For Liverpool University, TI was another research institute funded by local shipping men, while for those funders TI was part of a wider modernising agenda aimed at increasing the throughput of Liverpool port and make the passage of the increasingly large ships they owned and managed safer and faster. While all the actions and motivations by those involved should be seen with the First World War and an increased role by the state in supporting science in the background, TI was established without state support or involvement. While it was part of the creation of new scientific organisations described by Hull it was not directly linked to state actors at this early stage through funding, especially not after the collapse of the plans for the Geodetic Institute.[113] However, while the state initially provided no funding, a particular part of it, the Hydrographic Department of the Admiralty, quickly became involved in defining TI's research programme.

As part of developing TI's research programme the scientists there started corresponding with Hydro in late summer 1919, exchanging information and asking for advice. The first preserved letters are from August 1919, more than half a year after TI was established, and fairly formal and impersonal. While this is indicative of the initially distant relationship between the two organisations, the letters also show how Hydro became increasingly involved with TI's work. In their correspondence Hydro told TI what they thought were the main problems with current tidal predictions, for example that they were particularly poor for shallow water ports. This was an issue identified

111 Proudman; Joseph (1888-1975) Elected 1925, FRS Certificate of a Candidate for Election, see http://www2.royalsociety.org/DServe/dserve.exe?dsqIni=Dserve.ini&dsqApp=Archive&dsqCmd=ImageView.tcl&dsqDb=Catalog&dsqImage=EC_1925_11.jpg. Last accessed 25th June 2010.
112 Jones, "Science, Utility and the 'Second City of the Empire'".
113 Hull, "Passwords to Power".

from Warburg's work on prediction accuracy during the war.[114] In these letters Hydro tried to influence TI's work, suggesting it should work on the issues with predictions identified by Warburg. TI took them up on their suggestions, but not literally. For example, Warburg explained to the Hydrographer in 1920 that he had told Proudman that he disagreed with TI's choice of port for in-depth analysis, as he thought Newlyn tides had too little shallow water effects to provide a good basis for new ways of calculating the tides, but TI continued working on Newlyn tides.[115] Without paying for the work, Hydro tried to influence TI's tidal research to fit its own agenda and to some extent achieved this aim.

About the time of TI's establishment there were discussions within the Hydrographic Office regarding the quality of the tidal predictions they purchased. When the India Office moved its tidal predictor machine from NPL in Teddington to India in 1920, Messrs. Roberts & Son became the only external provider of predictions to the Hydrographic Office.[116] Several staff at the Hydrographic Office discussed whether they should purchase their own machine and take at least some of the calculations in-house. Doing this would prevent Messrs. Roberts & Son from becoming a monopoly and also reduce the government's dependence on private business which could potentially increase the charges or reduce their services without warning.[117] Both Hydrographer Learmonth and the Director of Scientific Research, FE Smith, initially argued that Hydro should purchase a tidal predictor. However, Warburg then paid a visit to Messrs. Roberts & Son. His report, a part of which was quoted above in section 2.1.1, reduced Hydro's concerns about the quality of predictions and the possibility of a sudden cessation of provision sufficiently to put the matter on hold for a few years. In addition, finance was an issue for Hydro which influenced the decision to stick with Messrs. Roberts & Son.[118] This discussion further supports my earlier contention that Hydro's staff were concerned about the quality of tidal predictions at this time. However, despite their frustration with the state of tidal research they were not involved in TI's

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[114] Correspondence in folder "Machine tests", Box 126, BA Doodson acknowledges this in his work, Doodson, "Report on Harmonic Prediction of Tides."
[115] Minute, 3rd Jul 1920, H 4567/20, UKHO
[116] I have found no information on why the India Office moved their machine.
[117] "Proposal to establish an Admiralty tide predicting machine," Memorandum by Hydrographer Learmonth, 25th May 1921, H4434/23, UKHO
[118] Documents in file HYD 587/1921, within H4434/23, UKHO

establishment and did not provide any direct funding to the young TI – it was not even mentioned in the debates regarding the supply of predictions.

While my findings regarding Hydro's lack of involvement in TI's establishment may seem to support those who have argued that the Navy was slow on the uptake of academic science, this is not how I interpret the situation. Instead, as at the Royal Astronomical Society geophysical meeting on tides in 1918, they wanted to encourage 'scientists' to work with 'practical' men, like themselves, to improve tidal predictions. The Navy was keen to communicate and co-operate, but does not at this stage appear to have even considered that financial support of TI from the Navy's limited resources would be a way to do so.[119] At this time there was not an obvious way for state and science to co-operate; instead the form of such co-operation was up for discussion.

If anyone was, it was Proudman who was slower on the uptake of the naval needs for tidal research. He later claimed that he had not realised how bad the predictions were seen to be by seamen until after he had established TI, implying he had not picked up on Hydro's complaints during the geophysical meetings.[120] However, as we now turn to, it is clear that the complaints and suggestions of Hydro and other seamen like the Booth brothers changed TI's early research programme towards one which emphasised tidal predictions.

2.6 TI'S RESEARCH PROGRAMME

TI's research programme, as presented to potential or actual patrons, changed as TI was established. It changed from the theoretically oriented research programme developed as part of the BAAS report which had failed to get funding, towards a research programme which received funding from shipping men and was also influenced by TI's discussions with Hydro.[121] In TI's first annual report, written by Proudman and Doodson and presented both to TI's actual and potential funders, a key justification for TI's existence and funding was said to be to improve tidal predictions generally. It is

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119 Documents in file HYD 587/1921, within H4434/23, UKHO.
120 Proudman, "Report on Harmonic Analysis." and Proudman to DSIR, 8th Jul 1919, DSIR 36.13.4, NA
121 Liverpool Tidal Institute, *Tidal Institute: First Annual Report, 1920*.

thus a first example of how the patronage TI received from shipping men, interested in tidal predictions, interacted with their stated research programme.[122]

TI's First Annual Report constructed the research problem of the Institute by stating that existing tidal predictions faced several problems. Some of these problems had been pointed out to them by Hydro, while TI had identified others through their own work. Like Warburg had, they mentioned that for a port such as Liverpool the existing tidal predictions varied substantially between different suppliers of prediction. Second, when comparing predicted and observed tidal heights, using records from tidal gauges, there were also large differences TI's researchers called residuals. TI saw the reduction of these residuals as important to increase the 'accuracy' of tidal predictions, and a crucial aspect of this was in turn to investigate meteorological effects, as can be seen from their frequent mention in this programmatic statement of TI's proposed research in its First Annual Report:

> In predicting tides we want firstly an accurate record of observations taken over a number of years; secondly, an accurate analysis of the record so as to discover the laws which the normal tide follows and to disentangle the irregular *meteorological effects*; thirdly, an accurate method of predicting the normal tide; and lastly, a method of predicting the irregular *meteorological effects*. At the present time these wants are far from being satisfied, but it has been the general opinion that only the last is serious. It may be remarked that the *meteorological effects* cannot well be investigated until we know accurately what the tides would be without them.[123]

TI's research programme, as presented to its potential and actual patrons in its First Annual Report, thus emphasised accurate tidal predictions. In order to predict tides more accurately predicting meteorological effects was seen as a key task.

TI's objectives were of course wider than that covered by the above quote: the Institute's aims were to do scientific research into tides, to train students in applied mathematics, be a "bureau of organised information concerning the tides" and to do

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[122] More examples of this will follow, especially in chapter four.
[123] Emphasis added. Liverpool Tidal Institute, *Tidal Institute: First Annual Report, 1920*, 7-8.

commercial research.[124] While their research programme itself was wider and included work not directly linked to predictions, it is clear that the way in which Proudman framed the needs for the work changed as his audience changed. With shipping men as key patrons and in collaboration with Hydro, he now emphasised the importance of improving tidal predictions for the use of shipping that he had denied were necessary when writing the report to BAAS with Lamb. Within this research programme the need to study storm surges was emphasised, which illustrates that initially the study of storm surges at TI was not linked to flooding but to a wider programme of increasing the accuracy of tidal predictions. The next chapter will look at how TI tried to implement this research programme.

2.7 Conclusion

This chapter has analysed how TI was established and has introduced some of TI's early patrons: Proudman, Liverpool University, BAAS, the Booth brothers and MDHB, and also discussed the role of Hydro. TI's initial patronage represented a combination of academic and industrial interests, with the Navy providing TI with the resource of research ideas. While TI's heavy reliance on industrial patronage was common at Liverpool University, as at other similar universities in England at the time, it makes TI a relatively unusual case in the history of physical oceanography which has focused on naval patronage. The First World War was an important background factor for an increase in interest in tides and oceanography generally, as it had cut off the supply of tidal predictions, led to congestion in ports such as Liverpool and seen the use of submarines. However, while the Navy was neither directly involved in the establishment of TI nor provided funding, fairly quickly it became involved in the definition of TI's research programme.

In relation to the debates regarding the role of the First World War in increasing state involvement in scientific research, such an increase was not obviously evident in TI's establishment. The BAAS plans were not given state funding and in the end TI relied on industrial funding. In TI's case the question posed by historians needs to be framed differently. It was not so much whether the First World War led to increased state

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124 Ibid., 5.

support of science in general, but whether it led to increased interest in and support of particular *types* of research. That it certainly did: the TI that eventually resulted had a research programme that was focused on tidal predictions, instead of the more theoretical programme pictured as part of the proposed Geodetic Institute, and this was linked to Hydro's concerns and to TI's patronage from the Booths, which in turn was linked to the efficiency agenda prompted by the war. In addition, the border between a patron providing resources and a non-patron is not straightforward. While Hydro neither provided finance, like the Booths, nor space, like the university, it did provide assistance with research topics. As we will see in chapter four Hydro's patronage would grow from this beginning, as it explored various ways of co-operating with TI and influencing its work.

CHAPTER 3 (1920-23), TIDAL PREDICTIONS AND PRACTICES OF CALCULATION

The previous chapter showed that the key argument TI used to get funding was that its work would increase the accuracy of tidal predictions. This increase in accuracy had to do both with increasing the accuracy of periodic tidal predictions and with providing 'corrections' to such predictions to account for irregular meteorological effects. This chapter argues that TI attempted to increase the accuracy of tidal predictions by introducing new practices of calculation, such as new types of mechanical calculators and statistics. According to TI's researchers their practices of calculation were new. Arthur Doodson, for example, claimed that he was introducing a new way of calcuating constituents which was faster and more accurate.[1] As Andrew Warwick has suggested, such changes in the practices of calculations to increase accuracy need a historical and contextual explanation.[2] A context for these changes was provided in the previous chapter: TI's work to introduce new ways of calculating tidal predictions was set in the context of the First World War and a shipping industry increasingly using larger ships. In addition, their new ways of doing such calculations were inspired not only by scientific work but also by ballistics calculations done by Doodson, TI's secretary, during the war.

When offering him the job of secretary, Joseph Proudman linked Doodson's "ability" or skills to the future patronage of TI:

> When the number of years [of funding from the Booths] is up (and possibly before) the work done will be reviewed and if it is considered to warrant it, an attempt will be made to fund a permanent institute on a larger basis, by appealing to the remaining ship owners of Liverpool. From what I know of the subject and your ability I am certain that if you come now the thing will be a success.[3]

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[1] Doodson, "To Assist Works on the Tides," 218.
[2] Warwick, "The Laboratory of Theory," 317.
[3] Proudman to Doodson, quoted in Doodson to Galloway, 6th Feb 1919, Doodson papers

The future of the institute, especially its ability to get funding, thus depended on it showing it could improve the accuracy of tidal predictions used by TI's shipping industry patrons. This linked patronage to the practices of calculating tidal predictions.

Warwick has argued that new technologies of calculation enabled the application of the new nineteenth century mathematical analysis both to the need of business and military, and to scientific work.[4] His short piece on this concentrates on technologies of calculation such as table making and does not provide a detailed case study of how a particular "problem of generating numbers from analytical expression" was affected by these developments.[5] This chapter provides such a case study, showing how the industrial and military setting outlined in the previous chapter came together with the practices of calculation Doodson learnt during his early career, to change how the particular problem of generating tidal predictions from harmonic theory was done in order to make them more accurate. This was a key part of Proudman's and Doodson's institution building for TI. To justify their work and funding, TI's researchers had to convince others that the work they did needed doing, which they did by arguing that earlier methods were not 'accurate enough'. As Graeme Gooday has made clear the definition used by scientists and others of 'accurate enough' often changes over time: "What counted as accuracy was what constituted a sufficient degree of accuracy for a particular purpose to be undertaken within existing contextual constraints of money and time to the satisfactions of relevant audiences".[6] This applied not only to precision measurement, but also to precision calculations. Such arguments were also not only about science but also about patronage from state, industrial and other actors.

3.1 ARTHUR DOODSON AND THE PRACTICES HE BROUGHT TO TI

I will begin this chapter by introducing TI's key workers, first Arthur Thomas Doodson (1890-1968) and then the computers. In 1913 Arthur Thomas Doodson was one of Proudman's first research students, which is how the two met. When TI was established in 1919 Proudman was Director whilst also holding down other posts, while Doodson became the senior full-time member of Institute staff as Secretary. I will here describe

[4] Warwick, "The Laboratory of Theory," 313.
[5] Ibid., 344.
[6] Emphasis in original. Gooday, *The Morals of Measurement*, 268.

Doodson's career before he started at TI in some depth, including the influence of debt, deafness and religion, using letters from him as a primary source. While obviously one-sided, these letters provide a contextual understanding of the skills he acquired during his early career which help in understanding TI's practices of calculation and how they went about trying to increase the accuracy of tidal predictions.[7] His skills were in precision engineering and the use of mechanical calculators, as well as in doing statistics, applied mathematics and manual calculations.[8] In terms of research, his key skills were those of organising mathematical problems in a way that was amenable to manual calculations and then organising and managing these calculations; skills he would later use at TI.

Like Proudman, Doodson was not from a privileged background. Doodson's father was for a time a cotton mill manager, but had problems with employment; attendant financial difficulties meant Doodson could not attend full time secondary school. Again like Proudman, he was a pupil-teacher while also doing half-days and evening classes at various schools in Leigh and Rochdale outside Manchester.[9] Initially Doodson wished to become a teacher and started studying sciences at Liverpool University in 1908 while also training as a teacher, but as he became seriously deaf while a student he could not pursue this career. He gained a first class B.Sc. degree in 1911 in chemistry and

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7 Much of the biography I present here is based on letters Doodson wrote to his wife-to-be Margaret Galloway. In 1954 Doodson collected together some of these letters and put a selection into a folder which is now part of the Doodson papers at Liverpool World Museum. The selection is far from complete (often only part of letters have been preserved) and are self-selected. It is clear from some of the notes he has had added that the selection he made concentrated on his scientific work, but it also covers his early career in some details. There is little on his relationship to Margaret or e.g. religion, though the letters discuss some personal matters such as finance and his conscientious objections to the war. The same folder also contains a few letters to his university colleague Nightingale.
Some caution in the interpretation of Doodson's letters to his girlfriend is necessary, as he will have wanted to present his work in a good light to her and in extension to her parents, to convince them he was progressing in his career sufficiently so that he would eventually have the income it was felt he needed in order to marry Margaret. However, while the introduction of new practices of calculation may not have been as smooth as Doodson told Margaret, he did progress in his career during this period and it seems likely that it was his skills in organising and managing calculation that enabled this, especially as biographies of him (e.g. his FRS one by Proudman) similarly emphasise his skills in this area. The letters to Margaret provide the best available evidence of how Doodson acquired these skills. They also offer some insight into these kinds of practices and how Doodson worked.
8 While Doodson used his skills in light engineering and precision measurements at TI e.g. in redesigning tidal predictor machines, they are less relevant to the argument and will not be emphasised. For something about how he used these skills, see Doodson, "Tide-Predicting Machines."
9 Doodson Personal Information File, section 6, RSA

mathematics and then studied for an honours degree in mathematics which he gained in 1912 (also first class).[10]

Finding employment proved difficult with his disability but he worked first as a meter-tester at Ferranti in Hollinwood from late 1912. Doodson there set about learning electrical theory through self study, formal classes and by discussions with a superior. About a year into his post he claimed to have been able to fix a meter, using this knowledge of electrical theory, and also to have been able to successfully defend his way of fixing the meter to the Head Tester, who had not initially believed Doodson did it correctly.[11] Self study and long hours were a key part in how Doodson acquired skills at this point.

Money was very short for the Doodson family and he had run up debts while attending university so Doodson was continually looking around for better paid opportunities.[12] In the summer of 1914 the managers at Ferranti had been asked to recommend a person for a post at Manchester Corporation and Doodson was nominated.[13] He started work there in August at the Testing and Standardizing Department as "polyphase meter and instrument tester" and stayed there throughout the beginning of the war.[14] Doodson found the work at Manchester Corporation more interesting than at Ferranti's as it was less routine and involved "plenty of experience on all kinds of instruments taking them to parts and calibrating them to 1/10th per cent accuracy". He was in charge of testing the candle power of lamps and how they distributed light, whilst also calculating the results of all the testing and doing other work such as experimenting on meter discs.[15]

At the same time as this precision testing and standardisation work he was also working on precision calculations. In 1913 he registered as a part time research student at the Department of Mathematics of the University of Liverpool where Proudman supervised him. Here his skill in organising computations started to be developed whilst working on Riccati-Bessel functions and on tables of sines and cosines of radians, and he was

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10 Proudman, "Arthur Thomas Doodson, 1890-1968."
11 Doodson to Galloway, 9th Dec 1912 and "probably October 1913", Doodson papers
12 Doodson to Galloway, 1st Apr, 1st, 11th, and 18th Mar 1914, 14th Jun 1914, Doodson papers
13 Doodson to Galloway, 14th , 20th Jul 1914, Doodson papers
14 Doodson to Galloway, 1st Aug & 14th Oct 1914, Doodson papers
15 Doodson to Nightingale, 3rd Dec 1914, 4th Aug 1915 Doodson papers

awarded a M.Sc. degree in 1914.[16] Doodson sent these tables to the BAAS committee on mathematical tables, at this time organised by John William Nicholson, which published them and offered him a place on the committee which he took up for two years in 1915 and 1916.[17] His calculation work, often done while travelling or during lunch breaks, here gained him entry to national-level networks of professional mathematicians.[18]

Following his Master's work Doodson continued his collaboration with Proudman, doing calculations to produce tables and graphs related to what they called the Diffraction Problem; or more formally the diffraction or scattering "of a plane electromagnetic wave by a perfectly conducting sphere". The work went on through 1914 and into 1915, in preparation for publication by the Royal Society.[19] In letters to Margaret, his wife-to-be,[20] Doodson explained how he introduced new practices of calculations, not included by Proudman, to make the work more accurate by for example introducing checks on the arithmetical procedures.[21] These practices of calculation was what made Doodson's work different from others' work. He wrote to Margaret that he was "astonished to see the mass of interesting results" he had obtained as he had "succeeded in making the component curves depend on other curves of small amplitude and which can thus be easily graphed on a large scale … I have resolved the curves into simpler components which show the structure".[22] This is one example of

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16 Proudman, "Arthur Thomas Doodson, 1890-1968."
17 Doodson to Galloway, 14th May 1914, Doodson papers. For more on the Mathematical Tables Committee, see Martin Campbell-Kelly and Mary Croarken, "Beautiful Numbers: The Rise and Decline of the British Association Mathematical Tables Committee, 1871-1965," *IEEE Annals of History of Computing* 22, no. 4 (2000); Warwick, "The Laboratory of Theory." For more on mathematical tables, see Campbell-Kelly *et al.*, *The History of Mathematical Tables*.
18 Proudman, "Arthur Thomas Doodson, 1890-1968."
19 Doodson to Galloway, 20th Jul, 1st Aug, 17th Dec 1914, 14th Mar 1915, Doodson papers, Joseph Proudman, Arthur Thomas Doodson, and G. Kennedy, "Numerical Results of the Theory of the Diffraction of a Plane Electromagnetic Wave by a Perfectly Conducting Sphere," *Philosophical Transactions of the Royal Society of London. Series A, Containing Papers of a Mathematical or Physical Character* 217(1918).
20 Margaret had been to school and university with him, studied science or mathematics, helped him with calculations, created paper versions of slide rules and was teaching science at a girl's boarding school. Doodson to Galloway, 2nd Mar 1916, Doodson papers
21 Doodson to Galloway, 20th Jul, 1st Aug, 17th Dec 1914, 14th Mar 1915, Doodson papers
22 Doodson added that he did not have time to give details. Doodson to Galloway, 17th Jun 1915, Doodson papers

Doodson practicing the skills involved in going from mathematical theory to calculation, in this case by working out a way to divide curves into constituents.[23]

Through 1915 the war became an ever bigger concern for Doodson. He had been deeply religious, as was Margaret, since about 1914 and was an active member of a local church. This church was part of "The Churches of God in the Fellowship of the Son of God", a splinter group from the Plymouth Brethren (which in turn is an Evangelical Christian group, often socially conservative and separate from other free churches).[24] Despite joining recently his religion had a great impact on his life and meant he was a conscientious objector to the war. Doodson wrote to his friend Ernest Nightingale in August 1915 that "nothing will drive me into the army or even into association with it in any shape or form" and "my views are very decided on the question".[25] As a conscientious objector, but also referring to his deafness and scientific work, Doodson was brought in front of the local conscription tribunal in March 1916. After rough treatment he was eventually given "absolute exemption" on conscientious grounds with the condition that he must do work of 'national importance.'[26]

Doodson's search for more interesting work and a higher salary, to pay off his debt and to enable him to get married, continued.[27] When Karl Pearson at University College London advertised a post involving statistical work Doodson applied and received an encouraging reply. While Pearson's laboratory did war-related government work Doodson assumed this was medical work, which he could not object to, but he did not think he would be accepted because of his conscientious objections.[28] However,

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[23] Much later Doodson published a version of this method of smoothing numerical tables, see Arthur Thomas Doodson, "A Method for the Smoothing of Numerical Tables," *The Quarterly Journal of Mechanics and Applied Mathematics* 3, no. 2 (1950).

[24] Proudman, "Arthur Thomas Doodson, 1890-1968." See also John Rylands University Library, "Frequently Asked Questions, for the Christian Brethren Collections," http://www.library.manchester.ac.uk/specialcollections/collections/brethren/faq/#d.en.111853.

[25] Doodson to Nightingale, 4th Aug 1915 Doodson papers

[26] Doodson to Galloway, 13th Apr; 4th Mar 1916, added notes dated 1954; 23rd Mar 1916, Doodson papers. In a 1954 note he explains that the tribunal refused to deal with him as a conscientious objector because of the subsidiary grounds and that he was treated roughly and called names. He writes that his case was mentioned twice in parliament. For example he was called "a disgusting mass of shivering fat" by one member of the Shaw tribunal and this was referred to in Parliament as an example of poor treatment of conscientious objectors in the Tribunals, see Hansard, "Army Estimates, 1916-1917", HC Deb 16 March 1916, vol 80, c2435, http://hansard.millbanksystems.com/commons/1916/mar/16/army-estimates-1916-17#column_2435. Accessed 18th Jan 2010.

[27] Doodson to Galloway, 9th Dec 1915, Doodson papers

[28] Doodson to Galloway, 1st Jun 1916, Doodson papers

Doodson was offered the post, the tribunal agreed for him to go[29] and he began work at the Draper's Biometric Laboratory in late September.[30]

Initially he liked the work, and the ways in which he worked was praised as accurate by Pearson, as Doodson gave the results of a particular calculation to eight figure accuracy where others had given six figures using the interpolation method he had developed when working on the Diffraction Problem with Proudman.[31] However, in December Pearson told him that the laboratory was likely to be wholly turned over to defensive war work, such as calculations for anti-aircraft or anti-submarine defence, and if he did not accept this work he would have to leave his post.[32] The war-related work gave Doodson the feeling that his "hands were not clean but bloodstained" and according to his letters to Margaret he seriously considered refusing.[33] However, he would have had difficulties finding another post as a conscientious objector to do the sort of work he could and wanted to do – he seems to have become involved in the work at UCL quickly – and in the end decided to stay. While he in his letters claimed he was also doing the more Christian thing by honouring the promise he had had to give to Pearson before getting the post to do whatever work he was given, and claimed to take comfort in that the fellow Brethrens he consulted agreed with him that he must keep his promise, it seems likely that the prospect of unemployment was as great or a greater part of his decision.[34] Perhaps being away from family and old friends in London loosened his resolve as a relatively new recruit to the church.[35]

In 1917 Pearson's laboratory was turned over wholly to war work, doing calculations for the Anti-Aircraft Experimental Section (AAES) of the Munitions Inventions

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29 Pearson has been described as sympathetic to Quakers and their hardship, which may have had something to do with him taking the religious Doodson on. Theodore M. Porter, *Karl Pearson: The Scientific Life in a Statistical Age* (Oxford: Princeton University Press, 2004), 250.

30 While Magnello emphasises the differences between the work done in Pearson's different laboratories, the Galton Eugenics Laboratory and the Draper's Biometric Laboratory, with the latter focusing on statistical rather than eugenic work, Doodson did not differentiate between the two in his letters. The differences may have been blurred by the increase in warwork. Magnello, "The Non-Correlation of Biometrics and Eugenics: Rival Forms of Laboratory Work in Karl Pearson's Career at University College London, Parts 1 and 2."

31 Doodson to Galloway, summer and autumn 1916, especially 14th Nov 1916, Doodson papers

32 See also Magnello, "Karl Pearson and the Establishment of Mathematical Statistics."; David Alan Grier, *When Computers Were Human* (Oxford: Princeton University Press, 2005).

33 Doodson to Galloway, 24th Jan 1917, Doodson papers

34 Doodson to Galloway, December 1916 and January 1917, Doodson papers

35 He did however remain a member of the church for life, even when Margaret died young, something which Proudman described as limiting his social life, Proudman, "Arthur Thomas Doodson, 1890-1968."

Department.[36] The practices of calculations used gradually changed during this year. Many of these changes in practices were instigated by Doodson, sometimes in collaboration with colleagues, after disputes with Pearson. In February the staff was smoothing values in double entry tables produced as part of the ballistics work. Doodson had suggested using his method from the Diffraction Problem. This used the differences between values in the table to produce 'families' of curves, one for the vertical and one for the horizontal aspect of the table, and then smoothed the table by comparing the values in it to these curves. His method minimised differences between the values to be smoothed and a function derived from those values. However, Pearson suggested a slightly different method applied to only one direction of the table. Pearson found his own version of the method not worth the trouble and suggested abandoning Doodson's method, leading to a disagreement.

At this point Doodson described the war work in scathing terms:

> The work goes on in a silly fashion, curves are drawn with splines [aids for drawing curves consisting of a flexible strip of material that is fastened down] that have natural kinks in them and then read 'accurately' with a lens! No wonder the final results need smoothing![37]

As part of his spat with Pearson, who was unusually badtempered at this time according to Doodson's colleagues,[38] Doodson suggested to "graph not the function, but the function minus its first approximation". He claimed this would increase the accuracy "at least 10 times" based on his experience from the work on the Diffraction Problem.[39] Pearson did not agree with it, but A W Young, one of Doodson's colleagues, tried it and liked the method. Together the two developed this more numerical, less graphical method based on interpolation to calculate the tables for the war work, e.g. of coordinates, over the year. Only very gradually did Pearson come round to the method,

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[36] For more on the work of this Section, see June Barrow-Green, "Planes and Pacifism: Activities and Attitudes of British Mathematicians During WW1," Gresham College with British Society for the History of Mathematics, http://www.gresham.ac.uk/event.asp?PageId=45&EventId=616 ; Michael Pattison, "Scientists, Inventors and the Military in Britain, 1915-19: The Munitions Inventions Department," *Social Studies of Science* 13, no. 4 (1983); Meg Weston Smith, "E. A. Milne and the Creation of Air Defence: Some Letters from an Unprincipled Brigand, 1916-1919," *Notes and Records of the Royal Society of London* 44, no. 2 (1990).
[37] Doodson to Galloway, 14th Mar 1917, Doodson papers
[38] Pearson was known to be difficult to work with at the best of times, see Porter, *Karl Pearson: The Scientific Life in a Statistical Age*, ch 9.
[39] Doodson to Galloway, 14th Mar 1917, Doodson papers

with lots of arguments. However, towards the end of the year, especially after complaints in October 1917 regarding the accuracy of tables produced with the old method from the users of these at AAES, Doodson's method was used more and more.[40]

Doodson was gradually and informally promoted. Pearson would sometimes let Doodson take charge of the work, as in November 1917 when all the staff were working under his direction on developing a numerical interpolation method, but sometimes Pearson would insist that his own methods be used.[41] At this point Doodson claimed the changes he introduced made the work quicker and more "correct", i.e. accurate, partly as his method was numerical instead of graphical:

> We are exceedingly busy at College, just finishing a gun off by a new interpolation method I developed. There will [be] no necessity for any drawing work at all, and the method is a very good one, giving results correct to 0.1 foot easily where drawing was sometimes out by 10 or even 20 feet.[42]

While Pearson would sometimes still do the theory of a problem (i.e. turning it into solvable equations), he often left the planning out and organising of the work (i.e. dividing the solvable equations into smaller parts organised onto documents that could be used by the computers) to Doodson. This emphasises that Doodson's key skill was that of organising calculations, which included such things as being able to lay out the work clearly for the other computers and write neatly.[43] Doodson claimed his method to do the ballistics work was "much more accurate than the other, involves less work, is far more expeditious, [and] needs only a short table (I could get it on this sheet of paper) in place of a large volume of tables".[44] It was these practices of calculation, speeding up the work – he claimed by five times[45] – and making the calculations more "accurate" (e.g. closer to whatever was considered the 'true' value, however defined), that enabled him to slowly rise in the ranks at UCL.

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[40] Doodson to Galloway, spring to autumn 1917, Doodson papers
[41] Doodson to Galloway, 26th Nov 1917, Doodson papers
[42] Doodson to Galloway, 19th Nov 1917, Doodson papers
[43] Doodson to Galloway, 29th Nov 1917, Doodson papers
[44] Doodson to Galloway, 15th Oct 1917, Doodson papers
[45] Doodson to Galloway, 26th Nov 1917, Doodson papers

The development and usage of Doodson's methods and the practices attached to them continued in early 1918. In late January the Director of Ordnance at Woolwich paid a visit to UCL to find out about one of the methods Doodson had developed, the small arc method. The Director was "greatly interested" and Doodson had the method written up for him. Doodson wrote to Margaret that "[my] reputation is made as far as small arc methods is concerned". [46] However, Doodson was feeling less and less happy with Pearson, was concerned about his future prospects and still felt that the war work stained him: "This kind of work can never receive the blessing of God; of that I am certain".[47]

This state of affairs continued until late March, when Doodson was asked whether he would take over the managing of the AAES work as Pearson was stepping down. Initially he was minded to decline the post for conscientious reasons, as he would work directly for the Munitions Inventions Department, but in the end accepted the post.[48] The only indication why he decided to accept despite his conscientious objections is that he was "[r]econciled in measure to it ... as its principal service was in connection with the protection of London against Zeppelins".[49] There were also no better conditions elsewhere, even if he could have found another job. Again, the threat of unemployment, enjoyment of the work and perhaps distance from his fellow Brethren, combined with an awareness that this post would be a major step up in his career, appears to have overcome his conscientious objections against the work.

As Director of the Computing Branch of AAES Doodson was now "responsible for all computations and tables respecting anti-aircraft gunnery and a staff of about 15 computators".[50] The AAES work continued until the Armistice and beyond. Doodson continued to develop and distribute his methods, co-operating with Archibald Vivian Hill (the head of AAES), Ralph H Fowler and other mathematicians, many of whom were or became FRS and had close connections to Cambridge mathematics (often with Trinity College, Proudman's college). This gave Doodson connections to the

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[46] Doodson to Galloway, 22nd Jan 1918, Doodson papers
[47] Doodson to Galloway, 4th Feb 1918, Doodson papers
[48] Doodson to Galloway, 22nd Mar, 5th Apr, 1918, Doodson papers
[49] Doodson's Personal Information File, section 8, RSA
[50] Faculty of Science Minutes, 19th Feb 1923, S3025, LUA. Compare Grier, *When Computers Were Human*, 133.

Cambridge-focused personal networks in mathematics that Proudman had entered through his studies.

After the Armistice Doodson's team was eventually moved to Woolwich. Though the team was offered to continue working most left the military soon after the war. Proudman offered his former student Doodson the post as Secretary at TI in early 1919, which Doodson quickly accepted. He started work at TI and married Margaret in April. He refused to be put forward for an honour, which many of his mathematician colleagues received, as it was for war work. He was however keen to make sure that he and his team were clearly referenced and quoted in publications such as the *Textbook of Ballistics* produced in 1919, to make sure they were given their due credit for the methods they had developed.[51] This and his decision to accept the promotion to direct the Computing Branch of the AAES illustrates the tensions between Doodson's conscientious objections and his career building strategies. It is clear that Doodson had strong conscientious objections but he also saw that he and his colleagues needed recognition for their work to further their careers.

Before starting at TI Doodson had no experience of tidal work,[52] but during his early career he had acquired a range of skills including in precision measurement and precision calculation, and above all skills in organising complex calculations. These skills had enabled him to rise in the ranks during the war and later this chapter will look at how he used these on a particular computational and mathematical problem – that of tidal predictions – but first other workers at TI and some of their tools will be introduced. Both the workers and their tools were important for TI's attempts to increase the accuracy of tidal predictions through new practices of calculation.

3.2 TI'S COMPUTERS, CALCULATING MACHINES AND WORKING PRACTICES

At TI Doodson was from very early on assisted by assistants called 'computers'. These computers were usually female and school leavers, though the first computer was Miss A L Cooper who unusually for TI's computers had a B.Sc. She had worked for

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51 Doodson to Galloway, summer 1918 to February 1919 and 1954 notes, Doodson papers

52 David Edgar Cartwright, "The Historical Development of Tidal Science, and the Liverpool Tidal Institute" (paper presented at the conference Oceanography, the past: Third International Congress on the History of Oceanography, Woods Hole, Massachusetts, USA, 1980).

Doodson for a while in London during the war.[53] In 1921 a second computer, Miss S K Lowry, was added. The staff then remained the same until Miss A Ainsworth was added in 1926 and Miss D Dood in 1927, which correlates with a rise in tidal predictions done by TI during this period which will be discussed in the next chapter. None of the three later computers had degrees.[54] Later the number of computers gradually increased, reaching 13 women in 1959.[55]

TI's computers left behind sheets after sheets of numbers, graphs and other calculations – a major part of the Bidston archive – but there were obvious and clear status and gender divisions at TI, separating out the researchers from the computers.[56] Joyce Scoffield has described her duties as a junior 'computer' in the early 1960s (i.e. just after the end of the period covered by the thesis) as "to observe the weather, to operate the tidal prediction machines, to fire the one-o'clock gun, to do 'differencing' [mathematical smoothing work checking tidal predictions] and to make coffee for everyone and prepare lunch for the male staff".[57] In other words, the computers then did a mix of scientific work, general administrative duties and housekeeping. Of note is Scoffield's comment that the 'differencing' methods used to smooth tidal predictions were based on work Doodson had done during the war.[58] She also described some of the processes and practices involved in doing predictions: setting up the tidal predictor, running it while taking down the results, checking these results using 'differencing' (which involved calculations, checked by a more senior computer titled 'smoother' who then graphed the figures), writing up and photographing of the predictions. All in all she estimates that predicting the tides for one port took four days for several people, in total 30 people hours.[59] While there will have been changes to the routines over the years, especially in regards to the copying of the final results, her work will have been similar to that done by the female computers in the late 1920s as mechanical tidal predictors were still used in the 1960s.

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[53] See e.g. Doodson to Galloway, 1st Jul 1918, Doodson papers

[54] Liverpool Tidal Institute, *Tidal Institute: First Annual Report, 1920*; Liverpool Tidal Institute, *Tidal Institute: Second Annual Report, 1921*; Liverpool Tidal Institute, *Tidal Institute: Seventh Annual Report, 1926* (Liverpool: University of Liverpool, 1926); Liverpool Tidal Institute, *Tidal Institute: Eighth Annual Report, 1927* (Liverpool: University of Liverpool, 1927).

[55] LOTI, *Annual Report 1959* (Liverpool: C. Tinling and Co. Ltd., 1959).

[56] Compare Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (London: University of Chicago Press, 1994), ch 8.

[57] Scoffield, *Bidston Observatory*.

[58] Ibid., 275.

[59] Ibid., 225-228.

The computers and Doodson used various calculating aids from the start and TI acquired its first tidal predictor in 1924, with another added in 1929. Apart from such specialised calculating aids TI used general purpose calculating machines. By 1935 TI had five: one second hand Tate Arithmometer (bought in 1919), a second hand Muldivo (bought in 1921), two new Comptometers (one bought in 1921 and another one in 1931) and a new electrical Monroe calculating machine (bought in 1925). In 1935 Proudman argued that TI needed to buy a new calculating machine on efficiency grounds, claiming that the old second-hand ones were inefficient as they were liable to error and of "primitive types", and that "[a]n operator using one of them cannot do nearly so much work in a given time as he could if he were using a more efficient machine".[60] In response to Proudman's request a "very satisfactory" Mercedes Euklid No. 38 calculating machine was purchased.[61] This emphasises both the importance given to calculating machines by TI and also how Proudman linked error-prone and slow machines to inefficiency, i.e. lower output of work. He argued that a more accurate (less error-prone) and fast machine equalled more efficient work, which in turn meant the worker could do more work, so fewer workers needed to be employed. This argument clearly worked on TI's governing committee, as they gave funding for the purchase of machines.

There is some debate in the secondary literature regarding the extent scientists employed mechanical calculators in the early 1920s. According to Mary Croarken calculating machines, like the Brunsviga, were not commonly used by scientists until at least the middle of the 1920s, which would mean TI was an early adopter of such machines.[62] On the other hand, according to Warwick scientists were using mechanical calculators widely by the turn of the century and Paul Hughes has identified the use of arithmometers by British tidal researchers in the nineteenth century.[63] However, at the time of TI's establishment the tidal prediction company Messrs. Roberts & Son did not

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[60] Proudman to Roberts, 3rd Oct 1935, D/BO 3/3/1, MMM – North Street. Note how Proudman genders the operator as male, despite most of TI's computers being female.
[61] Doodson to Secretary, 22nd Oct 1935, D/BO 3/3/1, MMM - North Street. TI continued to purchase different calculating machines throughout the period covered by this thesis, e.g. a comptometer in 1948 (Doodson to Mountfield, 30th Jul 1948, D/BO 3/3/1, MMM - North Street).
[62] Croarken, *Early Scientific Computing in Britain*, ch 1&2.
[63] Warwick, "The Laboratory of Theory."; Hughes, "A Study in the Development of Primitive and Modern Tide Tables", ch 10.

use them.[64] Pearson has been identified as an early adopter of calculating machines, by both Warwick and Croarken, and his students clearly took after him regarding the use of such machines.[65] Like another of Pearson's 'disciples', LJ Comrie (1893-1950), who introduced mechanized computation at the Nautical Almanac Office, Doodson seems to have re-introduced mechanized calculators into English tidal science.[66] Their adoption of such machines were also part of a wider increase in the mechanization of calculation and information processing at this time, which by Jon Agar has been linked to the sort of modernisation and efficiency 'drive' Alfred Booth led during the First World War.[67] Proudman's argument that a computer with a more 'efficient' calculating machine could do more work is an example of this. The use of mechanical calculators was one of the ways in which Doodson organised TI's tidal computations in a way that increased their data processing capabilities.

The photograph in Figure 3.1 illustrates TI's working practices. The photo is likely to have been staged to show the range of work TI did, probably for some promotional literature or an article about them, and portrays TI as an efficient and orderly scientific computation office. Doodson and Dennis are posing, performing tasks deemed typical or representative of TI's work. Doodson is using a calculating machine at his desk, surrounded by writing material, including a ruler to construct documents, and a waste paper basket under his desk. The booklet on his table looks like a copy of one of TI's Annual Reports. The computer is using another ruler, taking measurements from a record, perhaps a tidal gauge record, which is being held down by a heavy stick to manage the roll of paper. In the foreground another weight holds a large chart of what appears to be the Irish Sea.[68] In the background another desk has books on it and more papers, which can be seen as symbolising TI's more analytical and theoretical work as opposed to the measurements and calculations being done by Doodson and the computer. A university gown is hanging in the background, signalling to the viewer that this setting is an academic as opposed to a commercial or military computing operation.

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64 Proudman, "Report on Harmonic Analysis," 340. "Methods, staff, etc.", part of Minute from Warburg to Hydrographer, 28 Sep 1921, in HYD 587/1921, within H 4434.23, UKHO

65 Croarken, *Early Scientific Computing in Britain*, ch 1&2; Warwick, "The Laboratory of Theory."

66 For more on Pearson and 'disciples' see Porter, *Karl Pearson: The Scientific Life in a Statistical Age*.

67 For example in A Booth's work for the war time committee. Agar, *The Government Machine*, 177.

68 The larger scale original shows the outlines of the land around the Irish Sea rather clearer but it is impossible to make out what was on the sea area of the chart.

Figure 3.1: Doodson and one of the computers, Mrs Dennis, in TI's room at the University campus in the Holt Physics Laboratory[69]

Judging from this picture, the image TI wanted to portray of their work was that of organised calculations, with one task per table and with different people specialising on specific tasks. While the chart is displayed to the camera hanging artfully off the shelf, even Doodson's somewhat dishevelled collection of papers can be interpreted as that he is writing on a number of documents simultaneously, and thus operating in an efficient and productive manner (or the papers may just have been disorganised by the photographer to soften the image and lead the eye towards the supposed result of the

[69] Permission to reproduce in unpublished material kindly given by the copyright-holder National Oceanographic Centre, Liverpool. The computer and the location has been identified by staff at NOC Liverpool, but there is no further information as to its context. I thank J Eric Jones and the NOC Liverpool library staff for providing me with a copy of the picture and allowing me to use it. As Mrs Dennis started work at TI in 1929, the picture is likely to be from the late 1920s or the 1930s given the age of Doodson and Dennis. The empty bookshelves in the background suggests it may have been taken soon after most of TI and its possessions moved to Bidston Observatory in 1929, so perhaps the picture was taken to promote the merger of TI and the Observatory (which will be discussed in the next chapter).

posed work process: the chart). Everything is thus well organised, seemingly staged to portray order and productivity. TI is thus displayed as an efficient producer of scientific calculations, which would be a suitable image to put across to potential funders or purchasers of tidal predictions.

Another issue that can be noted in the picture is the physicality of mathematical and computational work involving documents. If Dennis worked in the position in the picture regularly she probably developed backache, while Doodson's fingers or papers would get inkstains from the fountain pen lying on the table. In addition, of note is the physical materiality of documents: weights are needed to keep them in order and they need to be organised in particular ways to be productive, as in the case of Doodson's papers (whether they were organised as an example of mathematical practice or by the photographer to draw the eye) or the documents surrounding the books on the back table. While flat surfaces and documents are no doubt easier to work with than 'raw nature', as Bruno Latour has argued, documents need material work too – the comfortable office Latour refers to would soon give Dennis a bad back and the roll of paper would escape her control without the weight on it.[70] Adding this materiality to Latour's notion of chains of documents makes it stronger, by showing one aspect of how such chains are constructed.

This photograph provides a picture of how TI worked and its setting, and literally makes some of their computational work and workers visible, albeit in a very posed manner. It well summarises this section by emphasising the importance to TI of human computers, mechanical calculating machines and documents. Such calculating machines were linked to efficiency by Proudman, and TI was part of a group of organisations, often with personal links to Pearson, who were keen to use calculating machines. While the picture shows little of the non-mathematical work at TI – such as how the computers made coffee or the work involved in archiving documents – it emphasises the physicality of mathematical work. In addition it tells us something about how TI wished to be portrayed: as an academic, efficient and productive computational organisation that achieved results. However, it does not explain the details of how TI used practices of calculation to justify their work and funding by attempting to increase the accuracy of tidal predictions. This will be discussed next.

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[70] Latour, *Pandora's Hope*, 38, 53.

3.3 ACCURATE TIDAL PREDICTIONS AND PRACTICES OF CALCULATION

3.3.1 LACK OF ACCURACY AS AN ARGUMENT TO JUSTIFY FUNDING

One of TI's early sources of funding was BAAS, who provided £150 funding in 1919 for computer assistance to a tidal committee chaired by Doodson.[71] In order to justify the funding given to the committee and used by TI, Proudman and Doodson needed to convince their audience at BAAS that they were doing necessary research, which boiled down to justifying research on tidal predictions. This in turn meant convincing their audience that Darwin's methods of tidal analysis and predictions were no longer accurate enough. To justify their funding and their research they used a rhetoric linking trust and accuracy familiar to historians of precision measurements.[72] TI's use of the term 'accuracy' was very like that by physicists arguing for funding for teaching laboratories in the 1860s to 1880s, analysed by Gooday. Like these physicists TI used a rhetoric of accuracy to argue for funding and support, but unlike them TI emphasised accurate precision calculations, not measurements.[73]

Many TI's researchers needed to justify believed that tidal predictions were seen to be 'good enough' by seamen. Judging from his earlier BAAS report with Horace Lamb, this had included Proudman, but while procuring TI's funding and developing its research programme Proudman had changed his views. He explicitly admitted this, claiming that after further work he had found existing predictions wanting.[74] However, his earlier view that the predictions were good enough for seamen was still widespread. The responses to a paper by Harold Warburg from Hydro's tidal branch, whose work on tidal predictions was mentioned in the previous chapter, given to the Royal Geographic Society in 1919, exemplifies the surprise some expressed at the need to improve harmonic predictions. During the discussion of Warburg's paper, a Mr EC Barton expressed "shock to learn that the harmonic methods have fallen into such disrepute". The chairman of the talk, the geologist Sir Aubrey Strahan, was surprised at the need for "extreme accuracy" in tidal predictions, and questioned the value of

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71 *Report of the Eighty-Eight Meeting of the British Association for the Advancement of Science, Cardiff - 1920, August 24-28*, xx.

72 See for example Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain."; Gooday, *The Morals of Measurement*; Schaffer, "Accurate Measurement Is an English Science."; Hunt, "The Ohm Is Where the Art Is."

73 Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain.", also Gooday, *The Morals of Measurement*, 63, note 77.

74 Proudman, "Report on Harmonic Analysis," 323.

working with errors in periodic tidal predictions when meteorological factors could not be predicted.[75] Another example of the view that meteorological effects were a greater problem for the accuracy of tidal predictions than the harmonic methods used in their analysis and prediction comes from *Nature* in 1921. In an exchange of letters Proudman argued against the views of AC Tennant, a previous employee of MDHB. The latter had suggested that meteorological effects should be the priority of tidal research. Proudman instead claimed harmonic predictions were not as good as normally thought and needed work before meteorological effects could be dealt with.[76]

In this exchange Proudman was not only arguing for tidal research generally and specifically at TI but also justifying TI's initial concentration on periodic tides rather than meteorological effects. However, Proudman only came to concentrate on analysis and prediction after TI was given funding from shipping men and advise from Hydro. In the exchange in *Nature* Proudman was thus justifying an initial research programme, influenced by TI's patronage to focus on periodic tides, and defending this from accusations from Tennant and others that research on this topic was not necessary. More generally, this was also part of Proudman and Doodson's institution building strategy. To justify TI's existence, its research programme and funding Proudman and Doodson argued that the accuracy of tidal predictions produced through existing methods was now insufficient for the needs of the users of such tidal predictions. They did this through a number of routes, such as Proudman's article in Nature and the exchange there with Tennant, but I will here concentrate on how they attempted to change the definition of the necessary accuracy in predictions through reports to their patron BAAS.

In reports officially from the Tidal Committee to BAAS Proudman and Doodson used a strong rhetoric linking trust to accuracy, arguing that current predictions were not trustworthy as they were inaccurate and that further work by scientists – i.e. themselves – was needed on predictions.[77] For example, in his 1920 report Proudman tried to dislodge the belief in existing harmonic analyses as bringer of truth and a high level of

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[75] Parry *et al.*, "The Admiralty Tide Tables and North Sea Tidal Predictions: Discussion," 329.
[76] Joseph Proudman, "Ocean Tides," *Nature* 107(1921); "The Scientific Investigation of the Ocean : Need for a New 'Challenger' Expedition," *Nature* 106, no. 2653 (1920); A. C. Tennant, "Ocean Tides " *Nature* 107(1921): 299-300; Joseph Proudman, "[Letters to Editor], Reply to Tennant," *Nature* 107(1921): 300.
[77] On trust in science see e.g. Theodore M. Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Chichester: Princeton University Press, 1995); Shapin, *A Social History of Truth*; Gooday, *The Morals of Measurement*.

accuracy by critically analysing Darwinian harmonic analysis. He questioned how close such harmonic analyses were to the observed astronomical tide: it "yet remains to be found to what precise extent the purely astronomical tide at any station may be expressed as a series of a reasonable number of harmonic constituents".[78] As another example, he compared "ten different determinations of what ought to be the same constant", finding that "the deviations from year to year [in the values found for the constant] are so great as to prohibit any reliance being placed on the results of the customary analysing processes applied to a single year's data".[79] Again, he was talking about deviations, or lack of accuracy, stopping the results of established analysing procedures from being reliable, or trustworthy.

At the same 1920 BAAS meeting at which Proudman criticised harmonic analysis, Doodson reported, equally critically, on the accuracy of existing harmonic prediction of tides. He first argued that current harmonic predictions of the periodic tides *ought* to be satisfactory, given existing theory and methods of calculations, despite meteorological effects causing difficulties in producing closeness between predicted and observed tidal levels.[80] He then claimed that it was in fact well known that predictions of the periodic tide were not 'accurate enough': "there are periodic or systematic differences in height and time of high water which are sufficiently serious in many cases to cause distrust … the distrust has led in many cases to the complete abandonment of the method of harmonic prediction".[81] Doodson explicitly linked inaccuracies, i.e. differences between observed and predicted tides, to distrust, and this in turn to the use of tidal predictions. As an example of the "degree of inaccuracy [which was] considered by authorities to be unsatisfactory" Doodson mentioned that the average error at Quebec for the time of the high water was 16 minutes and for low water 28 minutes despite the analysis being based on what he said was good data.[82] Doodson also listed other problems, for example that predictions by different organisations gave different results despite using the same harmonic constants: "Where … we may have two different predictions, each supposed to be authoritative, which differ occasionally by nearly a foot and on average by five inches in height, then one's confidence in the accuracy of prediction is badly

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78 Proudman, "Report on Harmonic Analysis," 323.
79 Ibid., 327-328. He here uses statistical error analysis, which Gooday argues was still relatively uncommon practice in Britain. Gooday, *The Morals of Measurement*, 75-76.
80 Doodson, "Report on Harmonic Prediction of Tides," 321.
81 Ibid.
82 Ibid.

shaken".[83] Doodson did not give any examples of any practical problems caused by these issues, just that they were causing the abandonment of the harmonic method. Improving accuracy was presented both as necessary and as a self-evident goal of scientific work to ensure the survival of Darwin's harmonic method.

By attempting to convince their listeners or readers that existing predictions should not be seen as good enough, Proudman and Doodson were justifying their research programme and funding. In addition, like so many other scientists' calls for work to increase accuracy, TI's calls for increasing the accuracy of tidal predictions should be seen in the industrial and military context set out in the previous chapter. Their arguments were also very similar to those of earlier physicists arguing for funding for laboratories, but instead arguing for funding for mathematical research.[84] Another difference is that TI emphasised very recent, post-war, achievements by themselves and not the achievements of the wider scientific community over a few decades. They were thus using the stereotypically modern argument of insisting on what Steven Shapin has called "the insufficiency of authoritative texts and upon the careful inspection of testimony".[85] For example, Proudman gave a case where earlier workers had taken a calculation shortcut which Proudman wanted the reader to doubt the validity of, pointing out that William Thomson had stated that the equilibrium principle should be used to allow for "the changing inclination of the moon's orbit to the earth's equator".[86] Proudman claimed that even when it was later found that "the inclination of the moon's orbit … was not according to the equilibrium principle" it was still "afterwards always treated as if it were so".[87] Tidal researchers had trusted Thomson, the authority, and followed his advice instead of taking account of the evidence. Proudman was arguing this was not enough; an argument which justified TI's choices of research topic.

When complaining about the use of the equilibrium principle Proudman was also arguing that the ways the calculations had been done previously did not take into account factors he deemed necessary and thus did not produce sufficiently accurate

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83 Ibid., 322.
84 Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain.", also Gooday, *The Morals of Measurement*, 63, note 77.
85 Shapin, *A Social History of Truth*, 201. This is 'stereotypically modern' as arguments of this sort have been made throughout history by many new generations of workers as part of their attempts to portray themselves as 'modern' and as improving on earlier generations.
86 Proudman, "Report on Harmonic Analysis," 336.
87 Ibid., 337.

results. The chapter now turns to how TI's researchers changed the practices of calculating tidal predictions and argued that their new methods produced more accurate results.

3.3.2 New practices of calculation: Residuals and abacs

TI's first step in justifying their research and funding had been to encourage distrust in existing methods. The next step for them was to show they could do better, which they did by linking the introduction of new practices of calculation to increased accuracy. Doodson had not only questioned the status of Darwinian harmonic analysis as bringer of truth but also claimed that the methods involved were inefficient and cumbersome, producing uncertain and scanty results.[88] Doodson's work was linked to his experience of running mathematical laboratories during the war as well as his development of methods of calculation. He now took these new practices of calculation and turned them on tides, and argued that the new practices led to an increase in accuracy.

Overall Doodson's method was one of intensive analysis of tidal records, by which he meant subtracting partial tides as they were determined and examining the successive residues.[89] As part of this work he also re-developed the tide-generating potential, i.e. reworked how the harmonic theory had been worked out mathematically by Darwin, using new tables for the motion of the moon calculated by E W Brown (1866-1938).[90] He linked work on the theory of harmonic prediction to introducing new practices of calculation, making this a case study of the use of such practices to improve the accuracy of the numbers generated by a specific theory, which was also adapted through this work.[91]

I will concentrate on two key aspects of Doodson's method, which differentiated it from that developed by Darwin: Doodson's emphasis on residuals and his use of new methods of calculation. Doodson's method of analysing tides first removed major, known constituents and then analysed the residues, or the number left after the known

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[88] Doodson, "To Assist Works on the Tides," 234.
[89] Ibid., 217.
[90] Arthur Thomas Doodson, "The Harmonic Development of the Tide-Generating Potential," *Proceedings of the Royal Society of London. Series A, Containing Papers of a Mathematical and Physical Character* 100, no. 704 (1921): 305-329, quote from 305.
[91] Warwick, "The Laboratory of Theory."

constituents had been subtracted, for further constituents. These further constituents were then taken out and the residual again analysed. He claimed certain errors were "proportional to the size of the constituent producing it". Concentrating on the smaller constituents found in the residuals after known major constituents had been removed could account for such errors, especially ones related to the presence of other constituents.[92] This linked to what Doodson thought another important difference between his and Darwin's methods was. While Darwin may not have agreed with this portrayal of his method, Doodson and Proudman suggested that Darwin's methods did not account for the possibility of unknown or perturbing constituents. This was because of Darwin's identification of a suggested list of constituents to analyse for, as discussed in the previous chapter, and the lack, as Doodson portrayed it, of inbuilt checks for whether there were other constituents that might influence the ones found through the process.[93] Doodson and Proudman claimed it was "general practice" to minimise residuals through the least square method in relation to one *only* of the large constituents and then ignore the rest of the residuals.[94] Proudman had identified this as a major source of error in analysis, leading to "imperfectly isolated" constituents.[95] In particular he considered this a problem with shallow water effects with speeds similar to that of other constituents, as the effect might not be separated fully from the other constituents.[96]

To deal with this problem Doodson adapted Darwin's method of analysis for use on residuals, claiming that working with residuals instead of observations gave "greater freedom possible in the details of analysis".[97] To analyse for a particular constituent Darwin had taken values at a given hour for each lunar day and then averaged these values from thirty consecutive days. He then used the 12 averages from one year to calculate constants A and B, which were used to estimate the height of the constituent.[98] This had worked in the case he analysed as the periods in the constituent was either a

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[92] Doodson, "To Assist Works on the Tides," 236.
[93] Ibid., 220, 233-226.
[94] Proudman, "Report on Harmonic Analysis," 330, 332.
[95] Ibid; Doodson, "To Assist Works on the Tides," 220.
[96] Proudman, "Report on Harmonic Analysis," 333.
[97] Doodson, "To Assist Works on the Tides," 236.
[98] As Darwin explained the use of A and B: "Supposing *n* to be the speed of any tide in degrees per mean solar hour, and t to be mean solar time elapsing since 0^h of the first day; then the immediate result of the harmonic analysis is to obtain A and B, two heights (estimated in feet and tenths) such that the height of this tide at the time *t* is given by A cos *nt* + B sin *nt*." Darwin, "Report of a Committee for the Harmonic Analysis of Tidal Observations," 78.

year or half a year.[99] Doodson implied this did not necessarily work in all cases if the periods were different. He instead used sets of 10 days' data which were then further averaged to find the final value of A or B. However, instead of straightforward averaging using a pre-set interval of time, for example Darwin's 30, Doodson used asymptotic means to find the values of A and B. He constructed the asymptotic means graphically, by drawing curves of the sum of the values for A (or B) divided by the number of As (or Bs) and then drawing a smooth curve visually finding the tendency of the mean (the dotted curves in Figure 3.2). When working on the Diffraction Problem with Proudman and on ballistics calculations during the war Doodson emphasised methods that combined the use of numerical and graphical interpolation techniques, and used differences or residuals as an integral part of this work.[100] While asymptotic means are somewhat different from his earlier smoothing and interpolations technique, the graphs constructed for that again emphasise the (decreasing) differences between means and a subsidiary function, in this case the trend of the asymptotic mean.

[99] The meaning of 'day' and 'hour' is complicated in tidal science. Ideally these should be in 'special time', related to the time taken for the argument of the residuals to increase by 360 degrees if it was diurnal or 720 degrees if it was semidiurnal, but in another difference between the two methods Darwin had used mean solar hours and corrected for this, while Doodson generalised Darwin's method by using special time. Doodson, "To Assist Works on the Tides," 233-238.
[100] He later summarised his methods of interpolation and smoothing as follows: "A subsidiary function is built up of differences corresponding to those obtained from the table to be smoothed and is then compared with the tabulated function. The difference between the two is then smoothed graphically." Doodson, "A Method for the Smoothing of Numerical Tables," 217.

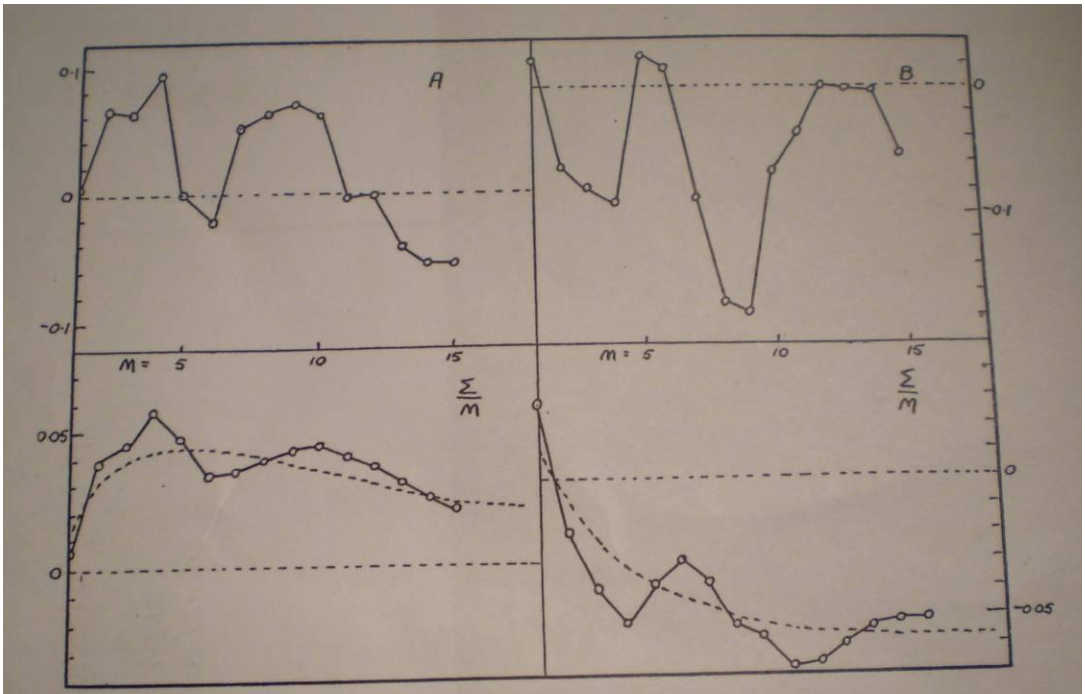

Figure 3.2: An example of Doodson's graphs to calculate the asymptotic means of the values of A and B for the constituent M2, as presented in the BAAS report. M is the number of contributory values of A or B respectively.[101]

In combination with his mathematical arguments, Doodson couched his work in a strongly rhetorical and moral, sometimes metaphysical, language justifying the funding TI had received. He linked his use of residuals and his practices of calculation to the trustworthiness of his methods. Twenty years earlier Darwin had claimed that the success of harmonic methods meant that the theory was true and that his method gave a full description of the behaviour of the seas.[102] Doodson questioned this, claiming that earlier methods assumed their own truth by only analysing for expected constituents: "the resulting numbers are taken on trust".[103] He claimed his own methods of calculation instead were designed to give indications of unknown constituents or disturbances, such as meteorological effects, as such unknown aspects would show up as residuals. This was not just about the calculations but also a moral and metaphysical claim. In terms of reality and truth, for Doodson it was all in the residuals: "Our sole

[101] Doodson, "To Assist Works on the Tides," 239.
[102] Darwin, *The Tides and Kindred Phenomena in the Solar System*, 225-226.
[103] Doodson, "To Assist Works on the Tides," 220.

index of reality lay in the residues. If, after taking out all the 'Darwinian constituents,' there were unmistakable signs of semi-diurnal constituents still remaining, then this would be regarded as sufficient proof of reality".[104] Reality was in the calculations and the data, not in how Darwin had worked out his theory or in Lord Kelvin's predictor machines.[105] To account for 'reality' Doodson argued that new practices of calculation accounting for residuals were necessary.

A more down to earth reason why Doodson saw his concentration on residuals as an important part of increasing the accuracy of tidal calculations was that he found new constituents this way. For example, Darwin had only analysed for six of the 29 constituents Doodson deemed necessary to predict the shallow water effects on tides at Liverpool.[106] Darwin had stated that he had only thought it was necessary to analyse for such effects for the main lunar and solar constituent, but Doodson ignored this statement and found many other constituents for shallow water effects using his analysis.[107]

In addition the idea of residuals was also important for his work on meteorological effects, and I will discuss it in a little more detail. The term 'residual' is still common in modern statistics where residuals can be calculated in a number of ways but indicate the distance between the observed data point and the equation line found by a particular statistical model.[108] Residuals are an integral part of least-squares analysis, a very common statistical model, as when such a regression equation is fitted, this finds values that minimises the sum of squared residuals.[109]

The concept of residual was still in development at the time of TI's establishment. Both Theodore Porter and Donald MacKenzie identify Francis Galton as the first who saw residuals as variation, arguing that earlier workers in the area had seen such residuals as measurement errors and not 'real' variation. They argue Galton's emphasis on variation

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104 Ibid.
105 Gooday has pointed out that machines that were connected to William Thomson were especially highly regarded and trusted, almost revered, in electrical measurement at the end of the nineteenth century. The same regard may have been extended to his tidal predictor. Gooday, *The Morals of Measurement*, 269.
106 Doodson, "To Assist Works on the Tides," 222.
107 Darwin, "Report of a Committee for the Harmonic Analysis of Tidal Observations," 74.
108 Andy Field, *Discovering Statistics Using SPSS*, 3rd ed. (London: SAGE Publications, 2009), 215.
109 Ronald R. Hocking, "Linear Regression," in *Encyclopedia of Statistical Sciences*, ed. Samuel Kotz*, et al.* (John Wiley & Sons, Inc., 2006). Accessed 13th July, http://mrw.interscience.wiley.com/emrw/9780471667193/ess/article/ess1468/current/pdf.

came from his interest in hereditary traits.[110] He shared this interest with Doodson's former employer Karl Pearson, who also saw residuals as real variation.[111] According to Desrosières, Pearson interpreted residuals in regression models as a combination of measurement errors, omitted variables or as variability of unknown origin. Desrosières argues that this analysis of residuals as made up of different parts was taken further by econometric modellers in the 1930s and 1940s. By these modellers the residual was further divided into errors in specification of the model (i.e. how the theory was translated into a statistical model including the choice of specific measurable variables to stand for the theoretical ones), irreducible variability and errors of measurement. Analysis of unexplained variability could be used to formulate further refinements of the econometric model.[112] What these econometric modellers did is also what Doodson did in his tidal work – analysing unexplained variability through residuals to refine the harmonic model of tidal prediction.

The second aspect of Doodson's work I will concentrate on was his methods of calculation. He argued that the new methods and practices of calculation he introduced were what had made his investigation possible, as he claimed they reduced errors and increased the speed of work:

> The investigations were made possible by the invention of a scheme for the numerical calculation and summation of the harmonic constituents; this scheme very greatly reduced the labour of calculation, and the results of summation of one set of constituents could be relied on to within about 0.01 foot.[113]

His report detailed the scheme of calculation, or abacs, Doodson had created to help calculate and sum the harmonic constituents. This involved constructing special purpose scales graduated in degrees on one side and "the appropriate cosine scale" on the other side, enabling the calculator to read off cosines and multiples of these without referring to trigonometric tables.[114] He cut such scales into sections as long in degrees as

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[110] Porter, *The Rise of Statistical Thinking, 1820-1900*, 293-296; MacKenzie, *Statistics in Britain*, 56-72.
[111] In his case his view on residuals was also linked this to his views on causality. For example, in "The Grammar of Science" he wrote that "[t]here is always in non-organic as in organic phenomena a residual variation" and thus phenomena are not causal but contingent. Karl Pearson, *The Grammar of Science* (New York: Cosimo, 1911, this edition 2007), 174. He emphasised contingency, which he saw as similar to correlation, over causation. Pearson, *The Grammar of Science*, 176.
[112] Desrosières, *The Politics of Large Numbers*, 305-307.
[113] Doodson, "To Assist Works on the Tides," 218.
[114] Ibid., 224.

the speed of the constituent being calculated and put these together side by side, see Figure 3.3. This meant it was possible to find the sums by drawing horizontal straight lines between the sections. In his description of this and other methods and schemes for calculation, he frequently gave practical details. For example in this case he stated that "[t]he best procedure is to use paper ruled in quarter-inch squares with the vertical lines half an inch apart, and with one half-inch to a degree".[115]

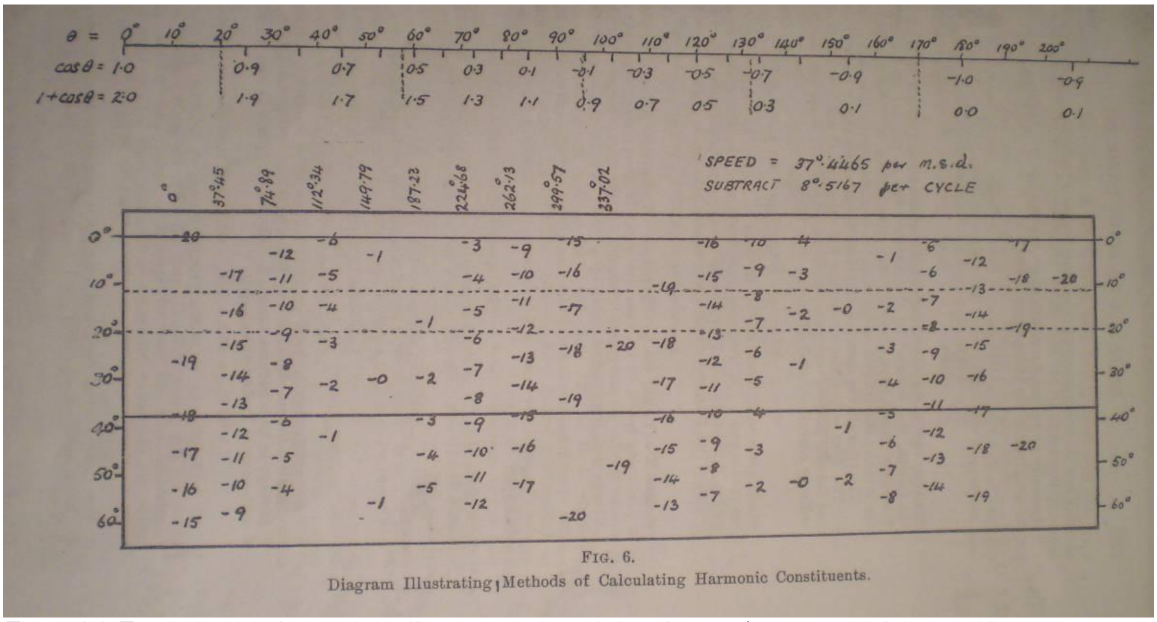

Figure 3.3: Example scale for reading off cosines and multiples of these. An example of the specific new technologies of calculation Doodson introduced to compute tidal constituents.[116]

Doodson repeatedly emphasised what he called the trustworthiness of his methods, discussing his error-checking procedures for calculations of predictions in detail and also claiming that his new methods reduced errors inherent in Darwinian harmonic analysis.[117] The same emphasis was found in his otherwise more formal and mathematical article in the *Proceedings of the Royal Society*, where he also redeveloped the tide potential. There too he discussed his practices of calculation, emphasising not just how his methods were fully numerical and harmonic throughout, but also for example how his new notational scheme made the work easier, how he introduced checks on the

115 Ibid.
116 Ibid., 229.
117 Ibid., 228-231.

calculations and how arguments could be grouped to save on writing and calculations.[118] He thus used new practices of calculation to work out the theory of harmonic analysis and predictions, as well as how predictions were to be calculated in practice. He also used these new practicess as rhetorical tools in his articles and presentations.

Doodson's work on the practices and technologies of calculation involved in tidal predictions meant he could manage a larger number of calculations than had been possible for Thomson, Darwin and other earlier tidal analysts, for example by using his abac to calculate and sum harmonic constituents.[119] This in turn made it possible for him to look for further constituents (e.g. 29 for Liverpool) and include more constants and to account for more variation (or residuals) in the data, both when he re-developed the tide potential and when he analysed residuals in tidal gauge data. This thus provides a case study of the processes Warwick has outlined, whereby demands for increased accuracy from scientists (expressed in Lamb and Proudman's earlier report to BAAS), industry and military combined with new practices of calculation.[120] These practices and technologies were used to rework the earlier version of the harmonic theory, using mathematical analysis techniques taught by Cambridge wranglers to Doodson during his studies at Liverpool University combined with computational analysis of tidal data, using techniques developed in mathematical laboratories in London during the war. Doodson linked calculations with theory, amending both to produce predictions which were closer to observations by including more of the variation in the data and which he could thus class as more accurate.

However, the processes of institution building was also very important in this. Doodson's work on improving the accuracy of tidal predictions was set within the context of justifying and applying for funding. How he and Proudman communicated their work was a political business full of rhetoric regarding trust and the morals of using particular practices of calculation, e.g. whether it was acceptable to rely on existing theory or if it was necessary to look at the data anew. This is similar to the debates regarding precision measurement and demands for funding of physics laboratory

118 Doodson, "The Harmonic Development of the Tide-Generating Potential."
119 Doodson, "To Assist Works on the Tides," 218.
120 Warwick, "The Laboratory of Theory."

discussed by Gooday,[121] with the difference that this was in another field, precision calculations, showing a wider applicability of his ideas. In their reports to BAAS Proudman and Doodson were justifying their own work by claiming that existing methods did not produce satisfactory results, and attempted to show that they could do better and thus deserved funding, supporting this argument by explaining in detail how their new practices of calculation worked. Doodson's rhetoric of accuracy and trust was in this way underwritten by new practices of calculation. Without such open display of these new practices, the claims for new, more accurate, calculations of periodic tides would have been much less plausible.

In the next chapter I will return to how TI were able to use this work on tidal predictions and practices of calculation as part of their institution building. All this work was however related to periodic tides. We now turn to TI's work on irregular meteorological effects.

3.4 METEOROLOGICAL EFFECTS: RESIDUALS AND STATISTICAL CORRELATIONS

As we saw in the previous chapter's discussion of the first annual report, investigations into meteorological effects on the tides were portrayed as a crucial aspect of increasing the general accuracy of tidal predictions in TI's First Annual Report. That the prediction of meteorological effects needed further work was also the view of many others, as discussed earlier. TI claimed that earlier work had defined the problem but only produced qualitative results, which were sometimes conflicting, whereas they aimed at producing quantitative results, reducing meteorological effects "to law".[122] This section describes TI's early attempts at providing a formula to numerically predict meteorological effects on a daily basis. Later chapters will discuss their work and practices in much more detail, but here I introduce two key aspects of their work which remained important at TI until at least the late 1950s: the definition of meteorological effects as residuals and the use of statistical methods in TI's attempts to forecast such

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[121] Gooday, *The Morals of Measurement*; Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain."
[122] Liverpool Tidal Institute, *Tidal Institute: Third Annual Report, 1922* (Liverpool: University of Liverpool, 1922), 6.

effects. Both of these were rooted in TI's earlier work on tidal predictions and further back to Doodson's training and early career – how TI constituted meteorological effects as objects for scientific study was historically contingent.

Statistical methods were introduced as soon as TI began work on meteorological effects, in 1920 when Doodson supervised a M.Sc. student, Frederick Williams Reece, writing a thesis on meteorological effects. Like TI itself this thesis was justified in terms of improving inaccurate tidal predictions for the benefit of shipping, in this case by attempting to produce a quantitative formula to correct for meteorological effects.[123] Much of Reece's thesis surveyed the earlier literature, concluding that most researchers had first calculated *mean* sea-level and *mean* barometric pressure over some fairly long period such as a month before trying to find relations between these two, which he claimed masked the immediate effect of local pressure on a specific tide.[124] The use of means was also one particular practice of calculation, which reduced the amount of data to be compared in a particular way. Reece's original research concentrated on correlating pressure and sea-level, as earlier scientists had done, but he used statistical methods instead of comparing means taken over long periods. Reece, on Doodson's suggestion, used correlation tables as in figure 3.4 to link each measurement of sea level to the corresponding barometric measurement, instead of comparing means. In other words, Reece and Doodson introduced statistical practices of calculation which had not previously been used on the problem of meteorological effects.

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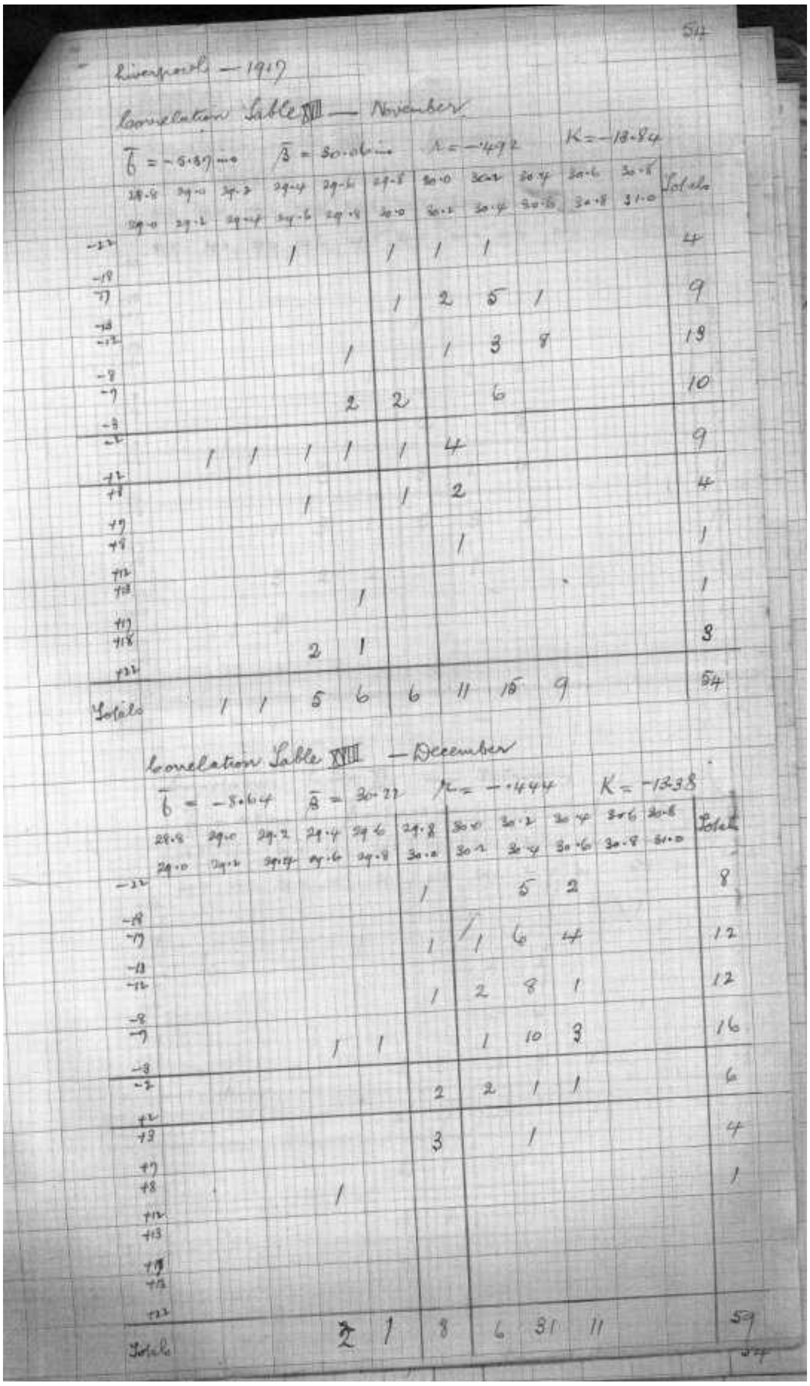
123 Frederick William Reece, "Variations in Mean Sea-Level" (M.Sc., University of Liverpool, 1921), 1-3.
124 Ibid., 19.



Liverpool — 1917

Correlation Table XVII — November

$\bar{b}$ = -5.37 ... $\bar{B}$ = 30.06 ... r = -.492 K = -18.84

	28.8–29.0	29.0–29.2	29.2–29.4	29.4–29.6	29.6–29.8	29.8–30.0	30.0–30.2	30.2–30.4	30.4–30.6	30.6–30.8	30.8–31.0	Totals
-22 / -18				1		1	1	1				4
-17 / -13						1	2	5	1			9
-12 / -8					1		1	3	8			13
-7 / -3					2	2		6				10
-2 / +2		1	1	1	1	1	4					9
+3 / +7				1		1	2					4
+8 / +12							1					1
+13 / +17					1							1
+18 / +22				2	1							3
Totals		1	1	5	6	6	11	15	9			54

Correlation Table XVIII — December

$\bar{b}$ = -8.64 $\bar{B}$ = 30.22 r = -.444 K = -13.38

	28.8–29.0	29.0–29.2	29.2–29.4	29.4–29.6	29.6–29.8	29.8–30.0	30.0–30.2	30.2–30.4	30.4–30.6	30.6–30.8	30.8–31.0	Totals
-22 / -18						1		5	2			8
-17 / -13						1	1	6	4			12
-12 / -8						1	2	8	1			12
-7 / -3				1	1		1	10	3			16
-2 / +2						2	2	1	1			6
+3 / +7						3		1				4
+8 / +12				1								1
+13 / +17												
+18 / +22												
Totals				2	1	8	6	31	11			59

Figure 3.4: Example correlation table produced by Reece for his Master thesis under Doodson's supervision, correlating barometric pressure and sea level in Liverpool in November and December 1917[125]

Doodson's work on meteorological effects depended on the analysis of residuals he had developed for periodic predictions. Formally, he defined the meteorological effect as

[125] Ibid., 54.

the ‘residual’ remaining after astronomic and other periodic constituents, such as shallow water effects, had been taken out off tidal gauge records. However, when working with meteorological effects, this residual was defined as the difference between periodic tidal predictions, ideally TI’s improved ones, and tidal gauge records. How TI constructed surges as residuals in practice will be discussed in chapter five. Here I want to concentrate on the concept itself and I have therefore created an explanatory graph of a particular storm surge using data fromTI’s archive, see figure 3.5. Defined as a residual a storm surge was the difference between the observed and predicted tide. For example, at 2am on the 17th of December 1917 the predicted tide at Southend was 17.5 feet but the observed tide was 19.5 feet. The meteorological effect, defined as the residual, was then 2 feet (19.5-17.5).

By defining meteorological effects as the residual of observed minus predicted sea level TI assumed it was possible to analyse tides and meteorological effects separately. In addition they structured the sea – or rather tidal gauge records – as calculable in a specific way, assuming that changes in the sea level/tidal gauge inscriptions could be straightforwardly divided into periodic tides and meteorological effects by simple and direct subtraction. The definition thus emphasised a dividing line between tides and meteorological effects in order to constitute surges as calculable objects.[126] However, their definition thus did not just emphasise *a* dividing line, but a particular dividing line of direct subtraction done ‘vertically’ at specific times.[127]

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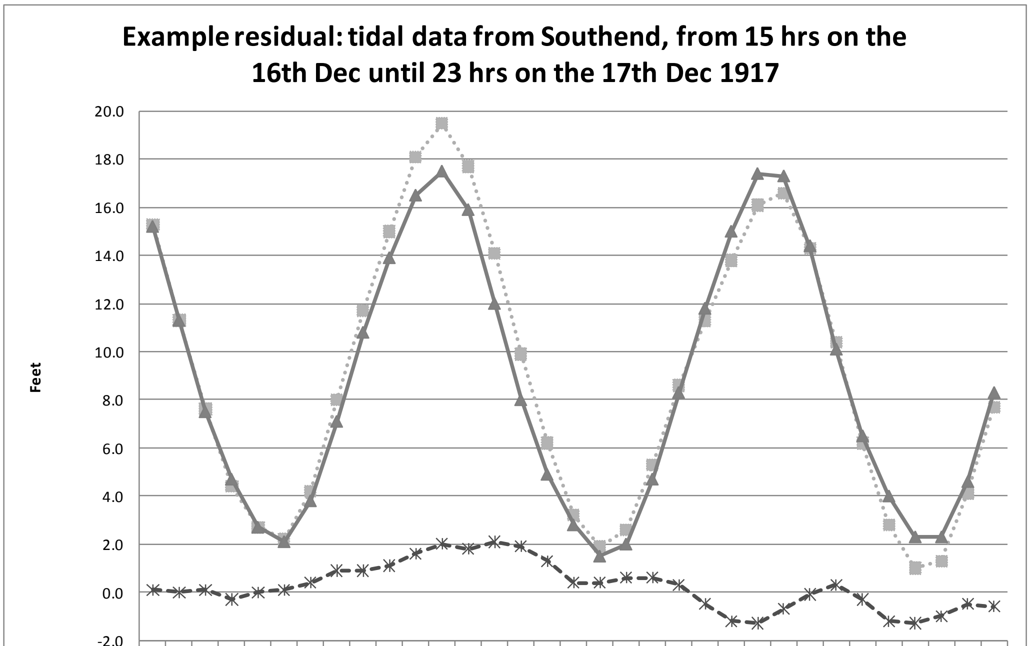
[126] Compare Lynch’s discussion of how dividing lines were emphasised in representations of biological specimens. Lynch, "The Externalized Retina."

[127] While not a historical example, the contemporary definition of storm surges as ‘skew surges’ instead of as Doodson’s ‘vertical’ residuals of observation minus prediction highlights that his structuring of his research object was one *particular* way of making the sea/tidal gauge record calculable. Skew surges are defined as the difference between the predicted high water level and the maximum observed sea level, whatever the timing of the two – the difference can be taken at an angle instead of vertically. For example, looking at the second high water, the maximum observed and predicted sea levels do not coincide. The maximum surge would be at 14hrs and -1.3 feet, whereas the skew surge is the maximum observed sea level, 16.6 feet at 3pm, minus the predicted high water level, 17.4 feet at 2pm, so 16.6-17.4 = -0.8. The skew surge is considerable smaller than the surge. See K. J. Horsburgh and C. Wilson, "Tide-Surge Interaction and Its Role in the Distribution of Surge Residuals in the North Sea," *J. Geophys. Res.* 112(2007): 6.



	15	16	17	18	19	20	21	22	23	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23
Obs	15.3	11.3	7.6	4.4	2.7	2.2	4.2	8.0	11.7	15.0	18.1	19.5	17.7	14.1	9.9	6.2	3.2	1.9	2.6	5.3	8.6	11.3	13.8	16.1	16.6	14.3	10.4	6.2	2.8	1.0	1.3	4.1	7.7
Pred	15.2	11.3	7.5	4.7	2.7	2.1	3.8	7.1	10.8	13.9	16.5	17.5	15.9	12.0	8.0	4.9	2.8	1.5	2.0	4.7	8.3	11.8	15.0	17.4	17.3	14.4	10.1	6.5	4.0	2.3	2.3	4.6	8.3
Obs-Pred	0.1	0.0	0.1	-0.3	0.0	0.1	0.4	0.9	0.9	1.1	1.6	2.0	1.8	2.1	1.9	1.3	0.4	0.4	0.6	0.6	0.3	-0.5	-1.2	-1.3	-0.7	-0.1	0.3	-0.3	-1.2	-1.3	-1.0	-0.5	-0.6

Figure 3.5: A storm surge defined as the residual, or observation minus prediction (obs-pred)[128]

Defining storm surges as residuals was part of Doodson's wider programme of making tidal predictions more accurate, i.e. closer to observed tides, by reducing the residuals in his harmonic model. The way he did this was by sub-dividing it into smaller harmonic constituents or other variables such as meteorological effects. While Doodson was well aware that meteorological effects were causally quite different from the other residuals he identified, such as shallow water effects or other new constituents, he discovered them in similar manner and called them all residuals. However, while Doodson linked meteorological effects to other residuals by using the same term, residual, they were predicted in different ways. His revised harmonic model for prediction of tides was not designed to include meteorological effects – to use statistical terms, the model had no variables for meteorological effects so could not be expected to explain the variation caused by them. Instead he needed to find another way of dealing with meteorological effects to increase the accuracy of tidal predictions.

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[128] Data from folder "Storm surges 1914-1928", Box 16, BA, as in figure 5.2. This is a purely illustrative graph. TI did not display surges in this format, partly because of the process they constructed them through (explained in chapter five) but also because displaying the surge on the same scale as the tidal level minimises the former. The graph does however explain the concept.

In 1922 much of TI's time was spent on meteorological effects and Doodson combined statistical methods with the idea of residuals to produce a forecasting formula for meteorological effects in Liverpool.[129] Doodson continued the work begun with Reece, developing a numerical formula to 'correct' the tidal predictions.[130] He did this by calculating the residuals for a number of events affecting Liverpool, and then correlating these with local atmospheric pressure and to pressure gradients in two directions over the Irish Sea, assuming this relationship could be represented as a linear function.[131] He used correlation tables and what he called the method of least squares to construct what is now called a multiple regression formula. Through this he first found "partial" correlations between two of the variables (the sea level residual, local barometric pressure and the two pressure gradients) at a time, and then a "compound correlation coefficient," representing the correlation between sea level and all of the meteorological variables in his formula.[132] For Liverpool the final correlation coefficient was 0.859, a quite high value. In other words, Doodson further used new statistical practices of calculation, such as correlation, he had already introduced to storm surge science with Reece, and combined these with his emphasis on residuals to create forecasting formulae. In an article on this work, Doodson did not blackbox the method of least squares/multiple regression, but instead described it and its rationale in detail, for example giving the formula defining standard deviation.[133] This shows that these statistical practices were new not only in storm surge science but also more broadly – Doodson did not expect other researchers to know it.

Doodson continued this work on forecasting formulae in 1923, alongside other statistically based investigations on meteorological effects, for example into 'time-relations' – essentially, does the correlation between the different pressure measurements and sea level improve if they are not all taken at the same time? In Liverpool this was the case, as the greatest correlation between local pressure and sea

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[129] Liverpool Tidal Institute, *Tidal Institute: Third Annual Report, 1922*, 5.
[130] He used a method developed by a Mr Jolly at the Ordnance Survey, which were interested in establishing mean sea level as a datum for their maps. Jolly later claimed Doodson was developing an almost identical method at the same time he was working on his method and that only luck got his own into print earlier, thus sharing the credit with Doodson. It was this simultaneously discovered method that Doodson developed further. The Earl of Ronaldshay *et al.*, "The Tides and the Work of the Tidal Institute, Liverpool: Discussion," *The Geographical Journal* 63, no. 2 (1924): 146.
[131] Liverpool Tidal Institute, *Tidal Institute: Third Annual Report, 1922*, 6-7.
[132] Doodson, "Meteorological Perturbations of Sea-Level and Tides," 134.
[133] Ibid.: 130-133.

level was found if the sea level was taken three hours *earlier* than the pressure, while different time-relations existed between pressure gradients in various different locations and sea level in Liverpool.[134] These investigations were aimed at making the forecasting formula more 'accurate', which was what TI's funders were interested in.[135] I will return to TI's use of this formula both in funding applications and for predictions in the next chapter.

Defining meteorological effects as residuals in the way Doodson did was new, and in different contexts others used other definitions. For example, in 1926 HA Marmer (Assistant Chief, Division of Tides and Currents, US Coast and Geodetic Survey) discussed the effect of winds on tides, but compared the observed tide during one particular storm with the curve of mean tide, not the predicted tide for those particular dates. Again in 1932 Marmer compared observed tidal curves over five days but did not explicitly compare these with predicted tides.[136] In both cases Marmer was more interested in the qualitative shape of the tidal curve and how this was changed by the effect of the wind. He was not after quantitative predictions the way TI were. In some work done for a short-lived BAAS committee in 1896, WH Wheeler had similarly compared storm surge events to mean water levels.[137]

Doodson's definition of storm surges as residuals was a contingent choice linked to his wider project of improving the accuracy of tides and also his research methods and practices of calculation, especially the idea of residuals, he used for this wider programme. However, while Doodson wanted to improve predictions of both periodic and meteorological residuals, different methods were used on different residuals: periodic residuals were included in Doodson's harmonic model for tidal predictions while the meteorological ones were analysed separately, using different models of predictions. The latter predictive models were based on statistical correlations, which was a new practice of calculation in storm surge science. This usage of statistics was

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134 Liverpool Tidal Institute, *Tidal Institute: Third Annual Report, 1922*, 7. Liverpool Tidal Institute, *Tidal Institute: Fourth Annual Report, 1923* (Liverpool: University of Liverpool, 1923), 3. See also Doodson, "Meteorological Perturbations of Sea-Level and Tides."

135 Liverpool Tidal Institute, *Tidal Institute: Fourth Annual Report, 1923*, 3.

136 H. A. Marmer, *The Tide* (London: D. Appleton and Company, 1926); H. A. Marmer, "Tides and Tidal Currents," in *Bulletin of the National Research Council: Physics of the Earth - V, Oceanography* (Washington D.C.: The National Research Council of the National Academy of Sciences, 1932).

137 W.H. Wheeler, "The Effect of Wind and Atmospheric Pressure on the Tides," in *Report of the Sixty-Sixth Meeting of the British Association for the Advancement of Science Held at Liverpool in September 1896* (London: John Murray, Albemarle Street, 1896).

linked to Doodson and his training and work experience in London, as well as to his earlier use of residuals as a particular practice of calculation to analyse tides. Using these practices Doodson was starting to construct *particular* chains of documents to constitute surges as calculable in particular ways, and – at least in theory – eventually predictable.[138]

3.5 Conclusion

This chapter has discussed how TI's researchers defined storm surges as residuals. This was in the context of a wider project of analysing tides for ever smaller residuals, in an attempt to try to improve the accuracy of periodic tidal predictions by incorporating further constituents revealed by these residuals. To do this, and to justify the funding they had been given by different actors, TI's researchers introduced new practices of calculation that increased the amount of calculations that could be done accurately in any given time, in order to be able to do more analysis and incorporate more constituents. These new practices of calculation, e.g. the use of calculator machines and new schemes of calculation, were introduced by Doodson, who had learnt them during academic, industrial and military work. His work to improve the accuracy of tidal predictions was linked not only to scientific research into the theory of tides, such as Proudman was doing, but also to demands for increased accuracy in tidal predictions from industry and the military, linked to an increase in the size of ships and to the First World War, e.g. Hydro's difficulties in predicting tides on the German coast.

Schemes to increase 'accuracy' have been studied repeatedly by historians of science, though they have predominantly concentrated on precision measurements.[139] This and the preceding chapter has instead provided a case study of efforts to increase the accuracy of precision calculations, linking changing practices of calculation to demands for increased accuracy from different actors. This case study has developed Warwick's work on the importance of the use of practices of calculation in work aimed at increasing the accuracy of numbers generated from theories, as well as the links

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[138] As will be discussed further in chapter five, my emphasis on the historical contingency of how scientific objects are constituted separates my work from Kalthoff and Lynch's. Lynch, "The Externalized Retina."; Kalthoff, "Practices of Calculation."

[139] E.g. Gooday, *The Morals of Measurement*; Wise, "Introduction."; Hunt, "The Ohm Is Where the Art Is."

between this and demands not only from science but also from industry and military.[140] The case study has also added the importance of institution building. Justifying funding was a crucial aspect of Proudman and Doodson's use of terms such as accuracy and trust, as well as their use and written displays of practices of calculation. In addition, there were some tensions between justifying their funding from academic actors, such as BAAS, and industrial actors, such as the Booths. What the latter wanted was not necessarily what the first expected, and vice versa. These tensions manifested themselves in the struggle Proudman and Doodson undertook to convince their BAAS audience that their work was necessary. The next chapter looks further at the development of TI's patronage structure.

140 Warwick, "The Laboratory of Theory."

Chapter 4 (1923-28), The use of new practices of calculation for prediction and patronage

The previous chapter discussed TI's work to try to increase the predictability of tides and meteorological effects. In this short chapter we turn to how TI used the products of their labour, both to gain further patronage and to do predictions. Through this, the chapter discusses how TI's triangular patronage structure developed, and thus how the Institute was linked to military, industrial and academic actors. These relationships were repeatedly renegotiated during TI's first decade and were also linked to the work TI was doing. This work was focused on providing an increasing number of predictions of periodic tides as well as of meteorological effects. In this period increasing the predictability of storm surges was about providing corrections to tidal predictions provided to TI's shipping industry patrons.

From 1923 onwards the Hydrographic Department (Hydro) became increasingly involved with TI by buying its tidal predictions. These purchases were TI's main connection to central government and the military and provided a source of steady income for the Institute. The role of tidal predictions in TI's work and budget until the 1950s will be discussed in this chapter, discussing the contributions of different types of clients, for example almanac publishers and Hydro.

4.1.1 A turning point for TI: 1923 and shipping patronage

In 1923, as the Booths' funding was coming to an end, Joseph Proudman and Captain FW Mace, MDHB's Marine Surveyor and a member of TI's governing committee, prepared a funding bid based on TI's early work. This was presented to LSOA in April by Charles Booth and Charles Livingston, both of whom were members of both LSOA and TI's governing committee. LSOA had been formed in 1858 to lobby MDHB but quickly became active nationally too, for example providing evidence to Parliamentary Committees. Its membership consisted of leading liner companies and has been described as "a 'Who's Who' of the great names of both the Liverpool and national shipping scene".[1] It had a clear free trade and free sea stance, arguing for equal access for all ships to all ports and against taxation and state interventions in shipping.[2] It thus represented the commercial interests of the shipping industry, especially large companies and deep sea liner companies; in other words shipping companies more likely to use large, deep ships, sensitive to the height of water.

TI's "appeal for subscription" to LSOA is also another example of how TI received funding by constructing the need and possibility of improving the accuracy of tidal predictions. The appeal was closely targeted, arguing that knowledge of tides was needed first and foremost for shipping, especially for large vessels such that members of LSOA were likely to own and manage. It claimed that "navigation in shallow water … is vitally dependent on accurate predictions on the depth of water", and that inaccurate predictions caused dangers, delays and expense for shipping. Present predictions were said to be "susceptible to much improvement" and the appeal claimed that "[i]n spite of the great increase in the size of ships little advancement in the accuracy of tidal prediction has been made for forty years". To improve this situation a permanent research institute was said to be necessary. Giving examples of TI's early work, such as how their work had led to the "discovery" of a particular shallow water effect that could affect Liverpool's tides by nine inches, TI were said to have made a very good start at being such an Institute, but it now needed support to put this work into practice. While the appeal also mentioned that tidal knowledge was needed to solve "problems of dock, harbour, river and coastal engineering" and was of "great scientific importance", linked with seismology, geodesy and astronomy, the emphasis was very much on improving the accuracy of tidal predictions for shipping purposes.[3]

In this appeal TI's supporters were arguing that with a grant from the shipping industry the Institute could put their earlier researches, described in the previous chapter, into practice. This, it was argued, would result in less danger for the ships LSOA's members owned and managed and thus less costs for their companies. The appeal continued and

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[1] Maritime Archives and Library, Information Sheet 51, Liverpool Steamship Owners Association, http://www.liverpoolmuseums.org.uk/maritime/archive/pdf/Business-Liverpool%20Steamship%20Owners%20Association,%20Source%20Guide%20no51.pdf, accessed 31st Oct 2009.

[2] Leslie Hughes Powell, *A Hundred Years On : History of the Liverpool Steam Ship Owners' Association, 1858-1958* (Liverpool: Liverpool Steam Ship Owners' Association, 1958).

[3] "Tidal Institute," Appeal attached to LSOA General Minutes, part 2: 1920-1964, (illegible date) April 1923, Vol 29, D/SS/2/4, MMM

widened the institution building strategy Proudman had begun to use perhaps with the Booths and definitely in TI's first annual report, further emphasising those aspects of TI's research programme of appeal to shipping men. The strategy worked and from 1923 until at least 1930 LSOA "unanimously" gave grants to TI. In 1923 the grant was £1200 after which it gradually decreased, for example in May 1928 LSOA gave them £250, as TI's other income increased.[4] In their appeal, TI's researchers said laid out their planned programme of work, to be done if they received a grant. This was generally aimed at improving tidal predictions. In particular, TI promised to provide forecasts of meteorological effects in Liverpool within a year, using the forecasting formula they had developed.[5]

4.1.2 MDHB AND THE GOVERNANCE OF TI

During the first decade of TI's existence its governance structure was in constant flux, and 1923 was one of the many turning points. Though there had been a strong contingent of shipping men, consisting of the Booths and representatives of MDHB, on it, academic representatives initially had numerical dominance of TI's governing committee.[6] This changed in 1923, when MDHB and the University formed a Joint Committee to govern TI and Liverpool Observatory.

Since the mid-nineteenth century MDHB had been charged by Parliament to maintain an Observatory. The Liverpool Observatory had been started in 1845 and was responsible for astronomical, seismographic and meteorological measurements in Liverpool as well as chronometer testing. It had moved to Bidston Hill, across the Mersey on the Wirral peninsula, in 1864. In December 1922 the University, which already had strong links to the Observatory,[7] approached MDHB to discuss "the possibility of closer co-operation between the Board and the University in the administration and activities of [the Observatory and TI], on the grounds both of

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[4] No conditions appear to have been attached to the grants. How the grants decreased can be seen in the table in the Appendix. LSOA General Minutes, part 2: 1920-1964, 17th July 1923, Vol 29; and LSOA General Minutes, part 2: 1920-1964, 21st May 1928, Vol 36, D/SS/2/4, MMM . Also TI's ledger, S2147, LUA

[5] "Tidal Institute," Appeal attached to LSOA General Minutes, part 2: 1920-1964, p. 3, (illegible date) April 1923, Vol 29, D/SS/2/4, MMM

[6] See chapter 2, note 101 and 102

[7] Scoffield, *Bidston Observatory*, 152.

usefulness and economy".[8] The University saw a dramatic increase in students following the war which led to pressure for rooms, teaching staff and funding,[9] and it wanted to save money. In return MDHB suggested that they and the University should set up a Joint Committee, governing both TI and the Observatory, and also offered the University use of the Observatory building free of rent, stating that the Board would continue to maintain the building "in good and sufficient repair".[10]

The University agreed, and entered into an agreement with MDHB. Under this agreement the two organisations kept their finances and staff separate, but were governed by a Joint Committee with the same members.[11] After the implementation of the Joint Committee of MDHB and the University in 1923 TI's earlier committee was slimmed down to ten members, five appointed by the University and five appointed by MDHB. As the University representatives included Charles Booth, whose affiliation was both that of Booth Steamship Co. and Vice-President of the University Council, the numerical advantage was now with shipping men. A new Admiralty representative, tidal super-intendent Harold Warburg, was also part of the University contingent, further shrinking the number of academics.[12]

With this agreement and with LSOA's funding, the importance of the shipping industry as a patron of TI, both financially and in its governance, was emphasised. As the appeal to LSOA made clear this industrial patronage was linked to TI's early work on predicting storm surges and periodic tides. TI received patronage from LSOA and MDHB because these organisations had been convinced (by TI's rhetoric as well as other factors) of the need for improved predictions, which TI were able to successfully argue they could provide on the basis of their earlier work. This thus linked their patronage to their introduction of new practices of calculation.

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[8] MDHB Finance Committee Minutes, 5th Dec 1922, MDHB MP/10/42, MMM
[9] Kelly, *For Advancement of Learning : The University of Liverpool, 1881-1981*, 188, 245.
[10] MDHB Finance Committee Minutes, 5th Dec 1922, MDHB MP/10/42, MMM
[11] Agreement text, attached to letter from Proudman to Warburg, 15th Jun 1923, H 4434/23, UKHO
[12] The University representatives were Charles Booth, Proudman, Professors Johnstone and Wilberforce, and Warburg. The Board representatives were Plummer from the Observatory, F W Mace (MDHB's marine surveyor), Charles Livingston and two new shipping men: H Concanon and H F Fernie. In 1927 Concanon was replaced by L A P Warner (General Manager and Secretary of the Board) and in 1928 Plummer died and was replaced by A B Cauty (of the White Star Line and a member of the Board). Liverpool Tidal Institute, *Tidal Institute: Fifth Annual Report, 1924* (Liverpool: University of Liverpool, 1924); Liverpool Tidal Institute, *Tidal Institute: Eighth Annual Report, 1927*; Liverpool Tidal Institute, *Tidal Institute: Ninth Annual Report, 1928* (Liverpool: University of Liverpool, 1928).

There were continued links between TI's research programme and its patronage. In line with the needs of TI's major patrons, the researchers at TI continued to do much work on improving tidal analysis and predictions. This work was formally presented to their patrons each year in TI's Annual Reports. For example, TI's Annual Report in 1927 discussed some work Arthur Doodson had done to include further constituents into his method of analysis of records. It also stated that "much has again been done on the analysis of observations and on allied questions of research, while theoretical dynamical investigations have been continued".[13] This quote emphasises how they combined work on analysis and predictions, and theoretical work. Both types of work, on predictions[14] and on tides in theoretical seas[15] resulted in articles in academic journals. Such publications ensured that TI's work, whether on predictions or Laplacian theory of tides, could be portrayed as scientific and academic. TI tried to balance the work and image of a scientific institute with producing an increasing number of predictions and working on the accuracy of these.

Another way TI used its work on tidal predictions was to actually produce forecasts and sell them. At this time tidal predictions were published not only by HM Nautical Almanac Office but also by commercial publishers, often small specialised ones like Glasgow-based Brown, Son & Ferguson which had begun publishing their Brown's Nautical Almanac in 1876.[16] Nautical almanacs contained tables (e.g., of tidal predictions and astronomical information) and other information used by sailors to navigate. The information was either compiled from a range of sources (including Hydro) or written and computed by staff employed directly by the publishers. In the early 1920s Holden's Almanack, a nautical almanac from Liverpool, commissioned TI to produce tidal predictions for Liverpool. TI also saw their production of these predictions in 1923 as a

[13] Liverpool Tidal Institute, *Tidal Institute: Eighth Annual Report, 1927*, 3.
[14] For example, Joseph Proudman and Arthur Thomas Doodson, "The Principal Constituent of the Tides of the North Sea," *Philosophical Transactions of the Royal Society of London. Series A* 224(1924); Arthur Thomas Doodson, "Perturbations of Harmonic Tidal Constants," *Proceedings of the Royal Society of London. Series A* 106, no. 739 (1924); Arthur Thomas Doodson, "Application of Numerical Methods of Integration to Tidal Dynamics," *Geophysical Journal International* 1, no. s10 (1928); Arthur Thomas Doodson, "The Analysis of Tidal Observations," *Philosophical Transactions of the Royal Society of London. Series A* 227(1928).
[15] Such as Joseph Proudman and Arthur Thomas Doodson, "On the Tides in an Ocean Bounded by Two Meridians on a Non-Rotating Earth," *Geophysical Journal International* 1(1927).
[16] Brown, Son and Ferguson Ltd, "Our history", http://www.skipper.co.uk/history.htm, last accessed 05/03/2011.

research project in itself.[17] Such developments and use of TI's earlier research on predictions led to further changes in 1923.

4.2.1 State and naval patronage

In 1923 the importance of tidal predictions for TI's research programme and patronage was enhanced further by Hydro becoming increasingly involved in TI's finances and governance, through an agreement to purchase predictions. This section will set that agreement within a context of gradually increasing co-operation between TI and Hydro, in part by proxy through other state actors.

4.2.2 TI's DSIR-grants

Starting in 1921 TI received grants from DSIR for three years. This section will discuss TI's application for these grants, emphasising the role of the Navy in this, which provides a window into Hydro's views of TI at this point, and the gradual growth in closeness between the two organisations. TI's application to DSIR in early summer 1920 used the by now familiar arguments that TI would fulfil scientific research needs in geodesy as well as assist the shipping industry by reducing dangers and costs to it, by decreasing the difference between tidal predictions and observations. Initial discussions within DSIR were supportive of the application, though it was adjusted downwards by TI to £600, as this was the amount they expected to raise from other sources.[18] DSIR's standard practice was to give a 'pound for pound' grant equal to money raised from industry or other sources.[19] When deciding on TI's application, DSIR consulted other departments, including the Admiralty. With Admiralty support for TI's application, DSIR prepared a request to the Treasury for pound for pound grant of £600.[20] The Treasury grudgingly agreed to this.[21] TI continued to receive DSIR grants until 1923/24.

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[17] Doodson to Warburg, 7th Jun 1923, H 4038/23, UKHO
[18] DSIR 36/13/4 "University of Liverpool, Tidal Institute, Grant-in-Aid 1920-21", NA
[19] Varcoe, *Organizing for Science in Britain*; Rose and Rose, *Science and Society*.
[20] DSIR 36/13/4 "University of Liverpool, Tidal Institute, Grant-in-Aid 1920-21", NA
[21] The Treasury, who had only been given information on the objectives of the institute, thought it had "some 'pure research' value for mathematicians", that the "Admiralty value it (as a charge on other people's vote)" and that it might be of interest to the Ministry of Transport, who according to a newspaper article might be interested in tidal power using a barrage over the Severn estuary. The Treasury

This withdrawal of state funding, with DSIR claiming it should be replaced by shipping industry funding, had been intended from the start, with DSIR arguing that their grant was aimed to help TI do work which would "secure to the Institute a full measure of support from the shipping firms and other bodies interested".[22] As discussed above, TI did find shipping support and thus became 'self-supporting', making it an unusual case among DSIR supported institutions.[23]

The most interesting aspect of DSIR's grant is the role of the Navy, as while the grant ostensibly was funding from the civil state, it was only with support from the Admiralty that DSIR gave TI a grant, so in as sense this was naval patronage by proxy. The view put forward by the Admiralty when consulted by DSIR stemmed from the Hydrographic Department, especially from Warburg. Those within the Admiralty who had been contacted by DSIR in turn consulted the Hydrographer, who then consulted Warburg.

Warburg's response to DSIR's consultation, which in the end supported TI's application, curiously also provides an example of the distance between Hydro and TI at this point. He began by claiming that since Hydro's Tidal Branch had been set up in 1912 there had been a dramatic increase in tidal work, and argued the best way to meet this demand would be to set up a tidal prediction section within Hydro, doing both "scientific and practical" work, but this would require investment and a trebling of the staff numbers. He then went on to list a number of institutions and individuals who did tidal work, not just Hydro and TI but also Manchester University, the National Physical Laboratory, the Ordnance Survey, Messrs. Roberts and Son, and private individuals. Warburg saw no "general reason" why TI should be picked out for preferential treatment from these institutions and in fact favoured money to be spent on co-ordination of departmental work on tides, perhaps through a "properly equipped Tidal Branch" or development of the work at NPL rather than to "subsidise non-official

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had not been given information as to why the Admiralty and DSIR felt the research was of value or information on the promised benefits to naval and merchant shipping. The Treasury's reaction to TI's application, doubting the value of its research, shows the importance to TI of framing its research as essential to shipping if it was to be appreciated. T 161/74 "Tidal Institute, Liverpool. Grant-in-aid", NA

22 Report of the Committee of the Privy Council for Scientific and Industrial Research for the Year 1923-24, Cmd. 2223 (London: HMSO, 1924), 34; Report of the Committee of the Privy Council for Scientific and Industrial Research for the Year 1920-21, Cmd. 1491 (London: HMSO, 1921), quote p68.

23 Others have claimed few DSIR supported institutions became self-supporting, see Varcoe, *Organizing for Science in Britain*; Rose and Rose, *Science and Society*, 40-44. For more on DSIR, see Hull, "War of Words."; Clarke, "Pure Science with a Practical Aim."

investigations".[24] There are two issues at stake here. Firstly, Hydro did not at this point see TI as an institution that generally deserved special treatment or had any particular relationship to Hydro or the state. Secondly, Warburg was also arguing for an increase in funding for his own branch of the Hydrographic Department.

Having said all that, Warburg then turned around and argued that the application from TI was a special case that should be given a grant, though with the condition that their research programme should reflect Hydro's priorities. The key behind this switch was that TI were already doing work suggested by Hydro, e.g. on tidal predictions for the BAAS reports and on assessing the accuracy of machine predictions as discussed in the previous two chapters. He emphasised that DSIR's grant should be conditional upon TI doing work "of practical value as agreed to by the Hydrographer".[25] While the Admiralty was very supportive of TI in its official letter to DSIR, saying the work was of "considerable importance" to them, they endorsed Warburg's recommendations for placing conditions on the research, asking TI to focus on specific areas such as "the investigation and elimination of errors in harmonic tidal predictions and predicting machines".[26] In return for state patronage from another department's budget Hydro wanted influence over TI's research programme.

Warburg's report reflected a gradual increase of Hydro interest and involvement in TI's work. While TI did not get money direct from Hydro and was not in 1920 seen as an *institution* that generally deserved state support, Warburg thought that their *work* should be supported as long as it was 'practical' and under the influence of themselves, so supported TI's application for a state grant for this work. However, while Hydro advised on TI's research programme following the DSIR grants, they were only marginally more involved in this than before the grant.[27] A greater increase in this – and also the abandonment of Warburg's dreams of a much enlarged tidal branch – came in 1923.

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[24] Minute by Warburg, 26th Aug 1920, HD 1472/20, UKHO
[25] Minute by Warburg, 26th Aug 1920, HD 1472/20, UKHO
[26] Admiralty to Secretary, 8th Oct 1920, DSIR 36/13/4 "University of Liverpool, Tidal Institute, Grant-in-Aid 1920-21", NA
[27] Documents in HD 1472/20, UKHO

4.2.3 Increased Hydro patronage: The commissioning of tidal predictions

As was discussed in chapter two, Hydro was concerned with the quality of the predictions they bought from Messrs. Roberts & Son.[28] By 1923 the contacts between TI's researchers and Hydro had strengthened to the extent that Warburg claimed that Proudman regularly wrote informally of developments to him.[29] TI had not been mentioned as a potential alternative source of predictions during the debate regarding Messrs. Roberts & Son in 1920, but by 1923 this had changed. In 1923 some of the Roberts predictions contained large errors for the last four months of 1924 for four ports, including London and Liverpool. Embarrassingly the predictions for London had been sent to the US and were in print there before the errors were identified, resulting in profuse apologies from Hydro to their US counterpart.[30] At the same time TI had compared their new predictions for Liverpool (done on commission for Holden's Almanack and as part of their own research) with Messrs. Roberts & Son's. Having identified the existence of an error they had immediately sent a letter to Hydro, enclosing tables of TI's and Roberts' Liverpool predictions for January and November so Hydro could compare them. TI also provided Hydro with examples of how they had made "exhaustive and satisfactory tests ... of the accuracy of our work".[31] TI both showed their own reliability and Messrs. Roberts & Son's unreliability in this letter, using a display of their practices of calculation to underwrite this message.

Following this incident, Warburg produced a report listing mistakes made by Messrs. Roberts & Son in recent years, discussing how their work contained clerical errors, only some of which could be detected through checks at Hydro. Warburg was not arguing that Messrs. Roberts & Son's method of predicting tides was poor, but that the firm made too many mistakes in their calculations and that they did not check their work sufficiently – their practices of calculation were deemed too unreliable rather than faulty. Warburg argued that these issues with their work meant that the question of

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28 Documents in file HYD 587/1921, within file H4434/23, UKHO
29 Report to Hydrographer, 26th Jun 1923, H 4038/23, UKHO
30 Minutes and correspondence in H 4038/23, UKHO
31 These checks were done by calculating the mean of the interval between the time of Upper Transit at Greenwich (an astronomical event) and that of the predicted high water times. The mean interval of TI's predictions was then compared with the mean intervals calculated using other, fast, methods. If the different means were close, as in the case of TI's calculations, this indicated that the calculations had been done correctly. Doodson to Warburg, 7th Jun 1923, H 4038/23, UKHO

finding an alternative source of predictions should be re-opened, having been closed in 1920. In addition, he argued that while in 1920 there had been only two options available, either using Messrs. Roberts & Son or bringing the calculation of tidal predictions in-house, there was now a third: commissioning predictions from TI.[32]

Proudman had informally let Warburg know that TI was considering purchasing a tidal predictor machine to start producing predictions. Warburg argued Hydro should purchase these predictions. To the Hydrographer he wrote that while such a move was likely to "end for all time any hope" of a tidal predicting branch at the Admiralty, he thought it was likely to lead to satisfactory predictions. He gave two reasons for this: first, "the two leading scientific authorities in the country on modern tidal work" would be involved, and, second, "the good name of the University being involved". That the University was involved was crucial as it meant that TI was neither state nor private, which mattered as Hydro had concerns about the possibility of sudden changes in price and availability when dealing with private businesses. On the other hand, Warburg argued that a university-based business would not face the "prejudice" he thought state-run commercial businesses met, so as a university-based business TI was more likely to attract customers than a state-based equivalent at Hydro would be.[33] Using TI was also likely to be cheaper for the department than setting up its own calculation office.[34] Having apparently given up hope of extending his own tidal branch, Warburg presented TI as the relatively cheap solution to concerns they had regarding the provision of predictions. Hydrographer Learmonth put the case for TI similarly: "all the advantages to be expected from the provision of an Admiralty machine will be obtained without the disadvantage of expenditure from Government funds".[35]

Later that year, after the formation of the Joint Committee of the University and MDHB, Proudman sent a formal question to Hydro whether they would consider purchasing predictions from TI.[36] Hydro straight away told TI they would buy their predictions. Indeed, they offered not only to give TI all the new work on analysis and predictions Hydro wanted done, but also to gradually transfer their existing out-sourced

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[32] Memorandum to Hydrographer, by Warburg, 26th Jun 1923, H 4038/23, UKHO
[33] He did not identify potential customers, but these included almanacs and port authorities both in Britain and elsewhere.
[34] Memorandum to Hydrographer, by Warburg, 26th Jun 1923, H 4038/23, UKHO
[35] Minute by Learmonth, Hydrographer, 16th Jul 1923, H 4434/23, UKHO
[36] Proudman to Hydrographer, 27th Jun 1923, H 4434/23, UKHO

work to TI, so that eventually TI would be doing all of Hydro's tidal contracts.[37] With this Hydro offered to become a major patron of TI, providing a steady income in return for good value predictions on a long-term basis. They however laid down a number of conditions for this, asking for a detailed technical description of the proposed machine, various financial reassurances,[38] and also a commitment of permanence. As Proudman thought they would, the Governing Committee "at once appreciate[d] the opportunity it gives for the future of the Institute" and the University and TI agreed to Hydro's various demands.[39] This included the University Council passing a resolution dictated by Hydro when it thought the University's first attempt at providing a commitment of permanence was not definite enough. This resolution prescribed that three years notice must be given (or three years worth of predictions offered at normal rates) by TI if it wanted to cease providing predictions.[40] Hydro had a number of longstanding relationships and contracts with outside organisations, for example with JD Potters, the agent selling Hydro's charts, and TI had now been added to these.[41]

As part of the discussions regarding the creation of a Joint Committee to govern TI and the Observatory and the informal discussions regarding their potential purchase of predictions, Hydro had been invited to send a representative. Initially they hesitated, but once the agreement regarding predictions was shaping up they decided that to have a representative on the governing committee would allow them a desirable level of influence. Such a representative from the tidal branch could "to some extent, supervise Admiralty tidal work being carried out at the Institute and ensure that charges were reasonable; personal discussion with the scientific authorities on tidal matters would also be of great advantage".[42] With this move Hydro became not only a major financial patron of TI but also directly involved in the governance of the Institute, giving Hydro influence over TI's operation and work, both formally and informally through "personal discussions" that could potentially influence the tidal work at both TI and Hydro.

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37 Learmonth (Hydrographer) to Director of TI, 6th Sep 1923, H 4434/23, UKHO

38 For example that Hydro would always be charged the lowest rate TI offered, e.g. if there were different rates charged to different clients.

39 Notes and correspondence in H 4434/23, UKHO. They also purchased a tidal predictor at a cost of around £1500, with funds donated mainly from different shipping men and companies (£1200) but also BAAS (£300), see Tidal Institute Ledger, S2147, LUA.

40 Learmonth (Hydrographer) to Registrar, 15th Nov 1923, H 4434/23, UKHO.

41 Roger O. Morris, *Charts and Surveys in Peace and War: The History of the Royal Navy's Hydrographic Service, 1919-1970* (London: HMSO, 1995).

42 Minute by Warburg, 9th Oct 1923, H 4434/23, UKHO

Warburg became Hydro's first representative in 1924, sitting as one of the five representatives selected by the University.[43]

In summary, 1923 was an important year for TI, with changes to its financial and governance structure increasing both the naval and industrial patronage of TI's work. The industrial patronage from LSOA and MDHB has already been discussed: TI used the work done on tidal predictions and meteorological effects, discussed in the previous chapter, as rhetorical resources in arguments for increased patronage. The situation with Hydro and Holden's Almanack was somewhat different, as with these TI used their revised version of the harmonic method to provide tidal predictions in return for payment. To enlist Hydro as a financial patron TI told them the Institute was prepared to start producing predictions. This combined with other factors, such as the gradually built up relationship between Warburg, Proudman and Doodson, lack of resources within Hydro and the performance of competitors, in leading to them being offered the agreement by Hydro. By giving TI a long-term contract to provide them with tidal predictions Hydro were giving TI a secure income source which was to last for the entire period covered by this thesis and beyond. TI used rhetoric, negotiations and network-building to put their new practices of calculating tidal predictions into various uses (both direct and rhetorical), which in turn generated income. The next section looks at how they attempted to use their work on meteorological effects to produce predictions of these.

4.3 TI'S FIRST FORECASTS OF METEOROLOGICAL EFFECTS

For TI, one important use of their work on meteorological effects was in the grant application to LSOA, where TI claimed that forecasts of meteorological effects would be introduced later in 1923 in the Liverpool area. What such forecasts were supposed to produce was a number to 'correct' the tidal predictions for Liverpool with. Adding this number should decrease the residual between the predicted and measured height of the tides when the actual tides were affected by meteorological effects. The possibility of producing such forecasts was presented as a key reason for why LSOA should fund

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[43] Notes and correspondence in H 4434/23, UKHO. See also University Council Minutes, 21st Jan 1924, S2222, LUA

TI.[44] Having been given funding from LSOA, TI then tried to use their formula to fulfill the promise they had made.

Two attempts were made by TI to create a forecasting service, one late in 1923 and another in 1926. On both occasions Doodson was operationalising the forecasting formula he had earlier developed for meteorological effects in Liverpool. This method correlated the tidal residuals to local atmospheric pressure and to pressure gradients in two directions, predicting the error of mean tide level for one day. Though he wanted to develop the method further Doodson claimed in 1922 that even as it stood, applying the correction produced tidal predictions that were noticeably closer to observations.[45] This statement he supported by producing a graph, figure 4.1, comparing observed and predicted errors, or residuals.

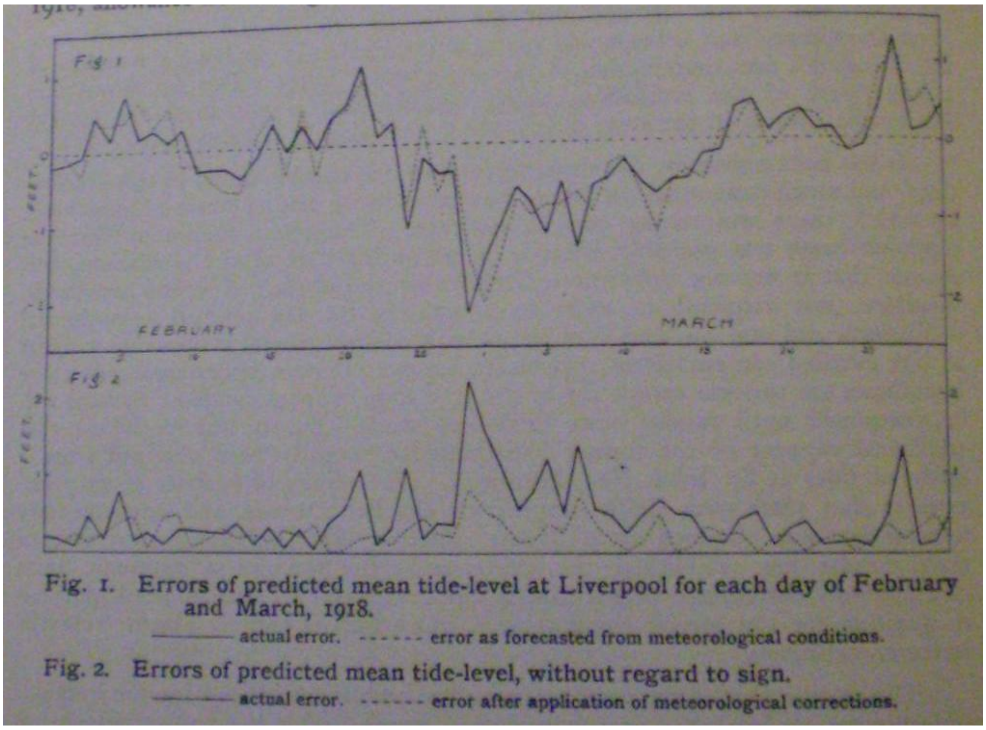

Figure 4.1: This diagram illustrates the results of TI's early forecasting formula for meteorological effects in Liverpool, comparing the observed "error", i.e. storm surge effect or residual, in the mean tide for that day with the residual forecast by TI's formula[46]

44 Appeal attached to LSOA General Minutes, part 2: 1920-1964, (illegible date) April 1923, Vol 29, D/SS/1/29
45 Liverpool, "Tidal Institute: Third Annual Report, 1922," 6-7.
46 Ibid., 6.

However, both forecasting attempts were deemed unsuccessful, the first one by TI and the second by MDHB, who claimed that uncorrected predictions were often closer to observations of high water than those corrected with TI's formulae.[47] TI accepted MDHB's judgement after a brief discussion, though Doodson argued the forecasts were "a step in the right direction", particularly for large surges.[48] In 1923 the blame for the unsuccessful predictions was laid on poor periodic tidal predictions which TI were not yet producing. They thought their own predictions would be closer to observations whatever the weather, which would mean the meteorological forecasts correcting the tidal ones would also be better.[49] Another problem raised early on was a lack of meteorological information, which provided an argument to purchase a radio-telegraphy set.[50] During the second attempt in 1926, blame was laid on the computer, though Doodson put it more politely, claiming he needed to keep a closer look on the calculations as they were being done.[51] Particularly after the first attempt Doodson defended TI's work by framing the forecasts as an experiment in network building, claiming that an important aspect had been to test the practicality of transmitting the necessary meteorological and tidal information and the Met Office's skill at predicting pressure and wind. According to Doodson this networking had worked well. [52]

However, establishing that forecasting network had taken much work. This work was that of setting up a system that would combine what they deemed the right bits of information in the right way at the right time to produce the sought after corrections in time for them to be distributed to sailors.[53] The information was not only tidal information but also meteorological, specifically wind speed and barometric pressure observations and predictions for three locations and different times.[54] Acquiring this information took correspondence and negotiations with the Met Office, as well as work by them to produce and transmit this information.[55] Before the second attempt in 1926 the creation of the forecasting network also took purchase of equipment (a radio-

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47 Captain Mace to Doodson, 2nd Dec 1926, Box 132, Bidston Archive

48 Doodson to Mace, 1st Nov 1923, Mace to Doodson, 2nd Dec 1926, and Doodson to Mace, 10th Jan 1927, Box 132, BA

49 Doodson to Director at the Meteorological Office, 8th Dec 1923, Box 120, BA

50 Liverpool Tidal Institute, *Tidal Institute: Fifth Annual Report, 1924*, 7.

51 Doodson to Mace, 3rd Dec 1926 and 10th Jan 1927, Box 132, BA

52 Doodson to Captain Mace, 1st Nov 1923, Box 132, Bidston Archive

53 Doodson to Mace, 1st Nov 1923, Box 132, BA

54 Doodson to the Director of the Meteorological Office, 4th Oct 1923, Box 120, Bidston Archive

55 Correspondence between Doodson and the Met Office (variously the Director or Superintendent Dines) in October, November and December 1923, Box 120, BA

telegraphy set installed at Bidston in 1926)[56] and a spatial re-arrangement of the forecasting network. In 1923 Doodson had had the meteorological information delivered to him at the University of Liverpool where the calculations of the correction had been made. For the second attempt the calculations were moved to the Liverpool Observatory at Bidston Hill. At this time the newly-acquired tidal predicting machine was already housed there and the Observatory had existing links with the Met Office as a provider of meteorological observations.[57] This however meant that Doodson could not always be present when the calculations were made, as he was based some distance away at the University campus, which he claimed caused problems during the second forecasting attempt.

There is no indication that the gradual decrease in LSOA's patronage to TI had anything to do with their failure to provide meteorological corrections that satisfied MDHB. In fact, it is more likely that it fell as TI simply asked for less and less money as other income went up.[58] On the other hand, the most directly involved of TI's patrons, MDHB, maintained and in fact increased their relationship with TI at this time, as we will see next. TI's failure to keep its promise of delivering meteorological corrections to tidal predictions appears to have had very little effect on its patronage. Instead, that they repeatedly tried to use their formula for the benefit of shipping in Liverpool appears to have been enough to satisfy their patrons. By now they were closely connected with their patrons. The governing committee had been sufficiently involved in the network building, for example assisting in the purchase of the radio-telegraphy set, to know the amount of work that had gone into it these forecasting attempts. The closeness of the personal networks and the involvment of TI's patrons in the forecasting attempts seems the best explanation why the failures of the forecasts had little impact.

4.4 Further MDHB patronage: The merger of the Observatory and TI

Here I continue to trace how TI's patronage structure was established, as this had not settled in 1923. The University and MDHB again renegotiated their relationship in

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[56] Liverpool Tidal Institute, *Tidal Institute: Seventh Annual Report, 1926*, 6.
[57] Liverpool Tidal Institute, *Tidal Institute: Fifth Annual Report, 1924*.
[58] See the Appendix.

1928, merging the Observatory and TI. This section looks at how this came to be. As after the merger TI's patronage and governance structure remained almost unchanged until 1960 it also explains how TI's governance and finance worked during most of the period covered by this thesis.

Debates favouring a complete merger of the Observatory and TI began soon after the introduction of the Joint Committee in 1923, when they were prompted by discussions to install the new tidal predictor as well as electricity and wireless at the Observatory for TI.[59] The merger did not go ahead as the elderly Director of the Observatory, William Plummer, had no pension and therefore could not retire.[60] However, co-operation increased and as we saw above TI started to make increasing use of the Observatory building, both to house the tidal predictor machine and also for their predictions of meteorological effects.[61]

Following the death of Plummer in spring 1928, members of the two Joint Committees slowly negotiated an agreement to merge the two institutions in early 1929. The newly merged institute was called Liverpool Observatory and Tidal Institute (LOTI) but many actors continued to refer to the tidal side of the operation as TI or the Tidal Institute, as will I. Though many details would change the basic financial and governance structure set up in 1929 remained until 1960. The new governing committee paid salaries, other running costs, such as electricity and maintenance of equipment, and fulfilled MDHB's legal obligations to provide the services of an Observatory to the port.[62] The University was on the governing committee and paid a small grant to TI, while MDHB contributed more substantially both financially and in kind, by maintaining the Observatory building it lent without charge to TI.[63] The new LOTI Committee was very similar to the committee that had previously governed TI, though with 12 members, with a continued dominance of shipping men.[64]

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[59] Minutes of the Marine Committee, 14th Jul 1924, p. 475, File M.P.13.18, MDHB Archive, MMM
[60] MDHB and the others involved were clearly not prepared to turf him out onto the streets without income. Notes by Warburg in H 835/27 and H 8761/23, UKHO
[61] E.g. LSOA General Minutes, part 2: 1920-1964, 17th July 1923, Vol 29; and LSOA General Minutes, part 2: 1920-1964, 21st May 1928, Vol 36, D/SS/2/4, MMM
[62] Liverpool Observatory Joint Committee, Minutes, 5th Nov 1928, D/BO 1/1/2, MMM – North Street,
[63] Scoffield, *Bidston Observatory*, 175.
[64] Minutes of the Finance Committee, 14th Nov, 1928, p 129, file 46, MDHB Archive, MMM. In 1929 the governing committee included three members of MDHB as well as the Board's chairman, its general manager, and its Marine Surveyor and Water Bailiff. The University was represented by Warburg, Charles Booth and four academics, including the University's Vice-Chancellor and three Professors (Proudman, J

This dominance of shipping men on the governing committee was the most contentious issue during the merger negotiations.[65] Negotiations of the agreement took about half a year, with the proposed governance structure at stake.[66] As the Board's duties regarding the Observatory were set down by an Act of Parliament MDHB's solicitors insisted that the chair of the committee must always be from MDHB and would have the deciding vote if the Committee was equally divided. This led to heated discussions which according to Warburg were about ensuring "that the local prestige of neither side will suffer".[67] MDHB and the University were negotiating about who should have more control and power over TI and its research programme. In the end MDHB, who was also providing most of the financial support, got their way, and the shipping industry was strongly represented with a deciding vote on TI's governing committee as well as in its income until 1960.

Why were the actors involved in TI keen enough on this merger to insist on it despite these disputes and worries over? In the merger agreement the University and MDHB claimed the reason for the merger of the Observatory and the Tidal Institute was that they were "desirous that the facilities which the Observatory and Tidal Institute afford for the advancement of knowledge and diffusion of science and learning may be extended and increased". In addition to these noble arguments the agreement would limit their financial liabilities, e.g. for MDHB by limiting its direct financial contribution to £1,500 per year.[68] While the Observatory was somewhat short of cash and unlikely to find new ways of raising income,[69] the Tidal Institute's income from tidal predictions was steadily increasing at this time and was expected to continue to do so, following the agreement with Hydro.

Johnstone of Oceanography and LR Wilberforce of Physics). LOTI, *Annual Report 1929* (Liverpool: University of Liverpool, 1929).

65 LOTI, *Annual Report 1930* (Liverpool: University of Liverpool, 1930), 2.

66 The main changes to the different written versions of the Agreement were procedural, for example the Marine Committee of MDHB insisted that the Joint Committee appoint as Secretary a Salaried Officer of MDHB as well as an Accountant, to be a Salaried Officer of the University, and also insisted that the new Joint Committee should include the Chairman of MDHB and the Vice-Chancellor of the University as ex-officio members. Liverpool Observatory Joint Committee, Minutes, 5th Nov 1928, D/BO 1/1/2; Minutes of Meeting of Joint Committee of MDHB and the University, 25th Jun 1928 and 30th Jul, D/BO 1/1/1; and Liverpool Observatory Joint Committee, Minutes, 25th Jun 1928, D/BO 1/1/2, all in MMM – North Street. Minutes from University of Liverpool Council Meeting, 24th Nov 1928, Council Minute Book 16, S2223, LUA. Minutes of the Marine Committee, 26th Nov 1928, p. 402, File M.P.13.19, MDHB Archive, MMM.

67 Minute by Warburg, 8th Nov 1928, H7452/28, UKHO.

68 Liverpool Observatory Joint Committee, Minutes, 5th Nov 1928, D/BO 1/1/2, MMM – North Street

69 Minutes of the Marine Committee, 22nd Oct 1923, p. 398, M.P.13.18, MDHB Archive, MMM

Proudman later argued that what TI gained from the merger was space, finance, prestige (by virtue of being associated with "a prominent building") and stability in terms of governance.[70] The spatial aspect of his argument is supported by him asking the Joint Committee for an additional room at the Observatory for use of TI's staff in late 1927, as the "housing accommodation of the Institute" was "inadequate". This is likely to have been due to the increased number of predictions TI were doing, employing more staff.[71] The Vice-Chancellor argued for the merger to the Hydrographer, claiming it would allow all of TI's practical work to be done on one site and also that the Observatory's meteorological work would help work on meteorological effects.[72] A merger would enable Doodson to take over Plummer's position as Director of the Observatory, thus giving TI access to the substantial financial contribution MDHB gave to the Observatory. This would enable TI to take on both an astronomically focused assistant, to do the work the Observatory had to do to fulfil MDHB's statutory responsibilities, and a tidal assistant to do routine prediction work so that more research could be done by Doodson, which Warburg at Hydro saw as beneficial.[73] Nobody appears to have argued against the merger.

For TI the merger was deemed beneficial from a financial and spatial point of view, and it was thought a merged LOTI could produce more research, with meteorological effects singled out as an area that could benefit. With the merger of the Observatory and TI, the patronage and governance structure that would remain until 1960 had settled. Within this structure the local shipping industry was a key patron of TI, which makes TI an unusual case within history of oceanography, and also made tidal predictions a key part of what they did and worked on.

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[70] Proudman to Vice-Chancellor Mountford, 29th Oct 1959, P744/5, LUA.

[71] The Observatory agreed, giving the Institute rent-free use of the Clock Room, with the proviso that the present instruments (clocks and a wireless set) were to stay and the Observatory have access to these instrument at all times. Proudman was happy with this arrangement. Minutes of Meetings of Joint Committee of MDHB and the University, 16th Nov 1927 and 19th Mar 1928, D/BO 1/1/1, MMM – North Street.

[72] Vice-Chancellor to Hydrographer, 4th Dec 1928, H7452/28, UKHO

[73] See various notes and minutes by Warburg in H 8761/23 and H 835/27, UKHO

4.5 The role of tidal predictions in TI's finance, 1920s – 1950s

To enable the rest of the thesis to focus on TI's storm surge work, this section considers the role of tidal predictions in TI's finance up until the end of the 1950s, and also discusses some of their non-storm surge work in this period. By mid-December 1923 TI was receiving its first instructions from Hydro to provide predictions under the new arrangement between the two organisations.[74] Thereafter the number of predictions gradually increased, with a step change at the end of the 1920s when the Roberts family sold their tidal predictor machine and business to TI at the death of Mr Roberts senior.[75] The slow increase in predictions then continued until another step change during the Second World War (see section 6.3 for analysis of this). The graph in Figure 4.2 illustrates this development. By the late 1950s they were providing full predictions, giving times and heights of both high and low water, for over 180 ports, as well as a substantial number of less comprehensive predictions, making TI one of the largest providers of tidal predictions in the world.[76]

[74] Notes and correspondence in H 4434/23, UKHO

[75] Scoffield, *Bidston Observatory*.

[76] It is difficult to give an exact number for the less comprehensive predictions, but Scoffield quotes 600 almanac predictions per year in the late 1950s. Ibid., 216.

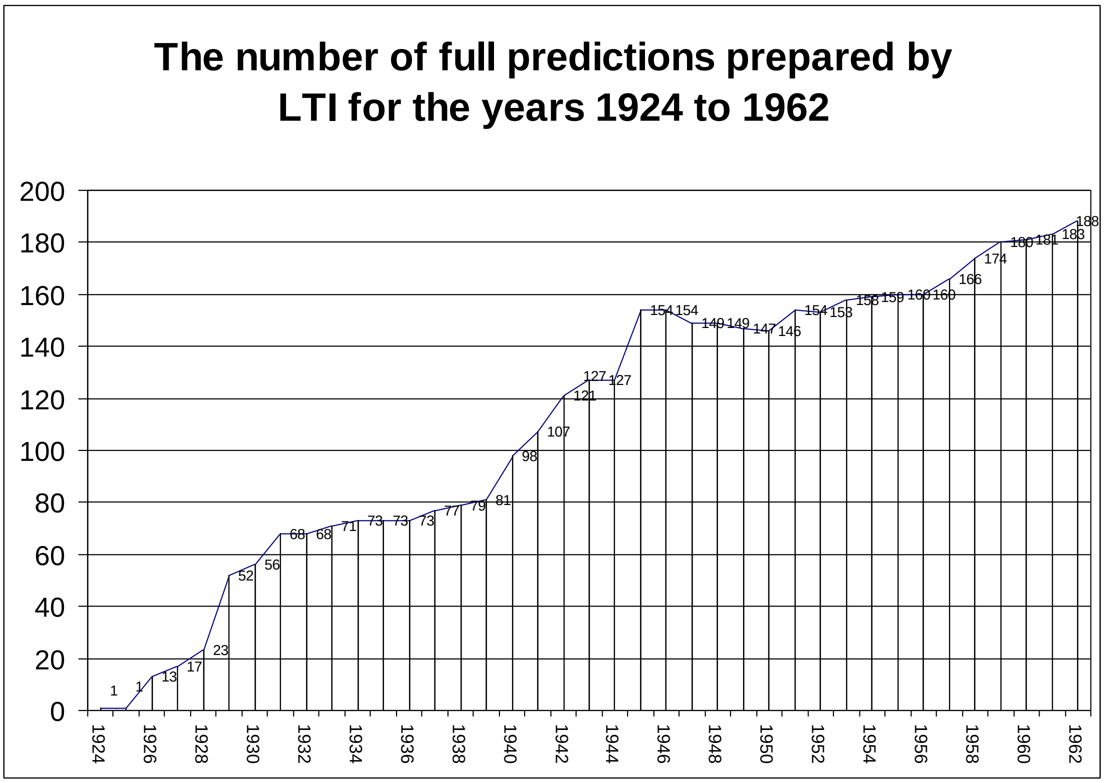

Figure 4.2: The number of full tidal predictions prepared by TI for each year 1924-1962. Predictions were made one or two years in advance, so the numbers represent work done a couple of years earlier. Numbers are for full predictions made for 'standard ports' prepared using the harmonic method, and do not include less comprehensive predictions prepared for almanacs (made using a simpler method).[77]

Such tidal prediction and analysis work gradually became the backbone of TI's finance.[78] Hydro commissioned many of the predictions TI made, either directly or by acting as an agent, e.g. for port authorities or dominions, but some clients went direct to TI.[79] I mentioned above that TI had been commissioned to calculate tidal predictions for Holden's Almanack in Liverpool. Such commissions from almanacs also increased over

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[77] Based on information in LOTI's Centenary Report and Annual Reports 1940-1945, and on LOTI's Annual Reports from the period 1946-1960, published in Liverpool by the University of Liverpool (after 1958 published by C. Tinling and Co. Ltd.).
[78] TI's ledger, S2147, LUA.
[79] "The Liverpool Observatory and Tidal Institute", Draft article for *The Dock and Harbour Authority*, Proudman to Roberts, 16th May 1935, D/BO 5/1/1, MMM – North Street. As an example of how Hydro's agency role worked, the very first predictions TI made under the 1923 agreement included a few for ports in New Zealand. The order had already been sent to Hydro, who decided to give the contract to TI. Hydro sent TI the order and the necessary tidal information (e.g. constituents for the ports). Once the predictions had been made by TI these were sent to Hydro, together with the accounts. Hydro checked the predictions, passed these onto the High Commissioner for New Zealand and certified TI's accounts. The accounts went back to TI, who sent them to the High Commissioner which then paid for the predictions. Notes and correspondence in H 3662/24, UKHO

the years.[80] Table 4.1 shows the gradual increase in income from what TI's ledger and accounts calls "commercial work" as well as the contributions from the primary components of this work: tidal analysis and predictions. The table also indicates the increase in TI's reserve account between 1929 and 1957, indicating the generally healthy state of TI's finance. During and just after the Second World War TI's commercial income increased dramatically, but even before this date TI's overall financial balance had been so strong that they between 1938 and 1951 repaid a substantial part of a grant they received from MDHB.[81] For example, in 1947 TI's commercial income was £5079 and they returned £500 of their £1500 grant to MDHB, still leaving them with a reserve account balance of £7695. The commercial work paid for itself and often made a profit. It made TI relatively secure financially, though they also continued to receive support from the University and MDHB.

[80] For example, there was a sharp rise in the number of places for which predictions were specially prepared for almanacs during the Second World War, from 20 places in 1941 to 59 in 1945, when over 30 publishers of almanacs bought predictions from TI. see LOTI, *Centenary Report and Annual Reports (1940-1945)* (Liverpool: University of Liverpool, 1945), 22.

[81] In 1938, the 1929 LOTI merger agreement was amended to allow part of TI's surplus to be returned to MDHB, as long as the Reserve Fund was maintained at an 'adequate' level, by which was meant £2000 together with the credit of the Income and Expenditure account. Minutes, LOTI Committee, 12 Dec 1938, D/BO/2/1/3/2, MMM – North Street

Year	Predictions	Analyses	Total income from all commercial work	Grants	Total income	Amount returned to MDHB	Balance of reserve account
1929	775	312	1781	1675	3456		1482
1930	1225	255	1869	1650	3519		1858
1931	1246	101	1867	1550	3417		2299
1932	1174	173	1530	1550	3080		2254
1933	1158	220	1593	1550	3143		2288
1934	1103	76	1406	1550	2956		2353
1935	1246	196	1779	1550	3329		2283
1936	1361	327	1988	1550	3538		2381
1937	1557	223	1973	1550	3523		2481
1938	1540	342	2155	1550	3705	Unknown	2461
1939	1803		2358	1550	3908	Unknown	
1940	2539		3275	1550	4825	Unknown	
1941	2067	58	2611	1550	4161	Unknown	4278
1942	2337	219	2631	1550	4181	500	4476
1943	2619	97	3039	1550	4589	500	4894
1944	3049	169	3338	1550	4888	1000	5020
1945	4680	648	5965	1550	7515	1000	5265
1946	3521	338	4220	1550	5770	1250	7416
1947	3626	642	5079	1550	6629	500	7695
1948	3513	1624	5782	1550	7332	500	8294
1949	3889	581	4992	1550	6542	500	9378
1950	4770	416	5850	1550	7400	500	6520
1951						Unknown	
1952	5382	362	6547	2000	8547		
1953	6504	386	7915	2000	9915		6793
1954	6831	1152	8325	2000	10325		7952
1955	7021	390	7909	2000	9909		10213
1956	7175	370	8295	2000	10295		8796
1957	8305	638	10029	2000	12029		9258

Table 4.1: Some of TI's income streams, the sum they returned to MDHB between 1938 and 1952 and the balance of TI's reserve account, all in pound by year. The label 'commercial income' includes tidal predictions and analyses as well as income from providing meteorological information, "special tidal work" and other small income streams such as from tests (of instruments) or sale of publications. Grants were given by LSOA (only in 1929 and 1930), MDHB (£1500) and the University (£50 until 1951, £500 thereafter).[82]

[82] Accounts 1929-38, D.BO 2/1/3/1 and Accounts 1942-57 (including figures from 1941), D.BO 2/1/3/2, MMM - North Street. Either side of these dates the accounts are either not available or in a format that is not directly comparable, e.g. displayed in a different format and using a different financial year before 1929 (see Appendix for some financial information for this period). The accounts for 1951 and 1952 are missing, though figures for 1952 were taken from the accounts for 1953. Accounts are also missing from 1939 and 1940 for which the commercial income has been calculated from the Revenue account summary, in Ledger, S2148, LUA.

TI's workers did not organise their predictions according to whether they were for a military, civil state or industrial client. Instead predictions were organised according to the place and type of prediction (a 'full' one of or a less full one). While they sometimes listed their clients in their annual reports, predictions were listed according to the type ordered, not the type of client.[83] In addition, work done for or via the Navy and published in the Admiralty Tide Tables will have been used by merchant shipping just as much as by the Navy, and also for example by engineers and port authorities. TI's prediction work was commercial in the sense that it brought income to the Institute but the predictions themselves cut across any boundaries between military, civil state and industry. While TI's researchers portrayed it as a scientific institute, much of TI's work and income came from calculations that were linked less to the concerns of academics than to the concerns of those who used or tried to manage the sea, such as the Navy, merchant shipping and port authorities.

Because TI's workers kept their records by the type of prediction and not by the type of the client, it is also difficult to get a sense of the importance of different groups of clients, such as the Admiralty versus almanacs and others. However, a rare analysis of TI's income from predictions by different regions gives a flavour of the scale of the importance of different group of clients, see table 4.2. In 1956 only 14% of TI's income from tidal prediction came directly from the Admiralty. A fifth of predictions were for named imperial countries (Canada, Australia and New Zealand), which are likely to have been administered by the Admiralty. A third of TI's income came from almanacs , while another third came from "elsewhere". Other figures from the 1950s confirm that about a third of TI's income from predictions then came from almanacs.[84] Unlike the Admiralty and MDHB, the almanacs were not directly represented on TI's governing

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[83] For example, while organising their predictions by location, in 1933 TI also listed who they supplied analyses and full predictions to, namely to the following clients: Hydro; Anglo Saxon Petroleum Co.; Corporation of Bristol; L'Adminstration des Ponts et Chaussées, Belgium; Port Director, Basra, Mesopotamia; Canadian Hydrographic Service; Crown Agents for the Colonies; Colonial Office; Donsink Observatory, Co. Dublin; Ministere de la Marine, Paris; Holden's Almanack; Harwich Harbour Conservancy Board; Survey of India and Hydrographer, Imperial Japanese Navy; Port of London Authority; London and North Eastern Railway Co.; Agent General for New South Wales; Queensland Government; Southampton Harbour Board; Agent General for South Australia; Sydney Harbour Trust; Coast and Geodetic Survey, USA; Agent General for West Australia and High Commissioner for New Zealand. It is noticeable that many, though not all, of TI's clients were from within the British Empire. LOTI, *Annual Report 1933* (Liverpool: University of Liverpool, 1933).

[84] "Liverpool Observatory and Tidal Institute", D.BO. 2/1/3/2, MMM - North Street

committee, but like them their primary interest in TI's work was in the provision of tidal predictions.

Analysis of prediction income 1956		
Admiralty	£1,000	14%
Canada	752	10%
Australia	540	8%
New Zealand	168	2%
Elsewhere	2440	34%
Sub-Total	£4,900	68%
Almanacs	2275	32%
Total	£7,175	

Table 4.2: TI's income from predictions by region and type of client[85]

The interest of so many of TI's funders in tidal analysis and predictions had an obvious impact of TI's research programme. They continued to regularly do work in this area, for example further developing the tidal predictor machines, finding ways of analysing for constituents from non-harmonic predictions of tides or where the tidal observations only listed high and low water heights and times.[86] All this work resulted in further publications, both in academic journals[87] as well as less academic publications such as the *Admiralty manual of tides* Doodson wrote together with Warburg, which summarised a non-harmonic method of predicting tides developed by the two, known as the Admiralty method.[88] TI's research programme also included much other work, such as continuation of the theoretical work led by Proudman, the production of cotidal charts published by the Admiralty,[89] work on currents, including measurements from ships for which Doodson devised a current meter,[90] and work criticising hydraulic models simulating tides.[91] One aspect of their work was on storm surges. The rest of this thesis

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85 D.BO 2/1/3/2, MMM - North Street
86 Arthur Thomas Doodson, "The Analysis and Prediction of Tidal Currents from Observations of Times of Slack Water," *Proceedings of the Royal Society of London. Series A* 121, no. 787 (1928); LOTI, *Annual Report 1950* (Liverpool: University of Liverpool, 1950); LOTI, *Centenary Report and Annual Reports (1940-1945)*; LOTI, *Annual Report 1946* (Liverpool: University of Liverpool, 1946), 5-6.
87 For example Arthur Thomas Doodson, "Further Comments on the 19-Yearly Tide," *Bulletin Géodésique (1946 - 1975)* 55, no. 1 (1937).
88 Harold Dreyer Warburg and Arthur Thomas Doodson, *Admiralty Manual of Tides* (London: H.M.S.O., 1941).
89 Arthur Thomas Doodson and Robert Henry Corkan, "The Principal Constituent of the Tides in the English and Irish Channels," *Philosophical Transactions of the Royal Society of London. Series A* 231(1933).
90 See TI's Annual Reports from the 1930s.
91 See correspondence in Box 129, BA

concentrates on this work on storm surges, especially that work related to making surges (as opposed to periodic tides) more predictable, in order to focus the thesis on how scientists and politicians have attempted to make irregular events more predictable.

4.6 Conclusion

This chapter has discussed how TI used their work on tidal predictions and meteorological effects to gain funding. Through the use of this work, as well as through networking, patronage appeals and various negotiations between different actors, their funding gradually coalesced into a particular patronage structure which was quite stable between 1929 and 1960, supporting TI and its work. This and the previous two chapters have followed how this hybrid, triangular patronage structure came to be, by following developments at TI up to 1929. The three corners of TI's patronage were industrial, Navy and academic patrons. More and more of TI's funding came from selling tidal predictions, partly to industrial actors such as almanacs but importantly to Hydro, which provided a strong link to the Navy. While this formed the backbone of TI's finances, such a backbone would have toppled on its own, and TI received important support from other patrons. The industrial patrons – Liverpool shipping owners, primarily through MDHB and also through LSOA – became key, providing much funding and a building. Liverpool University provided a link to academic science, which was an important point in TI's favour for Hydro, and was involved in TI's governance but provided only limited funding.

This hybrid patronage impacted on TI's work, much of which was focused on producing tidal predictions and making these ever more accurate. At the same time, its identity was as a scientific institution where academic research was done and published in scientific journals, for which the commercial work was 'just' income.[92] To TI this setup, as "a scientific institution owned and run by businessmen [and the Navy]"[93], remained a viable (though at times debated, as I will discuss in later chapters) way to fund research for several decades. One of the MDHB representatives described

[92] For example, in 1945 in an annual report, Doodson and Proudman emphasised that TI's "importance" came not from the analyses and predictions but from its academic tidal researches. LOTI, *Centenary Report and Annual Reports (1940-1945)*, 13.

[93] Proudman to Mountford, 29th Oct 1959, P744/5, LUA

Doodson during his retirement dinner in 1960 as "perhaps the only man whoever conducted scientific research at a profit".[94] While partly simply a flattering comment, it also summarises Doodson's, and TI's, balancing and combining of science with commerce.

TI's strong dependence on the shipping industry for patronage contrasts with the existing literature's emphasis on military support for physical oceanography.[95] It was the support from the shipping industry that enabled TI to do the work that was then put into practice generating income by producing predictions for Hydro and other clients. While both BAAS, Liverpool University and Hydro via DSIR also supported this work, they did so only *after* the Booths' funding had established TI, making the support of academic and state actors dependent on TI's earlier industrial patronage. In addition TI's initial research programme, once the Institute had been established, emphasised the production of tidal analysis and predictions for merchant and naval shipping to suit their shipping industry patrons, after which the concerns of Hydro gradually became important too. While naval patronage was important to TI, other sources of patronage were at least as important, something which could be further investigated in other case studies by historians of oceanography. In addition, paying detailed attention to the patronage structure of research institutes such as TI gives us a more detailed picture of how patronage from different sources interacted and depended on each other. While TI, through its contract with Hydro, became a contractor to David Edgerton's warfare state,[96] one reason Hydro preferred TI was because it was seen as neither state nor industry but as academic. Also, the relationship between Hydro and TI developed as a result of opportunistic negotiations on both sides, not in response to a unified policy from 'the state'.

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[94] Given the healthy bank balance of TI in the 1950s, see table 4.1, as well as the tone of the speech I do not think this was meant as a joke. Mr Paton's speech, folder Retirement, Doodson Papers. The retirement dinner was held in the Dock Office, Liverpool on the 22nd September 1960, and the guests were served Grapefruit Maraschino, Fried Dublin Bay Prawns, Roast Grouse or Lamb Cutlets with game chips, potato croquettes, runner beans and peas, Apple Charlotte and Kidney Vol-au-vent, with a selection of wines, see Menu, Doodson Papers.

[95] Hamblin, *Oceanographers and the Cold War*; Weir, *Ocean in Common*; Doel, "Constituting the Postwar Earth Sciences."; Mukerji, *A Fragile Power*.

[96] Edgerton, *Warfare State*.

Chapter 5 (1928), The turn from shipping to flooding and the construction of surges

Around 1am in the morning of Saturday 7 January 1928 the Thames broke through its banks in central London. In the flooding that followed fourteen people died and there was much material damage. This chance event changed the aim and patronage structure of TI's storm surge science, shifting its focus towards forecasting of flooding and shifting funding towards local government actors. However, other aspects of TI's work were retained, such as statistical techniques and the concept of residuals. Chapter three looked at TI's definition of meteorological effects as residuals in a fairly abstract way. This chapter instead focuses on how TI constructed storm surges, as they now called them, through practices of calculation. It looks at how TI constructed surges in two closely linked ways: as graphs on documents using particular practices of calculation and as *the* definition of the events that the scientific inquiry set up after the flooding would investigate.

The flooding event in 1928 quickly became a political issue debated in the newspapers and a committee was set up by the Prime Minister bringing together local and central government actors. This committee in turn commissioned a scientific investigation, funded by local authorities, which TI became involved in together with a number of other organisations such as the Met Office and the Port of London Authority (PLA). The investigation was supposed to lead to improvements in warnings for flood events like this one, as well as estimating the likelihood of such events recurring. Another key question for the investigation was the cause of the event, closely linked to which was the definition of what exactly it was they were researching. What had made the Thames rise? What other events were like this one and should be investigated by the researchers?

A list of 'high high tides' produced by PLA formed the initial definition of the sort of events the investigation were researching, but TI's staff questioned this definition. The chapter describes how TI convinced others to use their definition of the event through a combination of rhetorical displays of expertise and by work on constructing surges. To begin with TI constructed their own list of events based on their earlier definition of meteorological effects as residuals. However, this list was insufficient for the calculations of the other key research actor, the Met Office, as they needed more information on the timings of the events on the list. Before the Met Office did work with TI's definition of the event TI had to provide them with these timings, which they did by constructing detailed graphs of the surge events on their list. I describe in detail how they constructed surges as such graphs of residuals. By construction I mean how TI defined and calculated surges by putting them together from graphs and numbers, using pen and paper on specific documents. These practices produced inscriptions on documents that were a particular representation of the type of event TI argued had caused the flooding. They called this representation storm surge, which through their work was defined and constructed as a residual. Through these practices of calculation surges were constituted as scientific objects that both TI and the Met Office could calculate with.[1] However, TI's transformation of the event into a surge was contested by others and they had to do much work to have their definition accepted. In addition, they did not achieve their aim of making surges predictable. This together with other results of the research provided an argument for future work to be undertaken.

5.1 The 1928 flooding event and its party politics

On Friday 6 January 1928 a depression in the North Sea produced strong winds up to gale force over the eastern and south-eastern part of England. In words used at the time, these winds produced an 'abnormal' and 'extraordinary' rise in sea level at about the same time as the high water of a springtide, at 1AM on Saturday, which led to very high river levels in the Thames.[2] The flood defences along the Thames had been designed to withstand a tide of 18 feet above Ordnance Datum, a height decided on after a previous record tide, reaching 17 feet 6 inches, had caused flooding in 1881. The height of the tide early in the morning on the seventh of January 1928 exceeded this previous record by 11 inches, according to the official estimates. The flood defences were breached in several places. Many poor families slept in basement rooms into which the water entered fast and 14 people drowned.[3] There was flooding in the City, Westminster, Southwark and less central areas, including Putney and Hammersmith,

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[1] Kalthoff, "Practices of Calculation."; Lynch, "The Externalized Retina."
[2] See e.g. S. T. A. Mirrlees, "The Thames Floods of January 7th," *The Meteorological Magazine* (1928).
[3] Joan Gwilwym Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods from the River Thames in the County of London," in *Command Papers; Reports of Commissioners* (1928), 8.

and thousands of homes were damaged.[4] Most of the poor were uninsured and the insurance of many businesses did not cover floods.[5]

London based politicians from all parties immediately put the event to use in a wide range of debates, ranging from land drainage to the plight of the poor living in basement slum dwellings, and how to govern London. The event also became a focus for debates regarding how to govern flood defence policy: who should organise and pay for flood defence in the capital? More generally, should flood defence be seen as a local responsibility or as a national issue? At this time flood defence was by many seen as a local issue and an 1879 Act had, for London, enshrined this view in law.[6] While LCC had an overview role in London, the defences were paid for by the owners of the riverbank, but actors, including the Treasury, questioned why such riparian owners, for example government departments, had to pay for protection of buildings well beyond the river bank, not owned by them.[7]

Many of these debates hinged on how the causes of the event were understood. [8] Linking the flooding event to inland non-tidal flooding linked it to the issue of land drainage, the role of central government in this and who should pay for it.[9] If instead the deaths were said to be caused by poverty, with poor people living in vulnerable basement flats, this linked to demands for social reforms. On the other hand, if the event was said to be due to poor upkeep of flood defences, blamed on local authorities, this linked to demands for reform of local government. These kinds of socio-political causality-stories were told primarily by Labour politicians and in left-leaning media such as the *Manchester Guardian*. For example, a grouping of politicians on the left called London Labour used the event to call for political reforms to London's local government to create a "real Corporation of London" instead of the multitude of

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[4] Mirrlees, "The Thames Floods of January 7th," 17; "Plight of Victims of the Flood," *Daily Mirror*, January 9 1928; "London's Peril Not yet Over," *The Manchester Guardian*, January 9 1928.
[5] "Our London Correspondence," *The Manchester Guardian*, January 9 1928.
[6] *London County Council (General Powers) Act 1929 - Prevention of Floods*.
[7] "Thames Flood," Minute by AT Harris, 23rd Jan 1928, HLG 50/130, NA. See also Minute by WL to Mr de Normann, "Thames Floods," 29th Feb 1928, WORK 6/403, NA and other documents in that file for more on the views of the Ministry of Works. See also "Parliament and the Flood," *Times*, January 09 1928.
[8] For more on how the event was framed, see my Anna Carlsson, "What Is a Storm: Severe Weather and Public Life in Britain in January 1928," in *Weather, Local Knowledge and Everyday Life: Issues in Integrated Climate Studies*, ed. Vladimir Jankovic and Christina Barboza (Rio de Janeiro: MAST, 2009).
[9] This will be discussed further in the next chapter. See John Bowers, "Inter-War Land Drainage and Policy in England and Wales," *Agricultural history review* 46, no. 1 (1998); John Sheail, "Arterial Drainage in Inter-War England: The Legislative Perspective," *Agricultural history review* 50, no. 2 (2002).

councils that existed. They argued that the existing regional government, London County Council (LCC), had not done sufficient work on the flood defences. In addition, they argued that the deaths were due to poor city planning and a lack of warning system, both of which they linked to the weakness of the current system of governance of London.[10]

An alternative framing of the event as extraordinary and unpreventable, caused solely by natural causes, decreased attention to potential socio-political issues related to the flooding. These natural causes were said to be primarily the combination of wind and tide, with some small addition of up-river floodwater (not seen to be linked to drainage). The government and local authorities, together with supporting media such as *The Times*, frequently portrayed the event in this way. As an example of this, in its January manifesto for the March 1928 LCC elections, the controlling conservative Municipal Reform Party, presented the event as "due to abnormal and unprecedented conditions which could not be foreseen by the eminent technical and engineering advisors of all the authorities, both past and present".[11] By portraying the event in terms of natural causes and exceptionality (at least in public), central and local government exonerated themselves. For example, just after the event the General Manager of PLA told the press: "We are not responsible in any shape or form. We could not possibly foresee what was going to happen, especially when nothing like it had occurred for a century or more".[12] Ted Steinberg has found very similar arguments used in the US, where government actors frequently have framed disasters as 'natural' and exceptional, and avoided being blamed.[13] These kinds of arguments are thus the norm and not the exception after disasters, but how did this event become linked to TI's storm surge science?

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[10] Joint Committee on Thames Flood, Statement approved by the Conference, The London Labour Party, 11th Jan 1928, HLG 50/130, NA. See also "Labour Demand for Inquiry," *Times*, January 12 1928; "Responsibility for Floods," *The Manchester Guardian*, January 12 1928.

[11] "L.C.C. Election," *Times*, January 26 1928.

[12] "Could Warning Have Been Given?," *The Manchester Guardian*, January 9 1928. See also, "Two Lives Lost at Putney," *Times*, January 09 1928.

[13] Ted Steinberg, *Acts of God: The Unnatural History of Natural Disaster in America* (Oxford: Oxford University Press, 2006).

5.2 How the event turned into a scientific investigation

As just discussed, the 1928 flooding event led to attention by the press and politicians to flood defence policy, flood science and a wide range of other socio-political issues. Sheldon Ungar has argued that "dramatic events transform underlying dread into social scares", enhancing audience receptiveness to claims-making from scientists, and that such media attention after an extreme event can be an important trigger for scientific research.[14] This happened after the 1928 event, when the scientists at TI, via their contacts at Hydro, were able to make a claim for funding for research in an area they were already interested in as politicians got interested in tidal flooding.

Following demands in the media and Parliament for an inquiry into the event, the Prime Minister, Stanley Baldwin, invited national, regional and local government actors to a conference "to settle what action can, and should, be taken to obviate any recurrence of such […] disaster". The Conservative Prime Minister side-stepped issues of causality, blame and criticisms regarding the governance of London and the plight of the poor by emphasising future prevention: "The object of the conference is not to discuss the responsibility for the incidents of last week-end, but to consider steps that should prevent a recurrence".[15] By framing the event as he did, Baldwin framed it as 'just' about flooding, not about London governance or poverty-reduction.[16] He linked it to technical matters only – what flood defence and other measures were necessary to prevent a recurrence? This limited framing was used partly in response to demands from Labour politicians for other kinds of state assistance after the flood and in opposition to London Labour's wider view of the causes of the event as linked to poverty and the organisation of local government. Some officials saw Labour's arguments as aimed at making political capital of the event: "the Labour Party seem inclined to make a stunt of the business in Parliament and no doubt at the L.C.C. elections [in March]".[17]

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[14] Sheldon Ungar, "The Rise and (Relative) Decline of Global Warming as a Social Problem," *The Sociological Quarterly* 33, no. 4 (1992).
[15] "Prevention of Floods," *Times*, January 13 1928. I have been unable to find archival files shedding further light on the Prime Minister's or Cabinet's views on the event.
[16] The Minister of Health, Neville Chamberlain, later similarly limited the frame of the event to certain areas of public policy (e.g. land planning and flood defence). "The London Flood B," *Times*, February 3 1928; "Flood Lesson," *The Manchester Guardian*, February 3 1928.
[17] Minute by WA Ross to Kingsley Wood, 19th Jan 1928, HLG 50/130, NA. See also Joint Committee on Thames Flood, Statement approved by the Conference, The London Labour Party, 11th Jan 1928, HLG 50/130, NA.

The conference was chaired by Sir Kingsley Wood, Parliamentary Secretary to the Minister of Health, and held at the Ministry of Health. This was the government department that dealt with local government and supervised local authorities, so the choice of the Ministry of Health as the co-ordinating body for the conference mirrored and strengthened the existing allocation of responsibility for flood defence to local government.[18]

The Conference was attended by local and regional authorities and a range of government departments and it quickly set up a Technical Sub-Committee to look further into questions of flood defence and the establishment of a warning system, which had been much discussed in the press,[19] and also to look at the causes and frequency of surges. The conference allocated these matters to the Technical Sub-Committee to have them clarified and further considered.[20] This group of technical members of the conference were thus given control over the definition of particular aspects of the event by the establishment of a boundary between issues to be considered by technical experts and other issues, such as who should pay for flood defence and the role of basement dwellings, that should be considered by all. Through this boundary-drawing and appropriation, one part of the event was transformed into the concerns of particular expert techno-scientists.[21]

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[18] *Whitaker's Almanack*, (London: Joseph Whitaker & Sons, 1930), 516; Gail Savage, *The Social Construction of Expertise: The English Civil Service and Its Influence, 1919-1939* (London: University of Pittsburgh Press, 1996).

[19] In its coverage of the event, the newspapers paid attention to a range of technical matters related to flood defence, especially the rebuilding and strengthening of physical flood defences and the lack of warnings. This was for example a key aspect of the coroner's inquests, e.g, in Westminster, which in turn were heavily covered by the newspapers, see "The London Flood A," *Times*, January 11 1928. "First Inquiries into Thames Disaster," *The Manchester Guardian*, January 11 1928. "Thames Flood Victims," *Times*, January 19 1928.

[20] Minutes of Committee Meeting 19th Jan 1928, "London Floods," H 644/28, UKHO. The authorities represented at the conference were LCC, PLA, Thames Conservancy Board, the Corporation of the City of London, the City of Westminster, the riverside boroughs (Poplar, Stepney, Chelsea, Fulham, Hammersmith, Wandsworth, Battersea, Lambeth, Southwark, Bermondsey, Greenwich, Woolwich, and Deptford), Ministry of Health, the Home Office (representing national government interest into the police), the Ministry of Agriculture (ditto for drainage), the Board of Trade (government supervision of the foreshores), and the Ministry of Transport (which was Parliament's link to PLA). Each representative, e.g. the mayors from the boroughs, was allowed to bring a technical advisor. Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods "; "Prevention of Floods."

[21] While this was similar to what Gieryn has called boundary-work, he emphasises 'public science', and this was only partly public. Thomas F. Gieryn, "Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists," *American Sociological Review* 48, no. 6 (1983); Thomas F. Gieryn, *Cultural Boundaries of Science: Credibility on the Line* (London: The University

The Technical Sub-Committee consisted of representatives from the Admiralty's Hydrographic Department, the Met Office, PLA, the Thames Conservancy (the body responsible for the non-tidal Thames above Teddington), the Ministry of Health and the Board of Trade. In other words they represented government departments and river-related organisations, and not the local authorities who were also part of the Conference, though LCC was also present. The representatives either were or brought with them technical experts from their respective bodies, with for example PLA's Engineer, LCC's Chief Engineer and PLA's River Superintendent and Chief Harbour Master, Commander E C Shankland, all presenting evidence to the committee. This Technical Sub-Committee agreed with the other establishment and government actors that the main causes of the flooding event were "natural", defining the cause as an unusually high tide due to meteorological effects.[22] On the other hand, it claimed it was necessary to investigate the details of the process leading to such "abnormal" tides further to answer the Conference's questions, as the frequency and height of future floods could not be estimated until tidal flooding was better understood.[23] In turn this lack of knowledge meant, they claimed, that the benefits of increasing the flood defences could not be compared to the costs of building them. The experts thus argued that to research abnormal tides was to ensure that the flood defences would not be built unnecessarily high, i.e. unnecessarily expensive.[24]

In addition, the Technical Sub-Committee claimed that more research was needed to improve the emergency warning system that had been set up for the Thames.[25] The introduction of an emergency warning system straight after the event had been the main

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of Chicago Press, 1999); Thomas F. Gieryn, "Boundaries of Science," in *Handbook of Science and Technology Studies*, ed. Sheila Jasanoff*, et al.* (London: SAGE, 1995).

[22] Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods ", 22-23.

[23] Ibid., 13 and 22-23.

[24] However, after further work these experts in fact implemented an increase in the standard height of the defences along the Thames. While the experts claimed to be able to limit demands on finances through their work, their work also led to demands on finances, which is an unsurprising outcome. "Thames Flood Dangers," *Times*, March 10 1930; George Humphrey and Frederick Palmer, "On the Future Standard of Thames Floods Prevention Works in the County of London," in *Main Drainage Committee, Thames Floods Prevention* (London County Council, 1929).

[25] Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods ", 23-25.; and draft minutes attached to letter from [unreadable]Chief Civil Assistant to Hydrographer to Doodson, 6th Feb 1928, Box 16, BA; "London Floods Technical Sub-Committee 2nd meeting on the 3rd Feb 1928", Box 16, BA.

immediate "step[...] that should prevent a recurrence" in Baldwin's words.[26] There were of course political considerations involved in the creation of the warning system, with for example the issue of blame if another flood struck without a warning being weighed against the possible blame over "needless alarms" if the system warned too often.[27] The warning system involved a combination of weather watchers at the Met Office, triggering tidal watchers at Southend who if necessary triggered river watchers from the Police and awareness at various local authorities. The warning system thus relied on the Met Office's synoptic, i.e. graphical and pattern-based, weather forecasts and the broadcast of current measurements of sea level – nowcasts instead of forecasts – together with an intricate system of linkages of different authorities in the Thames area. This warning system did not try to predict the height and timing of the meteorological effects but instead warned of dangerous meteorological conditions and of high water levels.[28]

TI got involved in the Sub-Committee's work via their personal contacts at the Hydrographic Department. The Technical Sub-Committee was chaired by the Hydrographer, Rear Admiral HP Douglas. To the first meeting of the conference he had provided a sketchy outline of the data and research needed to establish a flood forecasting system, mentioning TI, as did the Met Office in their initial report on the causes of the event, presented by George C Simpson, the Director of the Met Office.[29]

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[26] "Prevention of Floods." Johnson, Tunstall and Penning-Rowsell have found that major flood events in the UK have led to changes in policy since the Second World War, but also that the changes have consisted of acceleration of existing incremental changes rather than the introduction of radically new ideas. This seems also to have been the case in 1928, as more radical policy options such as changes in land use or poverty reduction to reduce the number of basement dwellers were deflected in favour of increased flood defences and the introduction of a warning system. Clare L. Johnson, Sylvia M. Tunstall, and Edmund C. Penning-Rowsell, "Floods as Catalysts for Policy Change: Historical Lessons from England and Wales," *International Journal of Water Resources Development* 21, no. 4 (2005).

[27] "Emergency measures pending settlement of permanent policy", Minute by IGG [Gibbon] at Ministry of Health, 20th Jan 1928, HLG 50/130, NA

[28] In more details, the system consisted of weather watchers at the Met Office who would alert tidal watchers in Southend (and others, such as the police) if the meteorological conditions warranted it. These tidal watchers, men from the Royal Naval Shore Signal Service which manned the Southend signal station, would then alert the Metropolitan Police if the tide reached a certain level (1 feet above the Trinity High Water datum), and the Police would send out river watchers, who would alert residents if river levels became dangerously high (according to pre-set danger levels). Draft minutes attached to letter from [unreadable signature] Chief Civil Assistant to Hydrographer to Doodson, 6th Feb 1928, Box 16, BA; "London Floods Technical Sub-Committee 2nd meeting on the 3rd Feb 1928", Box 16, BA; and Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods ", 23-25.

[29] Minutes of Committee Meeting 19th Jan 1928, "London Floods," esp. Appendix A and B, H 644/28, UKHO. For more on Simpson see M E Crewe, "The Met Office Grows Up: In War and Peace,"

As discussed in earlier chapters TI already had close contacts with the Hydrographic Department, especially with its Superintendent of Tidal Work Commander Harold Warburg, and were at this point selling an increasing number of tidal predictions to the Navy. As a member of TI's governing committee Warburg was aware of TI's research on forecasting meteorological effects in Liverpool. These contacts now also got TI involved with the committee investigating the London flooding. Warburg visited TI in late January after which Arthur Doodson prepared a research proposal for the Technical Sub-Committee.[30] While both the Hydrographer and the Met Office had referred to TI's earlier work at the first meeting of the Conference, it was only at this point that TI directly began negotiating what the event might be and mean with other members of the investigation. The official reason given to get TI involved was that it, "the best-informed body in the country on the subject of tides" according to the conference, had "considerable experience of similar problems".[31] However, without TI's existing connections with the Hydrographic Department the Institute might well not have become connected with the event at all. It appears that the Hydrographer wanted to keep his control over the Technical Sub-Committee's work by for example choosing to work with TI, whose work they knew, and not e.g. the Ministry of Agriculture's expert on land drainage and flooding, whose services the representative from that Ministry had offered the Sub-Committee. Personal networks played an important role in getting TI involved in this work and without these networks their work on meteorological effects may not have become linked to the 1928 flooding event at all.[32]

To the Sub-Committee Doodson proposed an initial investigation limited to the Thames estuary, but from the start argued that a larger-scale investigation covering the wider North Sea was likely to be necessary to fully investigate the problem of tidal flooding in the area. In support of this, Doodson said it was already known (from TI's earlier research) that surges often were generated further afield.[33] TI's proposal for research, presented to the Technical Sub-Committee in late January in person by Doodson, was accepted. The work was to be done in association with the

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Occasional papers on meteorological history(2009), http://www.rmets.org/pdf/hist08.pdf. Accessed March 2009.

30 Doodson to Warburg, and Douglas to Doodson, both 31st Jan 1928, Box 16, BA

31 Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods ", 13 and 23.

32 Minutes of Committee Meeting 19th Jan 1928, "London Floods," p 3, H 644/28, UKHO

33 Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods ", 26-27.

Meteorological Office and Hydro, with the latter in charge.[34] The cost of the work would be shared by LCC and PLA, who agreed to each pay no more than £250.[35] The two bodies, who had both been accused in the press and by London Labour of not doing enough to prevent the flooding, without much delay took on the cost of this initial research, which was tightly defined as focusing on improving flood defences in London. Given the traditional definition of flood defence as a local issue, LCC and PLA could not protest against being asked to pay for this research, especially as refusing would no doubt have led to bad headlines in what was an election year for LCC. However, they and the rest of the conference made a point of recommending that if more research was needed beyond this initial work, central government ought to consider paying for it.[36] While accepting the initial work as of primarily local interest to be paid for locally, the conference laid the groundwork for future demands on central government for further work.

The Technical Sub-Committee had had three points "relegated" to them for consideration.[37] These points emphasised causes and frequency of tidal floods and the establishment of a warning system, and Doodson's work similarly mirrored this emphasis. Both the Sub-Committee and the Conference as a whole wanted to make these kinds of currently unpredictable events predictable and 'improvements' to the emergency warning system were sought both by Doodson and his patrons. Doodson's mathematical work on predictions was aimed at 'improving' the warning system: "I shall spend my time first of all on the meteorological phenomena associated with the storm surge, for the sake of the warnings".[38] This emphasis on improving the warning system was also found in statements from the Technical Sub-Committee. For example, the letter formally appointing TI to do the work emphasised improvements to the warning system, which TI was to provide by increasing the knowledge of which meteorological conditions brought unusually high tides.[39]

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[34][Unreadable signature] for Hydrographer to Doodson, 3rd Mar 1928, Box 16, BA

[35] "Thames Floods," *Times*, March 1 1928; Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods ".

[36] Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods ", 24.

[37] Ibid., 22.

[38] Doodson to Shankland, 10th Feb 1928, Box 16, BA

[39][Unreadable signature] for Hydrographer to Doodson, 3rd Mar 1928, Box 16, BA

At this stage the flooding event had to some extent been transformed from a socio-political issue into a specific techno-scientific problem to be investigated by experts, including TI. Their existing networks, especially with Hydro, got them involved in the investigation. Having been given this opening by Hydro, TI argued that their earlier work on surges could be developed to provide answers for the departmental committee. They clearly tailored their proposal to fit the audience, focusing on predictions and local London issues. However, TI were more concerned with achieving results that would justify their funding than with sticking to their original proposals. For example, Doodson almost immediately extended the local focus, which had enlisted London-based patrons, to include meteorological conditions not only near London but also further afield. To the Hydrographer he claimed to have been "compelled" to investigate storm effects not only local to the Thames but further afield in the North Sea "if any progress was to be made".[40] While the patronage TI received clearly influenced their work, making them focus on forecasting floods in London, they were primarily concerned with justifying this patronage, which they did both through scientific and rhetorical displays of expertise. The rest of this chapter focuses on this justificatory work, starting with how Doodson named the event.

5.3 THE APPEARANCE OF THE TERM 'SURGE'

When Doodson presented his research proposal to the Technical Sub-Committee, he introduced a new term to describe what it was he was proposing to research: surges. Earlier work had used terms such as meteorological effect or perturbation, but not the term surge.[41] The new term was introduced in the short proposal Doodson presented to the Technical Sub-Committee, which, in the first paragraph, stated that "storm effects [on tides] are surges with pseudo-periods of about six hours". The term surge was then used throughout the report, interchangeably with other terms such as storm effects and meteorological perturbations, without further definition.[42] From 1928 onwards

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[40] Doodson to Hydrographer Douglas, 2nd Apr 1928, Box 16, BA

[41] For example, Arthur Thomas Doodson, "Meteorological Perturbations of Sea-Level," *Nature* 112, no. 2812 (1923); Joseph Proudman and Arthur Thomas Doodson, "Time-Relations in Meteorological Effects on the Sea," *Proc. London Math. Soc.* s2-24, no. 1 (1926); Doodson, "Meteorological Perturbations of Sea-Level and Tides."

[42] Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods ", 26-27 quote p26.

Doodson and his colleagues at TI regularly used the term in published scientific work, especially in titles, but the older terms also remained in use, with 'storm surge' only slowly becoming more widespread in specialised scientific work.[43] It was however picked up quite quickly by those involved in the investigation. In March the Hydrographic Department's tidal officer, Warburg, wrote to the Met Office's Director: "[Inland flood-water] can only be dangerous if occurring at same time as a 'storm surge'".[44] It seems the term was picked up as a convenient short-hand, though marked out as new here through the quotation marks, by others involved with the investigation at this time. From here on the term 'surge' is an actor's term, though 'storm surge science' remains an analytical term rarely used by the actors I am studying.

Doodson did not provide a contemporary explanation of why he introduced this particular term. Much later, in 1953, TI's annual report stated that the term surge had been chosen by Doodson "to emphasise that the disturbances of sea level by wind were dynamic effects as distinct from a slow increase of level to a fairly steady condition lasting for one or more days".[45] He also gave a similar explanation in his Royal Society Personal Information File, where he said that his research on meteorological effects had led him to see "that the phenomenon was essentially dynamic and kinematic".[46] In 1933, discussing with his contact at the Met Office whether to reply to an engineer's article on meteorological effects which described them in static terms, Doodson emphasised the dynamic nature of surges: "Great surges seem to be dynamically generated- the speed of the disturbance relative to that of a free wave is a vital factor".[47] It seems the term 'surge' was picked to emphasise the speed and dynamism of the phenomena.

By introducing a new term Doodson also designated the kind of events he thought the investigation should study, which was dynamical meteorological effects with pseudo periods of six hours. This definition of the event was a development of his earlier work

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[43] For example, Robert Henry Corkan, "Further Investigations of North Sea Surges," *Association d'Oceanographie Physique, Proces-Verbaux* 5(1952); Robert Henry Corkan, "Storm Surges: Their Importance in Modern Tidal Science and Some Results of a Recent Investigation," *The Dock and Harbour Authority* (1948); Arthur Thomas Doodson, "Storm-Surges," *Hydrog. Rev., Monaco* 24(1947). Corkan was Doodson's assistant.
[44] Warburg to Simpson, 5th Mar 1928, AIR 2/331, NA
[45] LOTI, *Annual Report 1953* (Liverpool: University of Liverpool, 1953), 5.
[46] Doodson, special section on storm surges, Personal Information File, RS
[47] The spaces between the hyphen and the surrounding words are as in the original. Doodson to Dines, 31st Jul 1933, BJ 5/22, NA. The engineer who wrote the article they were discussing, Mr Ryves, had not been involved with the 1928 inquiry.

and much of his proposed research involved repeating for the Thames work that had been done for Liverpool, for example investigating the statistical correlations between pressure gradients over the sea and residuals in tidal records for Southend. His proposed work thus relied on him being able to use his earlier methods of calculations, for example defining meteorological effects as residuals. In order to do the work TI had been given funding to do, and thus justify this funding, the event had to be defined in a way that made it amenable to Doodson's statistical calculations. However, as we turn to next, the definition of the event used by the investigation became a contested issue.

5.4 THE CONTESTED DEFINITION OF THE INVESTIGATION'S OBJECT OF RESEARCH

Initially the definition of the topic used by the investigation was different from that used by Doodson in his proposal, with PLA producing a definition in the form of a list of "abnormally high high water tides" by late January.[48] PLA's Chief Harbour Master E C Shankland defined these abnormally high high water levels as those that reached 3'6" above Trinity High Water.[49] Throughout February the Met Office worked with the material they had been sent from PLA, which included PLA's list of abnormally high high water tides and tidal data for these high tides.[50] Using this information the Met Office produced data which Shankland used to support his own findings.[51] PLA's list was also used by the Thames Conservancy, which created lists of river flow for the dates on it.[52] In other words, Shankland's list of abnormally high high water tides was coming to dominate the work of the investigation. It was becoming the definition of what the investigation was researching.

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[48] Despite its awkward length this actor's term has been kept, as there is no shorter way of putting it without comprising the meaning.

[49] For sea levels in tidal areas, including tidal rivers like the Thames, there are many possible datums, generally defined as "a base elevation used as a reference from which to reckon heights or depths", see NOAA, "Tides & Currents, Tidal Datum," http://tidesandcurrents.noaa.gov/datum_options.html, last accessed 9th Dec 2010. Trinity High Water was a particular datum used to measure the height of water in the Thames. While there may have been discussions in the engineering community regarding exactly how to relate Trinity High Water to other datums, such as the Ordnance Datum (Newlyn), following the Second Geodetic Levelling (1912-1921), the definition appears not to have been at issue or questioned within the context of the 1928 flood event. W.B. Hall, "Abstract: The Origin and History of Trinity High Water," *Journal of the ICE* 21, no. 1 (1943).

[50] Shankland to Met Office (no named person), 2nd Feb 1928, AIR 2/331, NA

[51] Shankland to Doodson, 7th Mar 1928, Box 16, BA

[52] Simpson to Swarbrick, 19th Feb1928, AIR 2/331, NA

TI could not produce their planned work using PLA's list, as it did not define the cause of the event as a surge or a residual. PLA's list of abnormally high high water tides focused solely on unusual heights at spring tides, the time of astronomical high tides, providing numerical information about the height of the tide at these specific times.[53] TI's methods relied on defining surges as residuals, whatever the stage of the tide, and correlating these residuals with meteorological data. If they wanted to use their established methods they had to replace PLA's list, by producing their own list and by stopping PLA's from being used. They did this through numerical and scientific work as well as rhetorical displays of expert knowledge.

Underlying TI's work was a dense correspondence network which started with their existing links with the Hydrographic Department but quickly widened to PLA and the Met Office, as well as to other organisations, such as LCC, the Ministry of Health and a wide range of port authorities and foreign authorities. This correspondence network was used to exchange data and information for the investigation, but such exchanges of information, especially between key organisations involved in it such as TI, the Met Office and PLA, were also key in establishing who was expert at what. While the investigation was quite clearly led by the Hydrographic Department, the responsibilities of PLA, the Met Office and TI, were not very clearly defined.[54] To be able to do work Doodson had to convince the others that TI's definition of surges instead of PLA's should be used and more generally that TI should be seen as the expert on storm surge science. He attempted to do this through displays of such expertise in correspondence.

For example, one of TI's contacts at the Met Office, Director Simpson, early on sent Doodson a copy of an "interesting and useful" minute by one of his colleagues, which quoted papers from the 1890s by WH Wheeler (whose work was briefly mentioned in chapter three).[55] In response, Doodson two days later sent Simpson a list of 61 papers written since 1909 in several different languages on meteorological effects and pointed

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[53] "Abnormally high (3'6" above T.H.W. or over) high water tides in the River Thames", Box 16, BA. Also attached to Shankland to Met Office (no named person), 2nd Feb 1928, AIR 2/331, NA

[54] Warburg to Doodson, 3rd Mar 1928, Box 16, BA

[55] Simpson to Doodson, 11th Feb 1928, Box 16, BA. Simpson is likely to have been the Director TI was in contact with during the early forecasting attempts for Liverpool, so a personal relationship was already established. Wheeler had published on tides and been involved in committee's on tides by the British Association for the Advancement of Science, see W.H. Wheeler, *A Practical Manual of Tides and Waves* (London: Longmans, Green, and Co., 1906); Wheeler, "The Effect of Wind and Atmospheric Pressure on the Tides."

out that "[a] great deal has been done since Wheeler's time and some of his conclusions are not generally held".[56] Several of the papers on the list were by Doodson and Proudman, so it displayed their work in this area. Being able to quickly produce a list like this was a polite way of showing the extent of TI's expertise in tidal science. It showed they had a large specialised library, which was considerably more up-to date than the Met Office's in this area. Doodson also showed his personal expertise in storm surge science by commenting on what the problem with Wheeler's work had been.

A similar exchange regarding who was the investigation's expert on surge science took place early on with Doodson's contact at PLA, engineer E C Shankland. He sent TI information on the potential effects of dredging and well water on the flooding, and had "pleasure in forwarding a slight contribution of my own on this subject [the effect of barometric pressure on sea level] from the proceedings of the Royal Meteorological Society", which he thought offered good leads for future researchers.[57] Doodson in return sent Shankland a copy of one of his own papers on meteorological effects from a few years earlier, pointing out that this work included both wind and barometric pressure, which was what he intended to do for London, but also that his own work, as presented in the paper, needed development away from a statical view of the phenomena towards a dynamical.[58]

Doodson had to keep Shankland on side, as he was the keeper of crucial tidal gauge information to which TI needed access in order to produce his own list of events. On the other hand, Doodson also needed to show his expertise in order to establish a claim on defining the topic of the investigation in such a way that he could do work. By sending his paper to Shankland Doodson attempted to establish his and TI's expertise in storm surge science, by pointing out that his work already covered more phenomena than Shankland's did and that he intended to cover even more when moving towards a dynamical understanding of the effects. While Shankland allowed Doodson access to the crucial tidal information, sending him what he requested (e.g. tidal observations from the night of the flooding, a tidal diagram of the Thames and a PLA Engineering Handbook), his letters grew colder in tone. He displayed his engineering expertise as Doodson had earlier shown his expertise in surge science, by, for example, informing

[56] Doodson to Simpson, 13th Feb 1928, AIR 2/331, NA
[57] Shankland to Doodson, 7th Feb 1928, Box 16, BA
[58] Doodson to Shankland,10th Feb 1928, Box 16, BA

Doodson that the plotting of some meteorological information "diagrammatically proves that [his] conclusions were correct with regard to the lowering of the low and high water levels of mean springs at London Bridge".[59] Shankland responded to Doodson's displays of expertise by mirroring this behaviour, displaying his own expertise.

PLA, the Met Office and TI contested between themselves who should become the investigation's expert or obligatory passage point, as Bruno Latour has called it, on meteorological effects on the tides. For TI this contest was also about proving that they deserved the patronage they received from LCC and PLA and also from their contacts at Hydro. The three organisations tried to trump each other's claims by showing connections. TI for example showed connections to earlier work by themselves and others, to libraries, different phenomena and to theory. Latour has argued that the strength of a scientific claim depends on the number of collaborative associations it has with others – the more connections an entity has the more real, or strong, it is.[60] In this case, the more connections that were displayed, e.g. through Doodson's long list of references, the stronger the contested claim for expert status was, and in extension the claims for having one's definitions of the event accepted. However, there was no clear 'winner' of the contest at this stage.

Following these initial jostlings for expert status, Doodson set out produce an alternative list of events and to convince the others to use this instead of PLA's. In correspondence with Simpson at the Met Office Doodson claimed that the PLA's definition of the investigation's research topic was wrong, as PLA had simply listed unusually high high waters, and that he doubted that some of PLA's high high waters were storm effects (i.e. surges) at all. Doodson claimed PLA's list was "misleading" as it did not take into account the predicted height of the tide.[61] He did not put it so bluntly to PLA's Shankland, the producer of PLA's list and the gatekeeper to the tidal records Doodson needed, but instead simply wrote to him that he needed more information in the form of the full tide-gauge records and hoped he would not be thought a "pest".[62]

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[59] Shankland to Doodson, 13th Feb, 21st Feb & 7th Mar 1928, Box 16, BA
[60] Latour, *Pandora's Hope*, 158; Latour, *Science in Action*.
[61] Doodson to Simpson, 7th Mar 1928, Box 16, BA and Doodson to Simpson, 7th Mar 1928, AIR 2/331, NA, with list attached.
[62] Doodson to Shankland, 18th Feb 1928, Box 16, BA

Not even the full tidal gauges were sufficient but Doodson had to request even more information (handbooks with predictions) from PLA to do his work.[63]

Once received, Doodson used PLA's records to produce a list of 62 storm surge events identified through a visual inspection of the tidal gauge records from 1915 until early 1928. This visual inspection was possible as "[s]uccessive curves overlap one another on the recording sheet and any abnormality is shown at a glance by a displacement of the curve from its normal position relative to its neighbours".[64] This quote also gives some insight into Doodson's practices in working with the tidal gauge records, showing how it depended on skills such as identifying what the normal position of the curve was. TI's initial list included any storm effects, whether they produced an increase or decrease in sea level and at any part of the tidal cycle, but only gave rough timings and notes on the event.[65] Defining the investigation's research object like TI did, as storm effects or surges, produced a different list of events that covered a wider variety of events within a wider time frame than PLA's.

But how was TI's list of storm effects received? Was their definition of the investigation's research object accepted? The key organisation to convince to use the list and TI's definition was the Met Office.[66]

Initially the researcher at the Met Office, J S Dines, Superintendent in the Forecast Division, had some problems in understanding Doodson's list and he had to explain his terminology and why he had not yet given them the exact times of his storm effects.[67] The Met Office needed these times to enable them to do their work, connecting surges to particular meteorological situations. Like Doodson needed surges defined as residuals

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[63] Shankland to Doodson, 2nd Mar 1928, Box 16, BA

[64] "Discussion on the cause of High Thames Floods held at the Meteorological Office at 10.30am on Friday, March 16th", Box 16, BA

[65] Doodson to Simpson, 7th Mar 1928, AIR 2/331, NA, with list attached. Doodson's initial "List of Storm Effects" was roughly hand written with various corrections on it and did not give exact details of the timing or heights but only dates and rough notes on what had happened when (e.g. H[igh] on 18th p.m. L[ow] on 19th)

[66] The Hydrographic Department already had close connections to TI and had brought them into the inquiry, so they were positively inclined towards TI's work from the beginning, whereas there was little chance of bringing PLA's Shankland over on TI's side, given that it was his list TI questioned. In addition, while PLA paid half the cost of the inquiry and was expected to help by providing data, they had not been designated by the Technical Sub-Committee as one of the organisations that was supposed to do the research work – those organisations were the Met Office, the Hydrographic Department and TI. Gibbon, "Report of a Committee Appointed at a Conference of Public Authorities to Consider the Question of Floods ". There is no evidence of what PLA thought of TI's new list.

[67] For more on Dines see Crewe, "The Met Office Grows Up: In War and Peace."

to do his calculations, the Met Office needed more exact timings of the events in order to do their work. Doodson claimed it was "almost impossible to give the exact hour by inspection of the tide-gauge records; in the first place we have to estimate by eye what the tide would have been in due sequence, and secondly, we have to estimate the difference between this and the actual curves".[68]

This explanation was not sufficient for Dines and the others at the Met Office. A face to face explanation, or in other words an explanation of the tacit understandings that had not travelled with Doodson's list in the post,[69] was necessary. Before accepting TI's list of storm effects those involved at the Met Office (Dines, Simpson and Ernest Gold, Assistant Director) arranged a meeting with Doodson. At the meeting he explained that the timings the Met Office's researchers needed took longer to produce but that TI were working on it. As part of an exchange of meteorological and tidal data, Doodson agreed to send copies of "curves of hourly departure of observed minus predicted" tides, i.e. curves of residuals, as soon as they were finished, with times and heights of high water, which was what the Met Office needed. In return they would provide TI with meteorological data for the times identified by TI, so that both could work on formulating what meteorological conditions were responsible for flooding. At this meeting and through correspondence the work to establish who was expert at what and who should do what kind of research continued, with a boundary established so that TI concentrated on statistical correlations (numerical work) while the Met Office would analyse synoptic charts (less numerical, more graphical work).[70]

While Dines and Simpson had not yet completely accepted TI's list of storm effects as the investigation's main research object, the March meeting led to them agreeing to try to do work with TI's list once they had been given more information. The organisations involved with the work (including the Hydrographic Department) met face to face a couple of times while TI and the Met Office exchanged documents frequently over the next couple of months. Doodson sent the Met Office the required "curves of departures" as they were produced while they sent him tidal and meteorological

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[68] Doodson to Dines, 23rd Mar 1928, AIR 2/331, NA
[69] Collins, *Changing Order : Replication and Induction in Scientific Practice*.
[70] "Discussion on the cause of High Thames Floods held at the Meteorological Office at 10.30am on Friday, March 16th", Box 16, BA

information.[71] If the Met Office had previously worked with PLA's high high waters, using their list, they had now moved towards working with TI's surges. While TI's list had not yet been definitely accepted as the definition of events studied by the investigation, TI had negotiated a position whereby they could do the work they had planned, using their definition of surges as residual. Before discussing in more details how and why the Met Office worked with TI's list of surges, I turn to TI's practices of constructing surges.

5.5 The construction of surges at TI

This section looks at how TI constructed the "curves of departures", i.e., curves of residuals, that gave the detailed timings the Met Office's researchers said they needed to do their work. These curves were developments of TI's earlier work and were part of the work they had suggested in their proposal. This section discusses in some detail how TI constructed surges out of the tidal information that Doodson so laboriously had extracted from PLA and also had sent to him from other organisations. It will first look at the data TI used, how it acquired it and interpreted it. It then turns to TI's repeated re-inscribing of this data as other numbers and graphs, concentrating on their practices of calculating residuals through two different methods.

TI's construction of surges used observations of tides, which came to them through a network of correspondents at PLA, various other port and harbour authorities and other organisations dealing with tidal data, such as foreign meteorological offices. The observations came to TI in various formats: sometimes as raw tidal gauge graphs or specifically tailored graphs, sometimes as tables with information on the hourly height of tides. Much of this information, especially the raw tidal gauge data, was only lent to TI and had to be returned, often as soon as possible so that somebody else could borrow it. The documents were complicated to send back and forth, often large and heavy, for example scrolls of tidal gauge data perhaps like the one Mrs Dennis is working with in figure 3.1. Gaining access to data was hard work. Even without dealing with the sort of antagonistic problems that arose with PLA getting data to TI took

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[71] "Discussion on the cause of High Thames Floods held at the Meteorological Office at 10.30am on Friday, March 16th" and Dines to Doodson, 11th & 28th Apr 1928, Box 16, BA

much work: letter writing (in duplicates, sometimes typed), organising for packages being sent to and picked up from the railway station and making sure there was space at TI to store and process the documents, not to mention all the work by others involved in producing the documents in the first place.[72]

Apart from the tidal documents, TI also received specialised documents for this particular investigation, such as a table with the daily flow of water at Teddington Weir from Thames Conservancy to investigate the influence of inland fluvial flooding on the event in London, meteorological information from the Met Office and various information on datums and levels for the Thames from PLA and LCC. With these documents came instructions on how to use them. For example, when the Thames Conservancy sent its information on river flow, it also pointed out that an additional 5.5% was added by tributaries after Teddington Weir before the river reached London Bridge, which, it was claimed, needed to be borne in mind when calculations were made.[73] Sometimes further instructions were necessary, as when the Met Office asked Doodson to clarify what H and L in the list of storm effects he had sent them meant.[74] As we saw above Doodson's written reply to this question was not sufficient, but he had to visit to discuss the matter further, so the documents and instructions were not enough by themselves – written documents were not sufficient for TI's methods to travel. The other organisations involved with the investigation were also part of similar correspondence networks and received large amounts of data too, sometimes the same data but also different material.[75] While TI is the centre of calculation I am concentrating on here, there were others for this investigation, such as the Met Office.

As an example of the tidal information they worked with I include a Danish example with data from the gauge at Esbjerg (see Figure 5.1). TI rarely kept the tidal data they used so this is quite a rare specimen in their archive.[76] While this document had been specifically prepared for them to keep, it reflects one of the many types of tidal data they worked with. When it arrived at TI it included not only data on the hourly height

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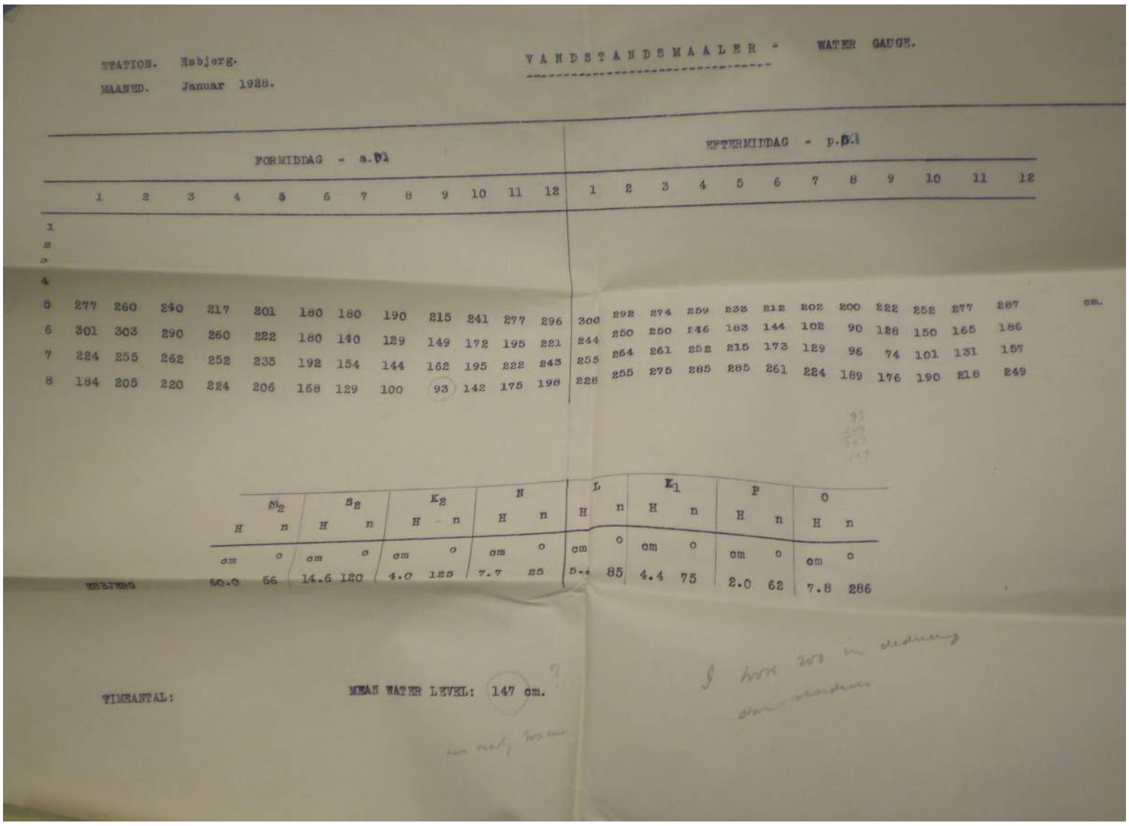
[72] Some of the work involved with establishing gauges and using the data they produced in the nineteenth century has been discussed by Reidy, *Tides of History*.
[73] [Swarhuik?] at Thames Conservancy to Doodson, 10th Mar 1928, Box 16 BA
[74] Dines to Doodson, 22nd Mar 1928, Box 16, BA
[75] For examples of the data the Met Office received, see AIR 2/331, NA
[76] However, TI did keep much of the correspondence regarding the tidal information they were sent. These letters provide much of the information on what they were sent and the work involved on which this section is based. See e.g. box 120, BA

of the water (potentially requiring conversion into other units as it was in cm) but also some information on the mean water level and on harmonic constituents for Esbjerg.[77] These harmonic constants could be used to calculate the predicted tides, though how close to observations such predictions would be even under calm weather conditions would have been difficult to know, especially as Esbjerg is a shallow water port where harmonic predictions were problematic and TI was only given a few constants. The handwritten comments near the mean water level questions the information given to TI by the Danes, claiming the mean water level was in fact closer to 200cm and informing us that the writer used this value in their work. In other words, a trace of the decision on what datum to use in the calculations was written on the document, which was a way to keep track of such decisions for future reference.[78] Such problems with the mean water level or other datum were common, with TI frequently writing back to the senders of the tidal gauge data, asking for further information clarifying the datum to which the information should be referred.[79] Archiving such letters was another way of keeping track of such information. Establishing how to interpret their data took much work – deciding what was zero and how to compare that zero with other zeros was an important part of TI's construction of surges. Such work took both care, as when checking the Danish information, and effort, in terms of letter writing or double-checking TI's own documents or calculations.

[77] The harmonic constituents are in the table in the middle of the page, with the top row starting M2, S2 and K2.

[78] Folder "Observations", Box 16, BA

[79] For another example of such problems from this inquiry, see Shankland to Doodson, 22nd May 1928, Box 16, BA



STATION. Esbjerg.
MAANED. Januar 1928.

VANDSTANDSMAALER - WATER GAUGE.

FORMIDDAG - a.m.

	1	2	3	4	5	6	7	8	9	10	11	12
1												
2												
3												
4												
5	277	260	240	217	201	180	180	190	215	241	277	296
6	301	303	290	260	222	180	140	129	149	172	195	221
7	224	255	262	252	235	192	154	144	162	195	222	243
8	184	205	220	224	206	168	129	100	93	142	175	198

EFTERMIDDAG - p.m.

	1	2	3	4	5	6	7	8	9	10	11	12	
5	300	292	274	259	233	212	202	200	222	252	277	287	cm.
6	244	250	250	146	183	144	102	90	128	150	165	186	
7	255	264	261	252	215	173	129	96	74	101	131	157	
8	228	255	275	285	285	261	224	189	176	190	218	249	

	M_2		S_2		K_2		N		L		K_1		P		O	
	H	n	H	n	H	n	H	n	H	n	H	n	H	n	H	n
	cm	o	cm	o	cm	o	cm	o	cm	o	cm	o	cm	o	cm	o
[illegible]	60.0	56	14.6	120	4.0	125	7.7	25	[illegible]	85	4.4	75	2.0	62	7.8	286

TIMEANTAL:

MEAN WATER LEVEL: 147 cm.

Figure 5.1: Tidal gauge data from Esbjerg[80]

Having received documents and decided how to interpret them, TI started re-inscribing the data. Their work on storm surges concentrated heavily on constructing surges as residuals, or the difference between tidal observations and what the tide would have been without the meteorological effect. They did this construction through computational and graphical work, constituting surges as graphs and numbers that calculations could be done with. For a place like Southend, where TI had predictions they trusted, the residual was further defined as observations minus predictions (shortened as obs-preds or 'O-P'). TI's definition of surges was closely linked to how they re-inscribed the tidal data. To calculate these residuals they wrote down numbers taken from the observations and other numbers from predictions on a special document (see Figure 5.2). These documents brought the observations and predictions together in a particular format which made the calculation of the residual more

[80] Folder "Storm surges 1914-1928", Box 16, BA

straightforward and more comparable across time and space.[81] The document had lines for observations, predictions and residuals, and each part will now be discussed in turn.

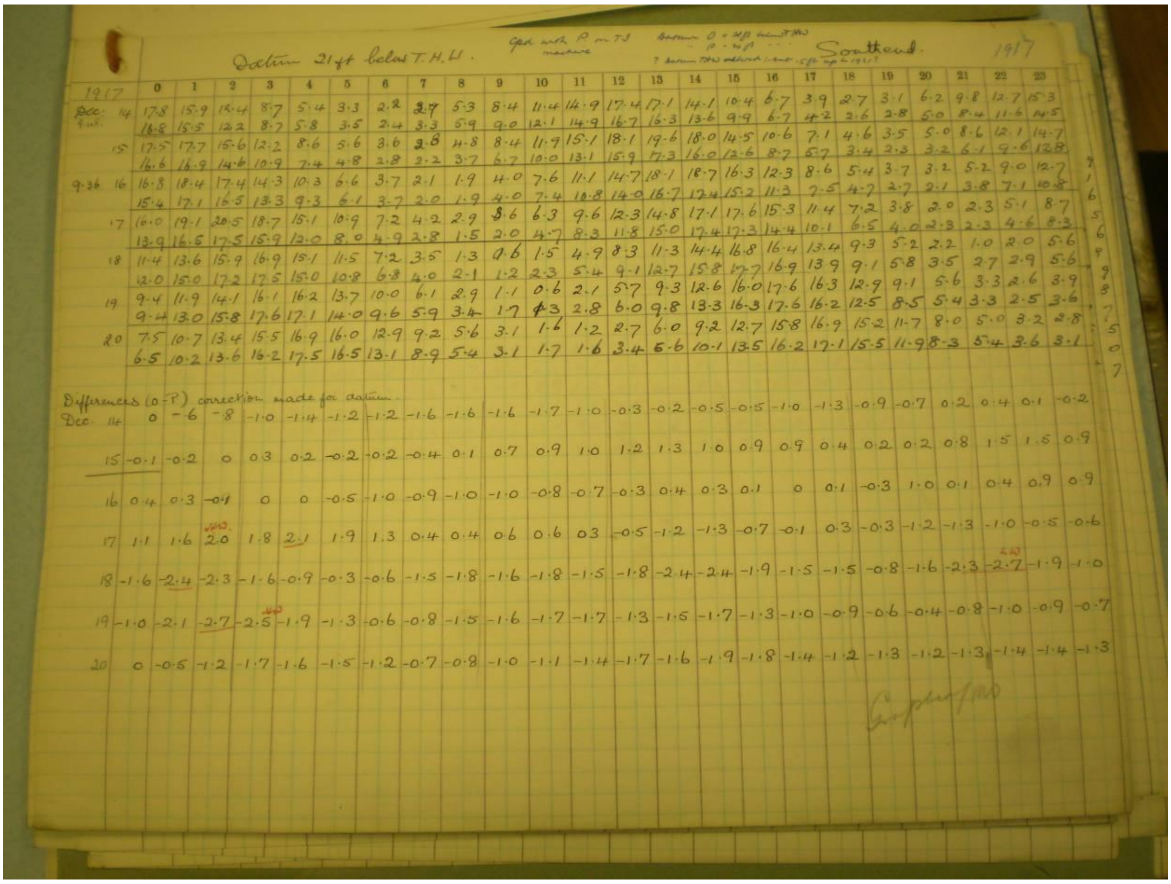


Figure 5.2: A table with observations, predictions and the difference between them: the residual[82]

The observations were extracted from the documents which had been sent to TI, while the predictions had to be specially made in-house. Re-inscribing the observational data could mean turning graphical data into numbers, or one number into another (for example, converting height of tide in cm into feet).[83] Because of the bulky nature of most of the tidal records, and the fact it had to be returned to the sender, TI needed to be sure they extracted the information they wanted correctly the first time round. This

[81] Folder "Storm surges 1914-1928", Box 16, BA
[82] Folder "Storm surges 1914-1928", Box 16, BA
[83] At TI both ink and pencil were used, unlike in Whittaker's mathematical laboratory in Glasgow. Warwick, "The Laboratory of Theory," 340.

was reflected in their practices for checking their work, which can be seen through the marks left by this practice, such as ticks, corrections and notes.

Published predictions usually only gave the times and heights of high and low water, but to do their 'O-P' calculations TI needed hourly predictions, so published ones did not give enough details. As discussed in the previous chapter, by this stage producing periodic tidal predictions was increasingly becoming TI's bread-and-butter work. In 1928 they were producing predictions for 51 ports plus a number of almanacs.[84] Where they had access to the necessary harmonic constituents, for example at Southend, TI used their existing methods and tidal predictor machines to produce the predictions for their calculations of residuals.

However, for many places, such as Dunbar, no analysis of the harmonic constituents of the tide existed, so TI could not use its existing methods to produce tidal predictions, as these methods relied on knowing such constituents. For such places TI developed a new method of separating the meteorological effects from periodic tides without having detailed tidal predictions. This method combined graphical and numerical techniques to calculate the residual and required hourly tidal observation data for six days.[85] This tidal data was then manipulated by TI's workers to construct graphs like the one for Dunbar in Figure 5.3, out of which they then extracted residuals.[86] For a clear example see figure 5.4.[87] In these figures the curved lines represent observations, whereas the faint smooth lines in the middle represent the unperturbed tide, which was the method's estimation of the periodic or astronomic tide. Each graph contained information for four hours per day, at six-hourly intervals, e.g. at 1am, 7am, 1pm and 7pm, across at least six days. As each curve gave information for four hours, together the six curves in Figure 5.3 gave information on the surge for all 24 hours for these six days. Within one graph, the data points for one set of twelve hourly intervals, say 1am and 1pm, were drawn in thick pen and represented observations in feet, translated into a suitable scale. The values for the data points on the broken line represented the other twelve-hourly interval, i.e. 7am

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[84] Liverpool Tidal Institute, *Tidal Institute: Ninth Annual Report, 1928*.
[85] Arthur Thomas Doodson, "Report on Thames Floods," in *Meteorological Office Geophysical Memoir no. 47*, Geophysical Memoirs (London1929), Appendix II.
[86] Folder "Storm surges 1914-1928", Box 16, BA. In order to understand this method I had to refer both to Doodson's account of it and the graphs, as well as testing it out myself.
[87] This example was sent to LCC but it was not included in the printed report. Illustrative example for Dunbar method, from "Report of an Investigation of Floods in the Thames, by A.T. Doodson", 31st May 1928, LCC/CL/MD/1/11, LMA

and 7pm. This second set was not direct observations but were instead the observation for that hour subtracted from twice the mean sea level, both in feet.

The person at TI creating these graphs will have been referring to tidal observations, probably already reduced to the format in Figure 5.2, i.e. tidal observations re-inscribed as a line of numbers of hourly observations of tidal height. They then used some method of keeping track of which line they were referring to, perhaps by folding the document diagonally by the hour or by the use of a ruler or other indicator. Sometimes these documents in the Bidston archive have sharp diagonal creases indicating folding might have been one way of keeping track of where the worker was, which was particularly important because of the six-hourly jumping about in the data and the potential use of calculating machines to produce the data points for the broken line. If the computer had pen in one hand and were operating the calculating machine with the other (or indeed both hands), they must have kept track of their whereabouts in the numbers in a way that did not involve another hand. Once the data points had been marked, the worker connected them into a curve. While the graphs in the examples here are quite angular, many other of these graphs have been drawn as smooth curves, requiring particular skills of curve-drawing.

If there was no meteorological effect the two curved lines would be identical when constructed like this, due to a particular numerical relationship valid for semi-diurnal tides. Semi-diurnal tides are common around the British coast and on average have two highs and two lows each day, i.e. extreme water levels take place at roughly six-hourly intervals, hence the six-hourly intervals of the data in the graphs. When there was a meteorological effect, the 'unperturbed' (periodic) tide could, according to Doodson, be taken as the pencilled line drawn at the mean of the two curved lines as the numerical relationship between different phases of semi-diurnal tides cancelled out the meteorological effects during the different phases. The workers at TI visually 'calculated' the mean of the two curved lines and drew this representation of the unperturbed tide as a smooth curve through the points where the curved lines crossed. The residual was then extracted by reading off the distance between the curve for unperturbed tide and the actual observations using a small paper scale. This difference between the data point on one of the curved lines and the curve for the unperturbed tid represented the residual, i.e. the surge, at the hour of that data point, with the residuals for the points on the broken line converted for sign. Once the residuals had been constructed this way, they could be re-inscribed onto tables and graphs like those produced through the obs-preds method used for Southend. Sometimes the graphs required adjustment for other, non-semi-diurnal parts of the tide, but Doodson claimed these parts easily could be smoothed out.[88]

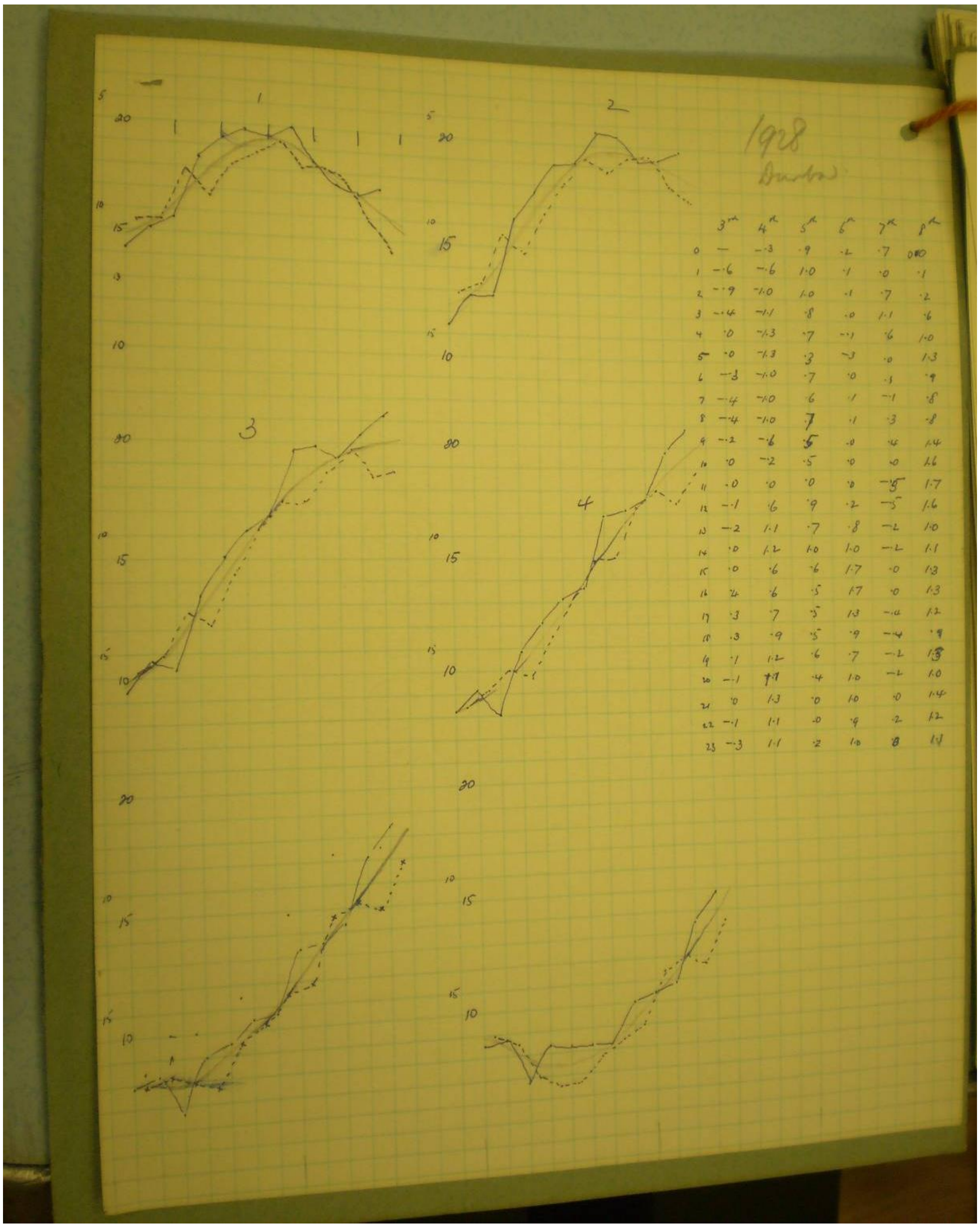

Figure 5.3: Graphs used to produce residuals when tidal predictions were unavailable. The curved lines represented observations, whereas the faint smooth line in the middle represented the unperturbed tide.[89]

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[88] Doodson, "Report on Thames Floods," 25.
[89] Folder "Storm surges 1914-1928", Box 16, BA.

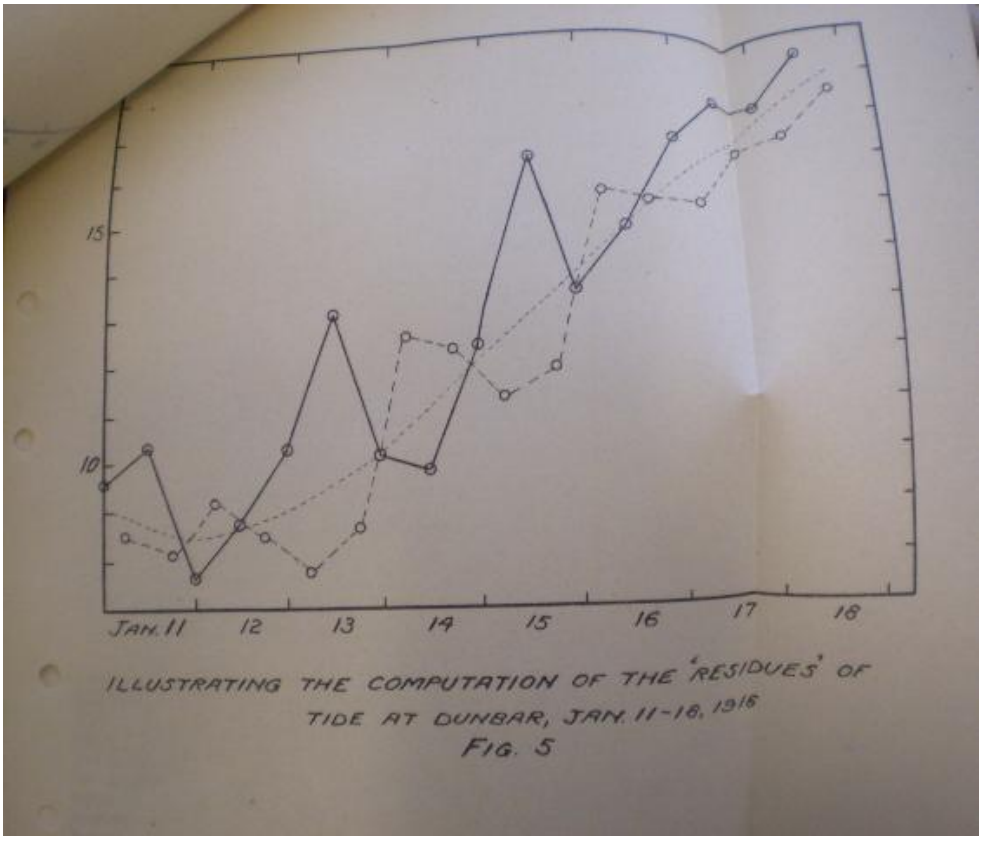

Figure 5.4: Illustrative example of the Dunbar method for calculating the residual when predictions were unavailable. This illustration was not included in the final report.[90]

This was a different way of constructing a surge, not as obs-preds but obs-'unperturbed' tide, though like the obs-preds method it emphasised the difference between observed values and the expected undisturbed values: the residual. For this method, however, the unperturbed values were constructed out of the observations themselves using a method specific for the purpose. Doodson deemed the direct O-P method more "exact", but thought this second method produced good results and would "be of further service".[91] Indeed, as will be discussed in later chapters TI continued to use this method into the 1950s and sometimes even used it for places where they had predictions (e.g. Southend). This was one solution to the problem that predicted and unperturbed tides were quite different things and not necessarily comparable.

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[90] "Report of an Investigation of Floods in the Thames", by A.T. Doodson", 31st May 1928, LCC/CL/MD/1/11, LMA
[91] Doodson, "Report on Thames Floods," 5.

The document illustrated in Figure 5.2 was a way of pulling together the numbers representing observations and predictions produced by TI in various ways. Reducing the data onto these documents enabled comparisons between different dates and places. These documents were what was kept and preserved by TI, once they had returned the often cumbersome documents with tidal information to their contacts. The documents were light, compact at about half a (modern) A4, and crammed full with information that could be (and were) stored for many years before being repeatedly re-used for different investigations. The production of these rows of numbers, carefully checked and neatly written, involved a range of practices of calculation – some routine, like the use of tidal prediction machines, others new, like the new graphical method of separating out surges when TI did not have the necessary information to make tidal predictions.

The example document in Figure 5.2 lists, hour by hour, the height of observations and predictions for Southend 14th to 20th December 1917. In this case, as a note at the top tells us, the predictions were produced on TI's tidal predictor machine, i.e. TI had harmonic constants available that they trusted enough to produce detailed, hour-by-hour, predictions, unlike for Dunbar. The second, lower, table, is of the differences between the observations and calculations, but in this case this was not a simple subtraction. As is noted at the very top, there was a one foot difference in the datum for the observations and for the predictions, so the O-P (obs-preds) calculation had to be corrected for this. That the correction was done is noted above the O-P table. Again, TI kept track of decisions taken as part of the calculations by notes on these documents. These calculations are likely to have been done with the aid of one of the four calculating machines which TI had at this point.[92] Another note tells us that the residual was graphed by "MD", probably Miss Dodd, one of the Assistants at TI. The work was often divided up into specialised tasks given to different people, which is reflected in the number of different handwritings on some of the documents. Sub-dividing mathematical work like this was one of the skills Doodson had developed in London during the war and which he further developed at TI.

[92] Proudman to Roberts, 3rd Oct 1935, D/BO 3/3/1, MMM - North Street

Two of the graphs 'MD' constructed can be seen in Figure 5.5.[93] These are graphs of a storm surge according to TI's definition, constructed using its practices of graphical and numerical calculations. The graphs are representations of a surge in two places, as these were constructed by TI in 1928 as the residual of tidal gauge observation minus periodic tidal predictions for Southend and observation minus 'unperturbed' tide for Dunbar. Usually only one surge in one place was graphed on one page, but this case took the analysis one step further by comparing the surges at two places on one page. Through such comparisons Doodson concluded that surges travelled down the East Coast, in other words that it was not enough to study the Thames estuary to understand coastal flooding in London, but that this needed an investigation with a wider geographical range.

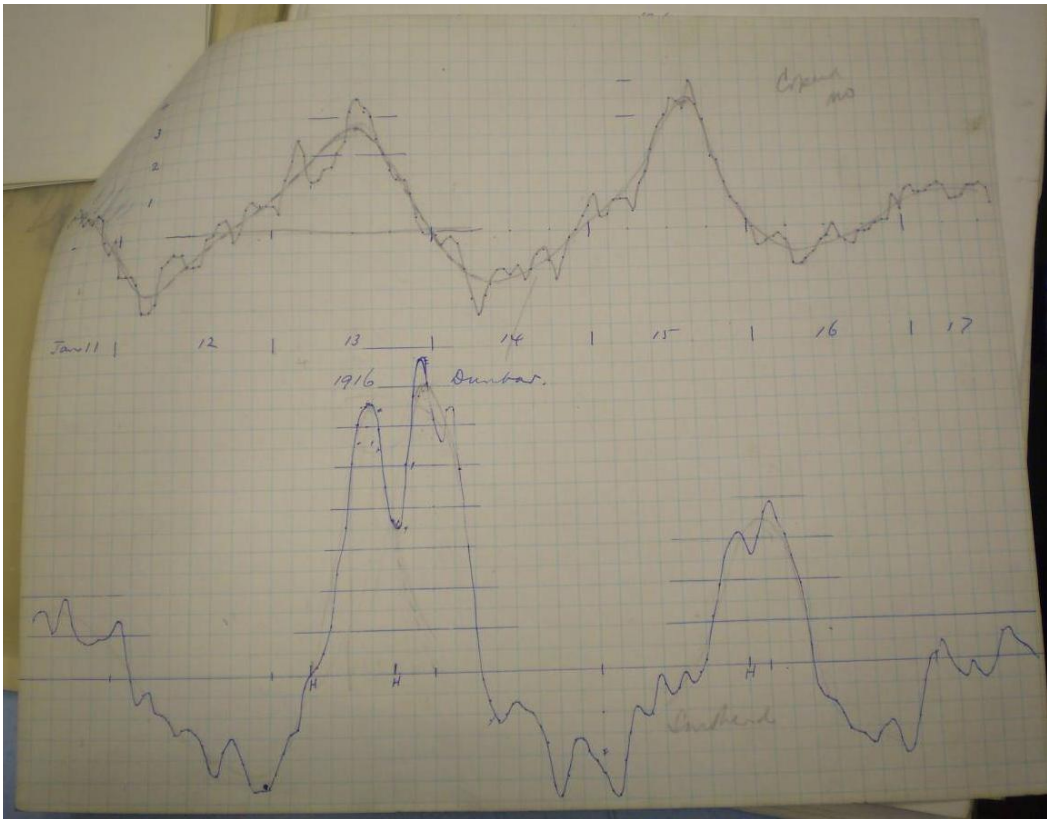

Figure 5.5: A document with the graph of the obs-preds residual for Southend at the bottom and the graph of the obs-'unperturbed' residual for Dunbar, January 11th to 17th 1916: a storm surge as constructed by TI for these two locations. Note the smoothing of the curve for Dunbar.[94]

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[93] Folder "Storm surges 1914-1928", Box 16, BA
[94] Box 15, BA

What TI's workers did to construct surges was to produce chains of documents. They took inscriptions of tidal observations and re-inscribed these, via intermediary steps, onto a standardised document, onto which they also put hourly predictions produced by TI. Then calculations were done on these numbers, with the results recorded on the same document and then finally represented graphically on another document. If tidal predictions did not exist a different method was employed, which used numbers from inscriptions of observations to produce graphs and numbers representing the residual, in this case defined as the difference between observations and 'unperturbed' tides. These ways of constructing surges remained similar at TI until the 1950s and the definition of a storm surge as the residual has been in use until recently at TI's successors, so these surges were what TI attempted to make predictable.[95] These graphs were TI's prime representations of storm surges, and the objects it offered the Met Office to work with. The graphs gave the residual, or surge, hour by hour, which meant the Met Office could extract the timings they needed from them. The graphs thus made TI's surges into objects the Met Office could calculate with. The residuals, whether in numerical or graphical format, was also what TI needed to do the work they had proposed and been given patronage to do.

Through these practices of calculation TI constituted surges as calculable, as entities that could be constructed through calculation and calculated with.[96] Through their constitution of surges TI picked a particular aspect of events such as the flooding in 1928 and structured them in particular ways, by defining them as residuals, naming them surges and constructing them through the particular practices of calculation described above. However, to constitute surges as calculable entities was not the same as making them predictable entities. We now turn to TI's attempts to make surges predictable as opposed to calculable.

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[95] A more recent definition is the skew surge, see Horsburgh and Wilson, "Tide-Surge Interaction and Its Role in the Distribution of Surge Residuals in the North Sea."
[96] Compare Kalthoff, "Practices of Calculation."; Lynch, "The Externalized Retina."

5.6 The use of the 'failed' attempts at producing a quantitative forecasting formula

When Doodson started to provide the Met Office with his detailed graphs with the timings of his surges, Dines and Simpson quickly accepted TI's designation of PLA's list as insufficient, as can be seen from the progress report Dines sent in early April to the Hydrographic Department. He there argued, just as Doodson had done, that the list of high tides PLA had provided them with during the early stages of the work had been "faulty in that only occasions of unusually high tide were noted, no account being taken of the predicted height". Another reason Dines gave for abandoning PLA's list was that results of early attempted forecasts based on PLA's definition of surges had been "very unsatisfactory" with only three out of 17 deemed "good" and six cases of "notably raised" tides were missed. No definition of good, unsatisfactory or notably raised was given. Dines wrote that in future work on forecasting, "the most favourable conditions" in terms of wind direction, and the time taken for the wind to produce the maximum effect, would be based on Doodson's "detailed curves" of surges, so the Met Office researchers had by now accepted to work with TI's definition of surges and its list of these.[97] They later claimed that TI's data was of "much greater precision and suitability for the task at hand" than the early data provided by PLA.[98] This shows that TI's definition of the event became the investigation's topic of investigation. The Met Office's researchers appear to have accepted TI's definition after TI combined displays of expertise and provision of graphs with timings.

After this both the Met Office and TI independently did work to analyse which meteorological conditions produced flooding, using different methodological approaches to the problem. The different approaches were aligned to common practices at the different institutions, with TI concentrating on mathematical work while the Met Office used a more synoptic approach.[99] The Met Office concentrated on the 17 cases identified by Doodson where the surge had led to an increase of water level of at least four feet. Out of TI's graphs Dines extracted a few specific pieces of data to work with: the time and height of the what he called extreme positive departure, i.e. the

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97 "High Thames Tides. Note on work done in the Forecast Division (To April 1928)", Box 16, BA
98 J.S. Dines, "Meteorological Conditions Associated with High Tides in the Thames," in *Meteorological Office Geophysical Memoir no. 47* (London1929), 29.
99 "Discussion on Thames Tides at the Meteorological Office, 10 a.m. April 27th 1928", Box 16, BA

time and level of the highest part of the surge, and the departure (i.e. TI's residual or surge) at the time of predicted high water. He inserted this information into his own documents and lists, added information about geostrophic wind[100] and the general weather situation, and then looked for patterns in this data. He looked in more detail at TI's curves for some cases, analysing them together with synoptic charts. Dines's work concentrated on synoptic analysis, though he did calculate means.[101]

Doodson's work on the other hand was much more statistical and numerical, including the creation of at least 150 correlation tables and other statistical calculations, the drawing of meteorological charts and of graphs of barometric pressure (sometimes under graphs of residuals, so correlated visually as well as numerically). The correlation tables linked meteorological conditions in various areas of the sea to the surge in the Thames estuary, as had been done for Liverpool. Doodson judged that the correlations for Southend were worse than those he had found in Liverpool, "indicating the greater complexity of tidal phenomena in Southend".[102] TI's construction of statistical formulae will be explored at in more detail in the next chapter.

However, their work did not produce the desired results of a flood forecasting formulae and both the Met Office and TI stated in their final reports to the Technical Sub-Committee that further work was necessary to be able to do forecasts of Thames flooding for the warning system. Doodson's conclusions were cautious and tentative: "The problem of forecasting the effects is very complicated and further investigation is required before the meteorological conditions can be specifically formulated in such a manner that forecasts can be prepared as a matter of routine".[103] His final multiple regression formula was said to provide "by no means a good representation of the variations of sea level", as the standard deviation of original residuals had only been

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[100] Geostrophic wind, which is defined in the list of technical terms, will be discussed further in the next two chapters.

[101] "Notes on discussion", unknown author 16th Mar 1928, Box 16, BA; "High Thames Tides. Note on work done in the Forecast Division (To April 1928)", Box 16, BA. Dines, "Meteorological Conditions Associated with High Tides in the Thames."

[102] "Investigation of Thames Floods. Report of Progress, 2nd Apr 1928", Box 16, BA. The largest correlation coefficient between mean sea-level at London Bridge found was -0.494, with what was called the East gradient taken over the Norwegian sea with a time lag of 19hrs, see Report on an investigation of floods in the Thames, Doodson, 31st May 1928, AIR 2/331, NA.

[103] "Summary of report on the Thames flood", Doodson, ca summer 1928, Box 16, BA

reduced by a small amount, to 0.866 times the original of 0.47ft, as opposed to halved for Liverpool during TI's earlier work.[104]

Like Doodson, Dines at the Met Office argued that further research was needed before forecasts of surges could be issued. However, the Met Office could provide a general rule of thumb describing under which meteorological conditions "disturbances in the water level amounting to 4 ft. or more" were likely, which was when a geostrophic wind of 60mph and over blew from the NW or N over the North Sea.[105] As the surges predicted by this rule tended to avoid high tides this rule would warn far too often, crying wolf, if used on its own as part of a warning system for *flooding*. However, these findings were used to slightly amend the emergency warning system. If the Met Office on the basis of their synoptic charts predicted geostrophic winds of 60 miles per hour from the NW or N over the North Sea, they issued a warning of this to the police at Scotland Yard, which then triggered a tidal watch as in the emergency warning system.[106] The warning system remained focused on the Met Office's synoptic wind forecasts and 'nowcasts' of tidal heights by tidal watchers for the next couple of decades with only minor modifications.[107] TI was not involved with this system and remained marginal to the day-to-day flood warning operation. The Met Office only forecast winds, not flooding or storm surges.[108] Thus, nobody was *forecasting* surges or coastal flooding in the UK in this period, though there was some warning of meteorological conditions favouring their development and if one took place it was supposed to be 'nowcast' down the East Coast.

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[104] Doodson, "Report on Thames Floods," 14.

[105] Dines, "Meteorological Conditions Associated with High Tides in the Thames," 35-39, quote from 39.

[106] "Superintendent's Instruction No. 791. Thames Flood Warnings," 27th Nov 1928, BJ 5/22, NA

[107] For practical details on the operation of the warning system from the point of view of LCC, the Metropolitan Police and the Met Office, see e.g. LCC/CL/MD/1/12, LCC/CL/MD/1/116 and LCC/CL/ESTAB/1/211 at LMA, and MEPO 2/6291, BJ 5/22 and BJ 5/181 at NA.

[108] This was emphasised in 1933 when Gold suggested they should synoptically forecast flooding. Simpson, who had been one of Doodson's main contact and who was in charge of the work, made it very clear he thought the Met Office were *not* issuing flood warnings, but warnings to the Police of "certain specified conditions regarding the wind over the North Sea". Indeed, he emphasised that the Technical Sub-Committee had concluded that "a meteorologist could not forecast floods" and that asking meteorologists to try do what the "specialists said they could not do" would be unfair to them. Simpson was using his status not only as boss but also as expert to keep the warning system operating as before. The flood warnings would instead continue to be done by others (the Police primarily) on the basis of measured water levels, i.e. the tidal gauge 'nowcasts' from the East Coast and Southend mentioned earlier. Minute 23 (1st Nov 1932) & 40 (7th Oct 1933), Gold, BJ 5/22, NA; Minute 24 (8th Nov 1932) & 42 (17th Oct 1933), Simpson, BJ 5/22, NA

Reports of the research by TI and the Met Office were sent to their patrons in London in early summer 1928, after which a meeting of the representatives from LCC, PLA, TI and Hydro on the Technical Sub-Committee was organised. The minutes of this meeting emphasised the problems with forecasting surges: "The probable occurrence of a storm surge can be predicted; as yet the time when its high water will reach the Thames estuary, or any other place, cannot". However, a further investigation could "possibly" lead to "the advance prediction of dangerous tides in particular places".[109] The problems with producing forecasts, together with some of the other findings of the research, were used to argue for a further investigation and thus for further funding. Aside from the question of improving the warning system, what the Conference into the event had wanted answered was what the causes of the flooding were and how often such events happened, to establish what the height of flood defences should be. TI's answers to these questions, and also the findings regarding tide-surge interaction, will now be briefly considered, as they are part of the background for demands for further inquiries into storm surges.

TI had made clear in their initial proposal that they thought a further, wider, investigation would be necessary to fully answer the questions that had been set by the Technical Sub-Committee and, rather unsurprisingly, they came to the same conclusion at the end of the initial investigation.[110] More work was said to be needed not only to improve the warning system but also to provide further information regarding the necessary height of flood defence. For this TI had worked out a number of different estimates for how often these events recurred. In his final report Doodson provided a range of answers, from a flood every 18 years to one every 1,700 years, using different methods to calculate the probabilities. On the basis of statistical and physical reasons he preferred the result of 1 in 60, but he made it clear that he considered this a quite uncertain answer. In terms of causes, Doodson had also looked at the impact of inland flooding on the event in London but had found that it was "not of primary

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[109] Emphasis in original. "Thames Floods: Technical Sub-Committee's Report", enclosure two to letter to LCC from Ministry of Health, 10th Aug 1928, LCC/CL/MD/1/11, LMA
[110] "Summary of report on the Thames flood", Doodson, summer 1928, Box 16, BA

importance".[111] His research came to a conclusion that could be used to downplay the arguments that had connected the event to the issue of land drainage.

A particular issue the research had raised was the problem of the timing of really large surges, such as an 11 feet surge Doodson had found. Could such a surge arrive at high tide? If it did it would cause much worse flooding than in 1928, when the surge had been about half that height. When discussing this Doodson emphasised what has become known as tide-surge interaction, which is that the effect of the wind is stronger when the water is less deep, i.e. the local wind effect is weaker at high tide and stronger at low tide.[112] If this holds, then an external surge from further afield will never be combined with a high local surge at high tide but only at low tide when the combination is unlikely to cause flooding. Doodson argued that this needed to be more fully investigated.[113] Such work would set physical limits to the highest possible storm surges, with important political and practical consequences, e.g. for the height, and thus cost, of coastal defences.[114]

Following the initial investigation members of the Technical Sub-Committee recommended that "the desirability of continued investigations should [...] be considered" and claimed that such an investigation would be of national interest, as storm surges affected a large area and travelled along the coast.[115] With this recommendation, the stage was set for different actors to ask for further research to be done on storm surges, with the research framed as of national interest and thus to be paid for by national government. The next chapter looks further at this.

5.7 Conclusion

The 1928 flooding event led to many changes in storm surge science at Liverpool, with patronage shifting from the shipping industry towards local government and a new

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111 The Met Office had not done much work on the recurrence issue or the role of inland water. Dines, "Meteorological Conditions Associated with High Tides in the Thames." Doodson, "Report on Thames Floods," 17.
112 Doodson, "Report on Thames Floods," 4.
113 Ibid., 11.
114 Work into tide-surge interaction continues to this day, see Horsburgh and Wilson, "Tide-Surge Interaction and Its Role in the Distribution of Surge Residuals in the North Sea."
115 "Thames Floods: Technical Sub-Committee's Report", enclosure two to letter dated 10/08/1928 from Ministry of Health, LCC/CL/MD/1/11, LMA

focus on flood forecasting. Their construction of surges was also to some extent new, such as the term surge and Doodson's graphical obs-undisturbed method of constructing residuals. At the same time TI's work also retained close links to what they had previously done for other patrons. Earlier its work on meteorological effects had been aimed at aiding shipping men get ships in and out of ports safely and economically, not at warning of potential floods, which was what TI's storm surge science was focused on from this event on. Doodson's earlier methods had to take into account meteorological effects that both increased and decreased sea levels, as both mattered to the shipping industry and to ports. The politicians based in London were concerned with flooding, and therefore increased sea levels. They were less concerned with decreased sea levels, but Doodson's usage of his earlier methods and concepts meant that the research they funded at TI dealt with both increases and decreases in sea levels. The Met Office, which had not done much work in this area previously, instead concentrated on increases only, so in contrast to TI's use of earlier methods and definitions they focused on only the aspect politicians were interested in. TI's whole approach to the investigation relied on developing their earlier methods, such as the definition of surges as residuals and the use of statistical correlations of such residuals with pressure gradients to produce forecasts, which is why they needed to convince others to use their definition of the event.

The chapter has looked at how TI constructed the meteorological effects they were trying to predict as calculable objects. They called these objects 'surges' and tried to convince others to use them. To TI, a surge was a meteorological effect, and also a graph on a piece of paper: the carefully and laboriously constructed residual after predictions or unperturbed tide had been subtracted from observations. For them sea level was connected to surges through chains of documents. Many different practices were involved in TI's construction of surges as the objects used by the investigation. These were not only those of calculation and mathematics but also of correspondence (in getting documents and information to TI), of social networking, building relationships with the Met Office and maintaining those with the Hydrographic Department, and of something which may perhaps be called diplomacy in dealing with PLA and London-based politics, e.g. by framing the investigation as locally focused, to get local funding. In order to construct their surges, TI was not only putting together documents from other documents but also putting together and maintaining relationships both with other researchers and with politicians. TI's researchers had to convince others that their particular centre of calculation should become the investigation's obligatory passage point regarding the definition of the events to be researched. They were sufficiently successful in this that the Met Office came to use TI's instead of PLA's definition of events. TI was also able use their definition of surges to do enough work to justify their funding and they got paid. However, they did not become involved in the operation of the warning system and the term surge was long used only by a few specialists. They were in the end only a partial passage point for storm surge-related work in Britain after this event.

The work done at TI with meteorological effects can be seen as a stereotypical example of how Latour describes science as the construction of chains of inscriptions and networks of connections. Their work used tidal gauge inscriptions – the height of the tide traced on a piece of paper by a machine, the tidal gauge – which were analysed at TI by making more and more inscriptions, often on specialised linked forms, culminating in attempted formulae or equations for forecasting.[116] In addition I have described how TI built a network of connections, at times through contests with other actors to define what the investigation should research.[117] It was a real struggle for TI to hold their connections together to do their work and they nearly did not. TI might not have been able to convince the Met Office to use their list of storm effects, which could have been the end of TI's involvement with the investigation and their local authority patronage. As such my narrative has emphasised the vulnerability – not the power – of scientists in centres of calculation and the contingency of their chains of documents, something which I will develop further in the next chapter.

TI's practices of calculation constituted surges in a particular way, which made them calculable both for TI and the Met Office, but this was not enough to make them predictable. They could now be both calculated and be used to calculate with, but not forecast. TI's calculations did not, according to themselves, produce the desired result of a numerical forecasting formula. This further differentiates Herbert Kalthoff's process of making calculable objects. He implicitly separates the process of loan-giving into the constitution/calculation *of* and negotiation /calculation *with* credit proposals, but does not consider the possibility that it may not be sufficient to calculate *with* the

116 Latour, *Science in Action*, ch 6.
117 See e.g. Latour, *Pandora's Hope*, 90-91, 98-108.

constituted entity to get the desired result. This is because he studies an already established process through which credit proposals are negotiated and then accepted or rejected, i.e. the process has already been organised to produce particular results. TI's surges were being constituted as part a process that was being established, and studying their work sheds some light on the choices made as part of the establishment of such a process. Kalthoff is well aware of the contingent and historical nature of the process he studies but concentrates on how it runs once established.[118] Similarly, Michael Lynch relies on published sources so does not emphasise the contingent or historical aspects of how a particular specimen came to be constructed as calculable in a particular way.[119] In this thesis I instead want to highlight that the process of constituting surges as scientific objects was historically contingent. The next chapter will discuss some of the effects of TI's constricted choices in constituting meteorological effects as surges and how their particular chains of documents limited as well as connected them.

TI's research produced various results sufficiently valued by their patrons for TI to be paid. However, their key aim of producing numerical forecasts for surges was deemed unsuccessful, as it had been for Liverpool earlier on in the 1920s. This had two effects. Firstly, TI was cut off from the practical operation of the warning system, which came to be dominated by more qualitative synoptic forecasts by the Met Office together with 'nowcasts' of the height of tides. Secondly, the 'failure' of this smaller scale investigation opened up the possibility for TI and other actors, such as LCC, to ask for funding for further work to by suggesting this could lead to numerical forecasts. The next chapter will turn to these requests for funding and TI's continued work on producing forecasts.

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118 Kalthoff, "Practices of Calculation," 74.
119 Lynch, "The Externalized Retina."

Chapter 6 (1929-1952), The role of state patronage in oceanographic research

How did the patronage pattern of storm surge science develop before, during and after the Second World War, and how did these changes interact with TI's practices? This question can be divided into two: first, how did different parts of the state relate to TI's work, especially its work on storm surge forecasting, and second, how did TI's practices relate to the patronage received? By covering a longer time span than the other chapters, this chapter concentrates on longer term developments such as a gradual increase in state involvement in research – but only some parts of the state in some types of research. The chapter traces debates regarding various decisions concerning whether different parts of the state would provide TI and its storm surge research with patronage. It also discusses in some detail TI's practices of calculation when constructing forecasting formulae, focusing on how various contingencies influenced this project.

After the initial investigation into storm surges in 1928 further work on forecasting of storm surges was sought after by some actors, such as TI, London County Council (LCC) and the Hydrographic Department (Hydro). However, the funding of the project became tangled up in wider debates over flood defence and the role of the central government in flood defence, and it did not begin until 1938. Very little surge related work was done at TI before the commissioning of this research so the first part of this chapter follows debates between local and central government taking place in London regarding the funding of the proposed research project. This explores which parts of the state were interested in surge science at this time. Central government repeatedly refused to fund the research project as they did not want to get involved with flood defence and instead local government commissioned the project, showing how only specific parts of the state were interested in this specific type of research at this time.

Once they had commissioned TI to do the research in 1938, LCC took a hands-off approach to the project. Apart from providing occasional progress updates to their southern funders, TI got on with the work without much involvement from them. The chapter briefly returns to TI, looking at the initial phase of the project. However, soon TI's work on the project was interrupted by the war when the key researcher on it went to work for the Navy. The war also led to interest in TI's storm surge work from the

UK and US Navies. Firstly, Hydro requested a storm surge forecasting method, which TI provided but which does not then appear to have been used. Secondly, the project's final report was literally appropriated by the US authorities, who published it in a very limited edition in the US. While the UK and US Navies had some interest in the project it was partial and they did not provide funding for it.

The chapter then shifts focus and analytic register towards TI's practices and their relation to TI's patronage. The naval interest in storm surge science had a definitive effect on it. At first TI's researchers experimented with a Laplacian approach towards forecasting surges, but they encountered some problems with this approach, in part related to the way they chose to calculate winds. Then, in response to Hydro's request, TI's researchers changed back to more statistical methods of forecasting. This represented a shift between two different traditions of practice that were strong at TI: Laplacian theoretical work and statistics. I argue that the choices TI's researchers made were contingent on issues such as the Hydro request, and also consequential for the results they achieved. This section also discusses how one of TI's researchers constructed a forecasting formula by first taking the meteorological effects apart into constituent residuals and then putting them back together, and how TI defined success in relation to this formula.

The final section shifts back to debates regarding patronage, first briefly covering the publication of the final report of the project by the US Navy before discussing the establishment of the National Institute of Oceanography. Many have argued oceanography saw a dramatic increase in naval patronage after the war. While this also happened in the UK, this naval patronage went to a new Institute while TI's overall patronage structure stayed similar between 1929 and 1952. TI's type of oceanographic research was still not deemed of sufficient interest to state actors to warrant further support than was already given through the purchase of tidal predictions.

6.1.1 THE POLITICS OF FLOOD DEFENCE

Following the presentation of the reports of the research project set up after the 1928 flood event, a second report was written by LCC, PLA, TI and Hydro. Both then and later this was presented as a report of the Technical Sub-Committee though this was formally not the case as the sub-committee had already been disbanded, but this lent the report more authority than it actually had.[1] The report recommended that "the desirability of continued investigations should [...] be considered".[2] It also recommended that such continued investigations should be paid for by central government, with a wider geographical focus aimed at forecasting storm surges. In addition the report claimed such an investigation would help to decide how high to build flood defence. Despite the strong sentiments voiced by various actors, such as Hydro, TI and LCC, in favour of the inquiry, it did not begin until a decade later, as it was repeatedly denied funding from the Treasury. The first section of this chapter follows these debates and the actions of those involved in it, as an example of debates regarding what central government's involvement should be in this type of research. I will first discuss the setting of this debate in wider debates between local, regional and national government regarding funding of flood defence and related measures, and then look at the specifics regarding the proposed storm surge project.

In the background of this debate whether central government should fund storm surge science was the depressed state of the national finance in this period, as well as the Treasury's general views on spending. According to George Peden, the Treasury's interwar view on central government spending on social services was underpinned by one general principle: that a community served by a service should contribute to it through the rates, before central government considered providing a matching grant.[3] The emphasis on flood defence as a local responsibility, mentioned in the previous chapter, echoes this same principle. In early 1928 the Treasury's principle was being questioned in relation to land drainage, closely related to flood defence. A Royal Commission on Land Drainage had reported just before the 1928 event, following bitter debates in the River Ouse catchment area regarding who should pay for 'improvements' to arterial drainage. Should those from upriver valleys help pay for improvements to drainage down in the Fens? 'Their' water caused floods in the Fens, but they would not see the benefit of works. Long-standing tradition had it that it was those who directly benefited from drainage works that should pay for them, but farmers in the Fens were unable to finance these expensive works. The Treasury was not keen in

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[1] Minutes by Warburg in June, H 3801/28, UKHO
[2] "Thames Floods: Technical Sub-Committee's Report", enclosure two to letter dated 10/08/1928 from Ministry of Health, LCC/CL/MD/1/11, LMA.
[3] George C. Peden, *British Economic and Social Policy: Lloyd George to Margaret Thatcher* (Oxford: Philip Allan Publishers, 1985), 117.

1928 to take on the increased financial responsibilities implied by the Commission's recommendations, but in 1930 the Land Drainage Act implemented them.[4] By then what is known as the 'Treasury view' – that central government spending funded by borrowing from the public was incapable of producing employment (as it would 'crowd out' private business) – had softened a little, but this view, and the principle of preserving balanced budgets remained strong.[5]

The Land Drainage Act broke some of the linkages between local benefit and payment, as well as increasing the Treasury's financial involvement in land drainage. In addition to the debates regarding who should fund land drainage projects, the whole system of local government finance was being restructured in 1928, with derating of agriculture, reduction in industrial rates, Poor Law reform and the introduction of a fuel tax.[6] Central government funding of local government in general and land drainage in particular was contested at this time.

However, these wider debates regarding land drainage and local authority funding were undertaken on the national level, while local issues were at least as, or more, important, as background for the debate on storm surge science. London had been given an exception from some of the reforms in the Land Drainage Act.[7] There was a history of disputes between the different layers of London government regarding who should pay for and control flood defence. An Act in 1879 had given the regional authority (LCC's predecessor) control of flood defence policy, but it was not prepared to pay for building the defences. After a long debate in the 1870s these costs remained a charge on riparian (riverside) landowners, in the face of protests from the smaller local authorities.[8] This meant that in 1928 LCC was responsible for setting the standard height of flood defences in London, though the riparian landowners were supposed to pay for the defences. Immediately following the event in 1928, LCC was given further

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[4] Bowers, "Inter-War Land Drainage and Policy in England and Wales."; Sheail, "Arterial Drainage in Inter-War England."

[5] George C. Peden, *The Treasury and British Public Policy, 1906-1959* (Oxford: Oxford University Press, 2000), ch 4&5, esp 222-223 and 268-229.

[6] Ibid., 209-211.

[7] Adjourned report of the General Purposes Committee, "Thames Floods Prevention – Report of Royal Commission on Land Drainage – Proposed Conference" in folder Thames Floods, 18th Mar, 1930, Council Minutes Cuttings, LCC/CL/GP/1/119

[8] *Report of the Metropolitan Board of Works for the Year 1877*, vol. (213), House of Commons Papers; Accounts and Papers, (1878); *Report of the Metropolitan Board of Works for the Year 1879*, vol. (212), House of Commons Papers; Accounts and Papers, (1880).

responsibilities related to the inspection of flood defences, but the overall framework had not changed.[9]

This history of disputes between different authorities was played out again following the 1928 event, when the question of how flood defence along the Thames should be co-ordinated was again raised and discussed by various conferences and committees until the mid-1930s.[10] The proposed investigation into storm surges was part of this wider debate on flood defence. The Departmental Committee favoured centralisation of the powers to manage flood defences to a joint committee hosted by one existing authority. For them the question was which authority should host it.[11] Several regional-level authorities, such as LCC, Port of London Authority (PLA) and the Thames Conservancy, wanted to host the co-ordinating powers. In the summer of 1934 LCC asked the Ministry of Health to make LCC the controlling authority.[12]

However, after the publication of the report of this committee in 1933 the Ministry was sent a number of letters from smaller riverside Boroughs, who felt the proposed committee threatened "local amenities" and asked for a number of added recommendations, including a right of appeal for local authorities against the decisions of the proposed committee.[13] Several other bodies, such as the Association of Public Wharfingers of the Port of London, various Commissioners of Sewers (old bodies traditionally responsible for flood defence and drainage), and several Catchment Boards (newer bodies set up through the Land Drainage Act), similarly complained to the ministries involved with the Departmental Committee about their lack of representation

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[9] *London County Council (General Powers) Act 1929 - Prevention of Floods.*

[10]For example, the Thames Valley Floods Conference, the Thames Floods Prevention Conference, and the Departmental Committee on Thames Flood Prevention. The different conferences and committees had slightly different names, membership and detailed questions for consideration, but they were all dealing with flood defence along the Thames and were closely related to each other. Materials on these are held in LMA (folders LCC/CL/MD/1/8, LCC/CL/MD/1/11, & LCC/CL/GP/1/119) and in the National Archive (e.g. folders HLG 51/39 and WORK 6/403).

[11] Like LCC, PLA insisted on becoming the controlling authority, see "Extract from Kew Gardens' File", 26 Mar 1931, WORK 6/403, NA. On the conclusions of the conferences see also Edwards to Davis, 15th Dec 1932. WORK 6/403, NA. "The Departmental Committee on Thames Flood Prevention," Joint Report by Clerk of the Council and Chief Engineer, 3rd Jun 1931, section 13, 14 and 16, LCC/CL/GP/1/119, LMA, "Thames Floods Prevention", General Purposes No. 947, Agenda No. 19, LCC/CL/MD/1/11, and Humphrey and Palmer, "On the Future Standard of Thames Floods Prevention Works in the County of London."

[12] "Report of the Departmental Committee on Thames Flood Prevention", Report of the General Purpose Committee, 16th Jul 1934, LCC/CL/GP/1/119, LMA

[13] For example, Town Clerk at the Borough of Brentford and Chiswick to The Secretary at Ministry of Health, 22nd Dec 1933, HLG 50/129, NA. Other authorities complaining in a very similar way included Richmond, Barnes and Twickenham.

on the proposed committee and the lack of protection of their interests.[14] After this campaign the Ministry of Health put the proposals low on their priorities. When LCC chased in 1936, Principal Secretary IF Armer stated internally that while the proposals had been included in the list of possible Ministry of Health bills since 1933 "pressure of other Bills has been such that inclusion has not become a live issue". Quoting a lack of requests from others than LCC for the proposals to be put into law, strong opposition by local authorities seaward of LCC and statements by LCC that strengthened flood defences had reduced the risk of flooding, Armer suggested that the matter be dropped, and this was agreed to by Assistant Secretary WA Ross.[15] To LCC, the Ministry responded that it was unable to do give LCC control because of the "diversity of opinion" among local authorities on the "principles to be embodied" by the legislation.[16] LCC complained, but the Ministry remained firm that it could not introduce the proposals to Parliament and the idea of a co-ordinating flood prevention authority petered out after this.

Garside, Gillespie and Clapson have argued that conflicts between different local authorities typified interwar London politics.[17] The failure to create a flood defence authority was typical of LCC-led attempts at further co-ordinating London government in the interwar period. When a co-ordinating body was actually created, e.g. dealing with transport or electricity, the LCC did not have a strong role on it. They instead tended to take the form of public corporations managed by independent experts and with a strong influence from private industry.[18] What LCC saw as rationalisation and coordinated planning was often resisted by other local authorities, e.g. the London boroughs and the nearby Counties. These resented what they saw as LCC's attempts at controlling them and extending its own powers. This problem of how to govern London was identified and discussed at the time, for example by William Robson, a Fabian protégé of the Webbs, who favoured a stronger regional-level authority, echoing Labour's calls in the

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[14] Documents in HLG 50/129, NA
[15] Minute by Armer to Ross, 3rd Feb 1936, HLG 50/129, NA
[16] Francis to the Clerk of the LCC, 24th Feb 1936, HLG 51/39, NA
[17] Ken Young and Patricia L. Garside, *Metropolitan London : Politics and Urban Change 1837-1981* (London: Edward Arnold, 1982); Mark Clapson, "Localism, the London Labour Party and the LCC between the Wars," in *Politics and the People of London: The London County Council, 1889-1965*, ed. Andrew Saint (London: The Hambledon Press, 1989).
[18] James Gillespie, "Municipalism, Monopoly and Management: The Demise of 'Socialism in One County', 1918-1933," in *Politics and the People of London: The London County Council, 1889-1965*, ed. Andrew Saint (London: The Hambledon Press, 1989).

previous chapter for a "real Corporation of London".[19] Flood defence and how it should be co-ordinated was part of a wider set of acrimonious power struggles between different local and regional authorities in London; power struggles that were not resolved. The storm surge science project also became part of such power struggles, as will now be discussed.

6.1.2 THE CONTESTED PATRONAGE OF STORM SURGE RESEARCH

During the debates on flood defence policy one of the few things the committees agreed on was the proposed storm surge research. In their various reports they repeatedly recommended that the second round of research should be undertaken and, crucially, that it should be paid for by central government.[20] LCC used these various recommendations, and especially the report of the (not quite) Technical Sub-Committee, to repeatedly ask the Ministry of Health to instigate this further investigation. Such requests for funding were sent to the Treasury in 1931, 32, 34 and 36.[21]

As Gail Savage's analysis of the policy of Ministry of Health officials has shown, it emphasised financial 'efficiency' over expensive social policies in the interwar period, just as the 'Treasury view' at this time emphasised balanced national budgets as outlined above, so it is unsurprising that both these departments resisted expenditure and in the end refused it.[22] Nobody, not LCC or the other local or regional authorities, the Ministry of Health, the Admiralty or the Treasury, wanted to spend their money on the

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[19] William A. Robson, *The Government and Misgovernment of London* (London: George Allen & Unwin Ltd, 1939). He did not discuss flood defence. For more on him see John Davis, "London's Evolution - from Parochialism to Global Metropolis," in *London Government - 50 Years of Debate : The Contribution of LSE's Greater London Group*, ed. Ben Kochan (London: LSE London, 2008); Michael Hebbert, "William Robson, the Herbert Commission and 'Greater London'," in *London Government - 50 Years of Debate : The Contribution of LSE's Greater London Group*, ed. Ben Kochan (London: LSE London, 2008).

[20] Humphrey and Palmer, "On the Future Standard of Thames Floods Prevention Works in the County of London," para 72; William Edward Hart, "Report of the Departmental Committee on Thames Flood Prevention," in *Command Papers, Reports of commissioners* (1933), para 19. For how LCC emphasised this part of the Departemental Committee Report, see Report of the General Purposes Committee, 16th July, 1934, Report of the Departmental Committee on Thames Flood Prevention, Section K, in folder Thames Floods, Council Minutes Cuttings, LCC/CL/GP/1/119

[21] LCC to the Secretary of the Ministry of Health, 15th Aug 1930, HLG 51/39, NA

[22] Savage, *The Social Construction of Expertise*; Peter Clarke, "The Treasury's Analytical Model of the British Economy between the Wars," in *The State and Economic Knowledge: The American and British Experiences*, ed. Mary O. Furner and Barry Supple (Cambridge: Cambridge University Press, 1990); Peden, *The Treasury and British Public Policy, 1906-1959* ; Peden, *British Economic and Social Policy*.

investigation. However, the rationale behind the refusals, which links the funding to general debates about the allocation of responsibility for flood defence, is what is interesting in this case. If land drainage, seen as a key part of flood defence, could now be given increased Treasury support following the Land Drainage Act in 1930, what about flood defence more generally, in particular research into it? What should central government's role in funding research of this kind be?

The initial proposals for research were jointly developed by LCC, PLA, TI and Hydro.[23] In 1931 TI and Hydro developed additional proposals for setting up a tidal gauge network and a centre of calculation for storm surges at TI. At this point Arthur Doodson made it clear that the aim of the work was to improve the warning system: "I take it that the question is not being reconsidered because of its general interest and importance from a scientific point of view, but rather for the evolution of an adequate forecasting service".[24] The project, as TI saw it, was aimed at making surges predictable. A similar emphasis was found in the Ministry of Health's internal briefs.[25] While there was a clear aim of forecasting and providing information to build flood defences, the proposals did not contain tight definitions of what exactly the research would do to get to these aims, though it emphasised the necessity for collecting more data from a wider area, including foreign ports.[26]

Following the initial refusal by the Treasury in 1931 to fund the proposed project, the driving force behind the repeated requests to the Ministry of Health for the research was LCC, not TI or the Hydrographic Department, though they continued to support it. In its contacts with the Ministry of Health, LCC claimed that more research was needed to know how high to build flood defences and framed the investigation as of national importance because storm surges affected a large area. Subsidiary arguments for it to be of national concern were also employed, such as that even if the research

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23 "Thames Floods: Technical Sub-Committee's Report", enclosure two to letter dated 10/08/1928 from Ministry of Health, LCC/CL/MD/1/11, LMA While this meeting, and thus the report, was not formally one of the Technical Sub-Committee, as the committee had already been disbanded, it was often presented as a formal report and treated as such, certainly by the LCC.
24 Douglas to Doodson, 13th Jan 1931, Box 120, BA, and TI (Doodson?) to the Hydrographer, 15th Jan 1931, Box 120, BA
25 For example, see "North Sea Surge", attached to "Minute sheet" by W.A. Ross, 2nd May 1933, HLG 51/39, NA.
26 "Data for the investigation of Storm Surges", appendix to Douglas (Hydrographer) to Doodson, 13th Jan 1931, Box 120, BA

would only assist London, the capital was of national importance, and also that London had already paid its due by LCC and PLA paying for the first round of research.[27]

Within LCC the main promoter was the Engineering Department, especially its Chief Engineer, T Peirson Frank, who had previously worked for Liverpool Council as City Engineer, including on embankments. He was a general city engineer with some concentration on water related engineering.[28] He is likely to have come into personal contact with TI in one way or other during his work in Liverpool, especially as he also lectured at the University, thus knowing its researchers and their work. As president of the Royal Society for the Promotion of Health in 1932 he argued for increased planning powers to be given to local authorities by central government.[29] He was President of the Institution of Civil Engineers 1945-46, devoting his presidential address to the important role engineers had played during the war, including the protection of flood defences in London.[30]

Another key promoter of the research within LCC was Lewis Silkin, representing South-East Southwark, one of the areas that had been hit by the flooding in 1928. He was at different times leader and deputy leader of LCC's Labour group and in 1934, when Labour gained control of LCC, he became chairman of the housing and public health committee. He repeatedly questioned the delays in granting funding for the investigation during public Council Meetings, e.g. in 1932 and 1933, and was part of deputations LCC sent to the Ministry of Health.[31] He left LCC before the conclusion of the saga, becoming MP for Peckham from 1936 to 1950 and Minister of Town and Country Planning between 1945 and 1950 in Clement Attlee's Labour government, where he led the creation of the Town and Country Planning Act (1947), which

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[27] Summaries of these debates are provided by the memo on "North Sea Surge", attached to "Minute sheet" by W.A. Ross, 2nd May 1933, and the report "Thames Flood Prevention – North Sea Surges – Further Investigations" to the General Purposes Committee, 4th May 1933, both in HLG 51/39, NA. This folder contains material on the Ministry of Health side. There is also further material on the LCC side in the London Metropolitan Archive (folders LCC/CL/MD/1/8, LCC/CL/MD/1/11, LCC/CL/MD/1/115, LCC/CL/MD/2/53, LCC/CL/GP/1/119). It has not been possible to locate any Treasury files.

[28] "Obituary. Sir Thomas Peirson Frank. 1881-1951.," *ICE Proceedings* 1, no. 1 (1952). See also Mike Royden, "Mike Royden's Local History Pages : Otterspool " http://www.btinternet.com/~m.royden/mrlhp/local/otterspool/otters.htm.

[29] T Peirson Frank, "Presidential Address by T. Peirson Frank, M.Inst.C.E., F.S.I., Chief Engineer to the London County Council. (Fellow.)," *The Journal of the Royal Society for the Promotion of Health* 53, no. 4 (1932).

[30] T Peirson Frank, "Presidential Address of Sir T. Peirson Frank. President 1945-46. (Includes Photographs and Appendix)." *Journal of the ICE* 25, no. 1 (1945).

[31] "High Tides in the Thames," *Times*, November 30 1932; "Thames Flood Inquiry," *Times*, November 1 1933.

increased the planning powers of local authorities.[32] Apart from their desire for further investigation into storm surges, Silkin and Frank seem to have shared an interest in planning, especially in strengthening the powers of local authorities in planning matters.

Within the Ministry of Health a wide range of people were involved with the case including the various Ministers of Health, their Private Secretary AN Rucker and the Permanent Secretary WAR Robinson.[33] The proposed project's usefulness and whether it was a matter for central government or not were two contested issues within the Ministry. Early on it was allocated to Assistant Secretary WA Ross.[34] His views varied over the years. Initially, before the first request in 1931 he gave a carefully worded endorsement of the project: there was "room for differences of opinion as to whether this work is necessary. In our view this work would be very useful".[35] While some doubted the necessity of the work, he approved at this stage. However, in 1933 he suggested the Ministry refuse to see LCC's deputation regarding the proposed investigation, so he was not a whole-sale promoter.[36]

Later, after about 1934, the project moved between different people, including IG Gibbon, the Principal Assistant Secretary who had authored the report of the 1928 Conference, and HWS Francis, the Director of the Local Government Division. The latter was enthusiastic of the investigation – clearly representing the interests of local governments, as is to be expected given his role – and claimed the local authorities had a "moderately effective criticism of the Treasury refusal" when he triggered the final request for funding from the Treasury in 1936 but made no further comments once the Treasury had again refused funding.[37] At that point Armer, in the same memo discussed above where he suggested dropping the idea of the flood defence co-ordination committee, was much more negative, writing that the issue had been "considered ad

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32 Richard Weight, "Silkin, Lewis, First Baron Silkin (1889–1972)," Oxford: OUP, http://www.oxforddnb.com/view/article/31684.
33 LCC usually addressed their correspondence to one of these two Secretaries.
34 The Ministers of Health in this period were Neville Chamberlain (1924-29), Arthur Greenwood (1929-1931), Neville Chamberlain again (1931), Edward Hilton Young (Nov 1931-1935) and Howard Kingsley Wood (1935–1938) – the latter's political career had started as a councillor at LCC. Neville Chamberlain went from being Minister of Health to Chancellor. Judging from the records, few Ministers were personally involved in the issue of the suggested second investigation, apart from Hilton Young.
35 Minute by WA Ross, 7th Aug 1931, HLG 51/39, NA
36 Minute by WA Ross to Gibbon, 6th Mar 1933, HLG 51/39, NA
37 Minute by S Francis, 7th Feb 1936, HLG 51/39, NA

nauseam" and that it was no more a national problem than many other issues.[38] Unsurprisingly, the Engineering Staff at the Ministry 'concurred' with their engineering and scientific colleagues at LCC and elsewhere in the need for further knowledge about storm surges.[39] Again unsurprisingly, those who had closer contacts with the Treasury were more negative, especially about the futility of repeated requests to it.[40]

While some within the Ministry thought the investigation would be useful and should be funded, they did not convince enough officers for the Ministry to fund the project itself. For example, the Minister, Edward Hilton Young, in 1933 thought the research would be "useful" but could not offer any money for it out of the Ministry's ordinary budget.[41] Instead money had to be applied for separately from the Treasury. Despite the internal disagreement the officials at the Ministry of Health agreed sufficiently with LCC regarding the usefulness of the research to contact the Treasury regarding it at least four times between 1931 and 1936. There these requests for funding were repeatedly refused and the LCC's arguments rejected.

Initially, the Treasury gave two reasons for refusing funding: it did not think the research was urgent, and there was an "essential need for economy", which given the timing in late 1931, after the financial crisis and cutbacks in government funding that year, was an unsurprising reply.[42] In 1932, after discussions between Ross from Ministry of Health and Assistant Secretary BW Gilbert and Prinicipal JB Beresford at the Treasury these reasons were repeated, with the addition that the Treasury saw this as a local matter, not a national issue, and that therefore it should be paid for by local authorities and not central government.[43] According to the Treasury, it did not matter that many local authorities over a wide area were involved: it was still something the local authorities should deal with. A similar response was given in 1934.[44] In 1936 Ministry of Health officials again contacted the Treasury and again were told no money

[38] Minute by Armer, 3rd Feb 1936, HLG 50/129, NA
[39] Minute by WA Ross 7th Aug 1931, HLG 51/39, and Memo on "North Sea Surge", attached to "Minute sheet" by WA Ross, 2nd May 1933, HLG 51/39, NA.
[40] Minute by SC Alford, Assistant Deputy Accountant-General, 25th Aug 1931, HLG 51/39, NA, Minute by IFA (IF Armer, Principal), to Ross, re views of Treasury, 6th Jul 1932, HLG 51/39, NA.
[41] Armer to the Clerk of LCC, 6th May 1933, HLG 51/39, NA
[42] Armer at Ministry of Health to the Clerk of the LCC, 16th Nov 1931, LCC/CL/MD/1/11, LMA. Officially the decisions of the Lords Commissioners of HM Treasury decisions were communicated from the Secretary there to the Secretary at Ministry of Health, giving little further information on who within the Treasury was dealing with the matter.
[43] Legge to the Clerk of the LCC, 8th Aug 1932, HLG 51/39, NA
[44] Robinson at Ministry of Health to Gater at LCC, 18th May 1934, HLG 51/39, NA

was forthcoming. The minute made after this meeting points toward a key underlying reason for the lack of Treasury support. They still did not see the issue as a national one, but more importantly did not want to set a precedent: "The argument, if conceded for the cost of the enquiry, could or possibly would also be applied to the consequential work on defences in the Thames and elsewhere".[45] If this investigation was accepted as a national responsibility this was thought to imply that flood defence could be seen as a national government responsibility, instead of the responsibility of the riverside or coastal landowner and the local authorities as it had traditionally been, and the Treasury did not want this. It was only grudgingly providing increased support for land drainage and it did not want to pay for the building of flood defences. While LCC claimed to simply be asking for patronage for a small scientific investigation, the Treasury saw the request as the beginning of a wider request for funding of flood defences. It was determined to stick to the general principle that flood defence generally – despite the recent debate regarding land drainage – remain a local responsibility, so the research also had to remain a local responsibility.

It is not clear from the sources whether LCC knew the Treasury's view on this (they do not appear to have been told formally, but may of course have heard of it informally), nor if they had any plans to widen their appeal towards flood defence more generally if this appeal for research funding was successful. A year and a half later, in June 1937, the positions of the Treasury arguing it was a local matter while local authorities argued it was a national matter, were described as a "deadlock of policy".[46] At this point however LCC had a change of mind. On the suggestion of Frank, the chief engineer, the council decided to ask TI if it was willing to still do the investigation at the previously agreed price of £1,100.[47] TI replied in the affirmative; they were "prepared to undertake an enquiry into the probability, frequency and amplitude of storm surges in the North Sea likely to affect the River Thames".[48] The only reason given for LCC's change of mind was "the undoubted necessity" of the investigation and the "continued refusal of H.M. Government to defray the small expenditure involved".[49] It appears LCC simply gave

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45 Minute sheet, Accountant-General SC Alford, 18th Feb 1936, HLG 51/39, NA
46 Minute sheet on Thames Flood Prevention, Note to the Clerk of the Council, signed A w B (?), 22nd Jun 1937, LCC/CL/MD/1/115, LMA
47 Peirson Frank (Chief Engineer) to Doodson, 16th Aug 1937, Box 120, BA
48 TI (Doodson?) to Chief Engineer, 17th Aug 1937, Box 120, BA
49 Extract of Minutes from General Purposes Committee, "Storm surges in the North Sea. Reply to memorandum of 11th May, 1936" , 8th Jul 1937, LCC/CL/MD/1/115, LMA

up on the Treasury ever giving in on this issue.[50] In autumn 1937 LCC's clerk collected promises of money from various coastal local authorities and the PLA.[51] With these in hand Frank recommended to LCC's Fire Brigade and Main Drainage Committee that the research be started. This Committee agreed and with approval from the Council as a whole,[52] the clerk confirmed the order for the research to TI in April 1938.[53] Payments were made over a period of several years and are included in the column for commercial work in table 4.1.

This issue was part of the often fraught relationship LCC had with central government. Even the official history of LCC published in 1939 described its relationship with central government as frequently dominated by fights.[54] After the protracted debates it appears LCC simply gave in and decided they wanted the investigation more than they wanted government money for it.[55] In addition, the funding of storm surge science in the 1930s was not just about the funding of a small piece of research, but was by the Treasury seen to have larger implications in terms of general flood defence policy and who should pay for building defences. In the end only local authorities (including the PLA) led by LCC found the appeal of storm surge forecasts strong enough to pay for research into them. While the Admiralty, especially the Hydrographic Department, had been involved in the work from early on, and provided assistance with collecting data, they were not prepared to fund the investigation.[56] In addition the people within departments mattered, with a specific officer within LCC, Frank, playing a key role in promoting the project. At least for now, the responsibility for flood defence had been firmly assigned to the local/regional level and the Treasury's view on flood defence as a local issue had prevailed.

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[50] There had been no recent or obvious changes in officials or politicians at LCC to explain the issue. LCC had changed to Labour control in 1934, so not particularly recently.

[51] Documents in LCC/CL/MD/115, LMA

[52] Reports and minutes of meetings from various committees, Mar 1938, LCC/CL/MD/115, LMA

[53] [Unreadable signature] Clerk to the Council to Doodson, 29th April 1938, Box 120, BA

[54] Reginald W. Bell and Gwilym Joan Gibbon, *History of the London County Council 1889-1939* (London: MacMillan and Co., 1939), 581-590.

[55] There is no archival indication that the investigation was part of preparations for the war, but it is possible that LCC became keener on the storm surge investigation as they started to prepare for potential bomb damage to their flood defences, something which Frank was heavily involved in. See file LCC/CE/WAR/2/11 for material on this work and also "Obituary. Sir Thomas Peirson Frank. 1881-1951.."

[56] The Admiralty kept certain tidal gauges running while the investigation waited for funding, occasionally at the request of the Ministry of Health. See for example minutes and correspondence between various people at the Minister of Health and the First Lord of the Admiralty in May 1933, HLG 51/39, NA

This was not just about funding but also about different interests being pitted against each other. What should the government support? Those who supported and pushed the investigation as a national charge often had close ties to local government and planning, as well as engineering. They were opposed by others, especially the Treasury but also some within the Ministry of Health, who did not see such work as a national responsibility. Mark Whitehead has described similar debates and tensions regarding what type of air pollution science central government should support, but in his case the end result was different: a slightly re-worked version of science, with more clearly defined and less "speculative" aims, was given continued central government support.[57] The key difference between the two case studies is that while the quality and appropriateness of air pollution research was questioned, it was already supported by the central government, so had already been deemed as of national interest. The pollution debate was as much about which part of central government should support it as it was about what research should be supported by the central government. In the case of storm surge science the question was more clear-cut: should central government support this kind of research at all? The final answer was no, as it was not deemed a national issue. Regarding storm surges, the rise of governmentality and planned research supporting engineering projects and concerns was contested.[58] The rise in central government involvement in research aimed at forecasting nature was contested between different interests and it was not obvious who should pay for research of this nature.

6.2 The first stage of the LCC research project: Construction of surges

We now return from London to focus on work at TI, where only limited work on storm surges had been done since 1929. In 1933 Robert Henry Corkan (1906-1952), under the direction of Doodson, researched meteorological effects, finding annual variations in tides that could be incorporated in work on periodic tidal predictions.[59] This is an example of research work more linked to TI's key research programme on periodic tidal

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[57] Whitehead, *State, Science, and the Skies*, 127-131, 217-120.
[58] Compare Armytage, *The Rise of the Technocrats*; Edgerton, *Warfare State*; William McGucken, *Scientists, Society, and State: The Social Relations of Science Movement in Great Britain, 1931-1947* (Columbus: Ohio State University Press, 1984).
[59] LOTI, *Annual Report 1933*.

predictions than with forecasting storm surges, but Corkan would become the key researcher doing the work during the second investigation. He came from the Isle of Man, where his father was a station master, and had taken a first class BSc in 1927 and then an honours degree in the Department of Applied Mathematics under Joseph Proudman at Liverpool University in 1928. Initially he was considering becoming a teacher, but when he was recommended for a post as Assistant at TI by Proudman he accepted and started work in 1929.[60] He was awarded the degree of M.Sc. in 1933.[61] Corkan's appointment was probably the result of discussions before the 1929 merger of the Observatory and the Tidal Institute, when Proudman suggested that a honours graduate should be appointed to understudy Doodson's work, as only he "thoroughly understood this work", implying that Corkan was from the start meant to be more than an ordinary computer.[62]

TI's researchers were part of wider interwar personal and scientific networks. A range of research work was done at TI in the 1930s, for example some work on measuring tidal currents in the Irish Sea using a new meter developed by Doodson. This work was done in collaboration with the Department of Oceanography, now run by Proudman, using a university-owned ship during the summers of 1936, 37 and 38. The work on the meter and the expeditions was sponsored by the Royal Society, through its Government Grant Committee, and used instruments borrowed from the Hydrographic Department and the Met Office.[63] It provides an example of oceanographic expeditionary work carried out in the UK in the interwar period, done as collaboration between a range of actors including major scientific actors (the Royal Society) and state-military actors (the Admiralty). Not only does this show TI's involvement in wider scientific networks but it is also a small example of interwar oceanographic work in Britain.[64]

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[60] He was initially paid at £250 per year, which was to increase by £15/yr up to £480. LOTI, *Annual Report 1952* (Liverpool: University of Liverpool, 1952), 5; Scoffield, *Bidston Observatory*, 176-177. Memo, Meetings of the Sub-Committee appointed by the Observatory Joint Committee, 30th Nov 1928, D/BO 1/1/5, MMM - North Street

[61] LOTI, *Annual Report 1933*.

[62] Memo, Meetings of the Sub-Committee appointed by the Observatory Joint Committee, 30th Nov 1928, D/BO 1/1/5, MMM - North Street

[63] LOTI, *Annual Report 1936* (Liverpool: University of Liverpool, 1936); LOTI, *Annual Report 1937* (Liverpool: University of Liverpool, 1937); LOTI, *Annual Report 1938* (Liverpool: University of Liverpool, 1938).

[64] Compare to those who have argued little such work was carried out, see Deacon, "Of Seas and Ships and Scientists."; Hamblin, *Oceanographers and the Cold War*.

In early 1938 the work for the further investigation into storm surges got going. There was little further specification from LCC of what the research should consist of or achieve, with TI's earlier definition of the project's aims repeated, defining the project as an "investigation into the probability, frequency and amplitude of storm surges in the North Sea and their effect on the River Thames".[65] The primary impact of the local authority patronage on TI's work and practices appears to have been that the work was begun at all.

The first stage of the work constructed surges using the practices discussed in the previous chapter, defining them as residuals or differences between observed and predicted/unperturbed tides at the same time, continuing practices developed earlier at TI.[66] The key worker was Corkan, who visually inspected ten years' (1928-38) worth of tidal gauge records for Dunbar and Southend, identifying 85 surges over two feet at each place and reducing this data into residuals.

In a change from the earlier investigation, all the residuals were constructed using Doodson's graphical obs-unperturbed method previously used only for Dunbar, which constructed surges by graphically comparing sea levels at six-hourly intervals and measured the distance between graphs to estimate the surge. In addition to making the residuals for both Dunbar and Southend comparable by constructing them using the same method, Corkan later explained that this method had "many advantages for it involves no preliminary calculations, is easy to apply, does not use up the valuable time of the predicting machine, and completely eliminates the semi-diurnal tide, the most important factor".[67] Corkan also considered the method theoretically sound, something he demonstrated visually by graphing the residuals for one event using the two different methods and stating that the two graphs were "in very close agreement".[68] No further justification was provided and no definition of what 'close agreement' meant given, but in the graph the largest difference between the two methods was about 1ft, implying this was considered close enough.

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[65] [Unreadable signature] Clerk to the Council to Doodson, 29th April 1938, Box 120, BA
[66] See also list of tasks, "RHC, Jan 1939", Box 59, BA
[67] Robert Henry Corkan, *Storm Surges in the North Sea: Vol 1* (Washington D.C.: Hydrographic Office, H.O. Misc. 15072, 1948), 12.
[68] Ibid., 13.

According to Doodson's first progress report to LCC, in January 1939, this first stage was completed after a year's work, which gives some insight into how time consuming the construction of surges was. In his progress report, perhaps forestalling potential critique of the length of time spent on this stage of the project, Doodson emphasised that the construction of the tables of residuals (like those in figure 5.2) were "an essential part of the investigation", giving them "ample data on which to work".[69] However, while no doubt providing ample data, it was also very specific data, defining surges in a particular way, as residuals. Having spent the first year producing residuals in this manner Corkan was locked into this definition of surges. While this definition was his and Doodson's choice, the time and effort spent on producing the residuals tied them to using this definition, and specifically the tables of residuals, to construct forecasting formulae. This choice of definition was in itself tied to Doodson's earlier experience and the work he had done in 1928.

6.3 The war, oceanography and TI's research

At about this stage of the project the Second World War broke out. This section discusses TI's work both on surges and more generally during the war, especially how its work was linked to other oceanographic research done during the war, arguing that TI did not take part in the development of new networks between the Navy and oceanographers but instead strengthened their existing role as the state's tidal contractor. While the Navy showed interest in TI's surge work this interest was partial and did not directly lead to further funding being given to TI, though it affected how they did their work. In addition, this section discusses how TI saw a dramatic increase in income during the war, emphasising that this was due not so much to their surge work but instead to a dramatic increase in the number of analyses and predictions produced by TI (see also table 4.1 and figure 4.2).

TI's work on the LCC project continued throughout the early stages of the war until Corkan was lent to the Admiralty in mid-1941 as a research assistant for the Nautical Almanac Office and the Hydrographic Department in Bath.[70] As discussed in the

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[69] Doodson to the Clerk to the Council, 10th Jan 1939, Box 128, BA
[70] LOTI, *Centenary Report and Annual Reports (1940-1945)*, 14-15, 17-18 & 20.

introduction many historians of oceanography have seen the war as a turning point, sooner or later leading to increased naval patronage of oceanography. In particular, during the Second World War the Navy and other parts of the military became increasingly interested in detailed forecasting of sea conditions near the coast to prepare for various amphibious military operations. Several historians have studied forecasting of waves, swell and surf by the Allied Navies during the Second World War, i.e. forecasts of short-period waves in comparison with the longer periods involved in tides and surge.[71] Early work in the UK on swell forecasting was done by Instructor Commander Suthons, technical adviser to the Director of the Naval Meteorological Service, and later when the waves and swell forecasting system was operationalised the involvement of George Deacon at the Admiralty Research Laboratory at Teddington was key. It then also involved the Oceanographical and the Scientific and Research Departments of the Admiralty.[72] Both Suthons and Deacon will turn up again in this thesis. The forecasting network also used research by Harald Sverdrup and Walter Munk at Scripps Institution of Oceanography in the US.

TI's storm surge work was drafted into the war effort in 1940 when the Hydrographic Department, which had been involved with TI's work for LCC and PLA, for example by providing data, asked TI to provide a formula to forecast meteorological effects on tidal predictions on the continental coast. The request reached Bidston in late May and TI provided their "Instructions for computing meteorological disturbances of sea level on the German North Sea Coast" in mid September 1940. The work was an extension of the investigation they were carrying out for LCC. Doodson claimed their method for calculating meteorological effects was easy to use, "exhaustively tested" and "a sound one" with satisfactory results – giving no formal statistical evidence or definition of

71 Schlee, *A History of Oceanography*, 304-310; Harald Ulrik Sverdrup, "New International Aspects of Oceanography," *Proceedings of the American Philosophical Society* 91, no. 1 (1947); Weir, *Ocean in Common*.
72 C. R. Burgess, "Climate and Weather in Modern Naval Warfare," *The Geographical Journal* 111, no. 4/6 (1948): 239-240 & 243. Burgess was a Commander in the Naval Meteorological Branch who was in charge of climatological planning during the war, A. A. Miller *et al.*, "Climate and Weather in Modern Naval Warfare: Discussion," *The Geographical Journal* 111, no. 4/6 (1948). On Deacon and the Admiralty Research Laboratory, see George Deacon, "Ocean Waves and Swell," in *Oceanography: Concepts and History*, ed. Margaret Deacon (Stroudsburg: Dowden, Hutchinson & Ross, Inc., 1978); Margaret Deacon and Anthony Laughton, "The Founding Director, Sir George Deacon," in *Of Seas and Ships and Scientists : The Remarkable Story of the UK's National Institute of Oceanography 1949-1973*, ed. Anthony Laughton*, et al.* (Cambridge: The Lutterworth Press, 2010).

what counted as a satisfactory result.[73] The point here is that Doodson appears to have believed the forecasting formulae worked well enough to be useful to the Navy.

However, there is no evidence that TI's formula was put into use by the Navy.[74] While the wave and swell forecasts included some consideration of tides, such as tidal currents, they concentrated on forecasting short-period waves, not sea levels or long-period waves and there is no evidence the forecasters used TI's formulae or anything like it. The formula for forecasting of swell included wind speed, fetch and windstorm duration, while TI's work concentrated on pressure gradients, which had some relation to wind speed but none to fetch or duration.[75] Indeed, there is no evidence TI was connected to the scientific networks involved in the swell forecasts, by for example being asked to help with research.[76] The available evidence suggests that TI's work and workers were not involved in these new developments in oceanography or part of the personal networks that wartime service created in oceanography. Corkan's work in Bath at the Hydrographic Department was for the Nautical Almanac Office and its offshoot, the Admiralty Computing Service. This meant his work was on advanced mathematical and large-scale computational work, not the 'new' oceanography, and he does not seem to have developed personal networks with naval oceanographers.[77] Such naval

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[73] TI to the Hydrographer, 12th Sep 1940, Box 62, BA
[74] There is no indication in TI's archive as to the use the Admiralty made of these instructions, and according to the staff at the UKHO archive the file (H/10027/39) cannot be found, and while they cannot determine that it has been destroyed they say this is likely. TI's Instructions probably required further clarification before an officer was able to use them and if they did not get any such clarification the Instructions may have been unusable and simply ended up on the Hydrographer's shelf. In addition, they may have taken too long to produce, being ordered in May 1940. Perhaps they were ordered as part of plans to enter Belgium and the Netherlands to hold back Germany, which the Allies thought would attack from the North, see Gerhard L. Weinberg, *A World at Arms : A Global History of World War II* (Cambridge Cambridge University Press, 1994), 122-126. The outcome of the German attack – with the British driven back across the Channel – had changed the strategic situation the Instructions were ordered in dramatically by September.
[75] Schlee, *A History of Oceanography*, 304-310; Sverdrup, "New International Aspects of Oceanography."
[76]A later letter implies that Deacon had not met Doodson until the 1950s, see "Copy of letter to Dr. Doodson from Dr. Deacon", 24 Aug 1951, D/BO/1/4/17, MMM - North Street
[77] For more on the war-time work of the Nautical Almanac Office (NAO) and the Admiralty Computing Service (ACS), see Croarken, *Early Scientific Computing in Britain*, ch 6. It is possible that Corkan was the additional temporary assistant the head of NAO, HD Sadler, was allowed in summer 1941, ibid. p 67. Corkan is mentioned as working at ACS in Donald H. Sadler, "Extract from a Personal History of H.M. Nautical Almanac Office, 30 October 1930 - 18 February 1972," ed. George A. Wilkins (Sidford, Devon: United Kingdom Hydrographic Office, 2008). http://www.hmnao.com/nao/history/dhs_gaw/nao_perhist_0802_cyh_part_appendices.pdf, accessed 1 Sep 2010. For a list of work done by ACS to the end of the war, see Admiralty Computing Service, Seventh Report on Activities, BLRD E7, Churchill Archives Centre, Churchill College, Cambridge. Some of the work listed there was potentially related to tidal research, such as a computation of an integral occurring in the theory of water waves, and an investigation of the reflection of surface waves, both done for the Admiralty Research Laboratory at Teddington. While these calculations potentially were for the

oceanography networks have been shown to have been very important post-war for US oceanographic institutes by for example Ronald Rainger.[78] TI's work on storm surges seems to have been a different kind of oceanography to that of swell forecasting, with the different kinds of oceanography done by different people. The people involved in swell forecasting were also involved in the appearance of a new 'integrated' oceanography after the war, which has also been linked to the publication of *The Oceans*.[79] The impact of the appearance of this integrated oceanography on TI will be returned at the end of the chapter.

However, even though TI's forecasting formula does not appear to have been used by the Navy, their request had a strong influence on TI's attempts at producing a forecasting formula for LCC, as it was one important reason they went down the statistical route for this work. This in turn informed TI's practices until the 1950s. Before looking more closely at this, it will be set in the context of TI's other work during the war and its role as the state's tidal contractor, which increased dramatically during the war, as it became increasingly focused on providing tidal predictions for the Navy and also had fewer staff.[80] Like the First World War had, the Second interrupted exchanges of tidal information between countries and companies, and in addition the military required extra tidal predictions, e.g. for the Normandy landing. As can be seen in table 4.1 and figure 4.2 there was a dramatic increase both in the number of predictions TI produced and the income they received from this during the Second World War. Their income from predictions increased from £1540 in 1938 to £4680 in 1945. At the same time, starting in 1939, staff left to join the services. Doodson's assistants (i.e. the female computers, Corkan and one other member of staff[81]) decreased from nine in 1938[82] to six female computers in 1943 and 1944. After the war

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swell and wave-work, according to Croarken the work at ACS was isolated from those ordering it to the extent that those at ACS often found it difficult to know what the work was for, indicating that even if Corkan worked on these aspects it would not have connected him to the personal networks of those doing 'new' oceanography.

78 Rainger, "Constructing a Landscape for Postwar Science."; Rainger, "Adaptation and the Importance of Local Culture."

79 Mills, *The Fluid Envelope of Our Planet*, ch 9, esp. p 260-264.

80 Little archival material has been preserved from the Second World War, so the overview of TI's work is taken from published sources, particularly the Centenary Report published in 1945, see LOTI, *Centenary Report and Annual Reports (1940-1945)*.

81 H J Bigelstone, whose primary duties were to do the Observatory-side of LOTI's work, such as managing the meteorological observations done at the site. He does not seem to have been much involved in TI's storm surge work.

82 LOTI, *Annual Report 1938*.

the number of assistants doubled to twelve in 1947.[83] The focus on providing tidal predictions and the depleted staff meant that little research work, apart from that in response to military demands for new types of tidal analyses and predictions, was done during the later stages of the war.[84] One reason TI did not become involved in the new oceanographic projects the Navy were doing was lack of time and staff. TI's workers instead focused on providing tidal predictions to the state – strengthening their existing networks instead of building new ones. At the same time this focus led to increased income for TI, improving its financial position – the balance of its reserve account went from £2461 in 1938 to £7416 in 1946.

David Edgerton has argued that universities were not "significant R&D contractors" during the war, arguing they instead focused on educating the future research corps.[85] TI, however, is an example of an institute with close connections to a university which increased its role as the tidal R&D contractor to the state during the war. In Edgerton's terms, TI's work was 'nationalised' in the sense that the work became focused on providing tidal predictions for the war, but it remained under the leadership of the religious Doodson, an academic mathematician and also continued to be governed as before. In terms of research, the main impact of the war was not changes in funding (though the level of this increased) but loss of staff and a change in research programme, temporarily focusing almost solely on tidal analysis and predictions.

6.4.1 The impact of Hydro's request on TI's work

This section again shifts register to analyse Corkan's practices in constructing forecasting formulae more closely, also discussing what TI meant by successful forecasting at this point. This section will look at TI's work on the LCC project, concentrating on its practices of calculation and how these changed in response to the Navy's request for a forecasting formula. Analysing traces of Corkan's work provides some insight into the practices of calculation that were used to make surges more predictable. It also emphasises the importance of contingent and constricted choices made by researchers. These choices placed them within particular traditions of practice

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[83] LOTI, *Annual Report 1947* (Liverpool: University of Liverpool, 1947).
[84] LOTI, *Centenary Report and Annual Reports (1940-1945)*, 14-15, 17-18 & 20.
[85] Edgerton, *Warfare State*, 160.

and enabled them to produce results, but also influenced or limited the possible results the work could produce.

As a result of his earlier construction of surges Corkan now had surges laid out on specific documents. The next stage of the project was one of mathematical experiments, which involved much work. For example, he constructed a number of different typologies for the 85 surges in his study.[86] To do this Corkan had to construct documents, draw maps, and repeatedly write and rewrite lists of surges and meteorological conditions. Such work puts a physical limit on the amount of experimentation that can be done; the number of different practices that can be attempted.[87] The process of producing and choosing typologies took much work and involved consequential, contingent and restricted choices. The choice of documents and patterns was not innate to them, or sprung 'naturally' from the work.[88] How the researchers chose to organise documents had much to do with the results they got, but they could not necessarily predict or know which pattern would produce a supplement, i.e. a result that increased their ability to do things, in this case forecast surges.

Corkan's experiments with different ways of forecasting surges illustrate how his constricted choices regarding practices of calculation influenced the results TI achieved. Many of these choices were linked to TI's earlier practices, setting Corkan's work within a specific tradition of practice. From theory, the choice of which was influenced by senior researchers such as Doodson, Corkan assumed that storm surges were linked to the tractive (frictional) forces produced by wind on the sea and could be seen as damped oscillations of the sea due to varying wind and pressure.[89] Corkan initially decided to experiment with directly applying theory by the Japanese geophysicist Takaharu Nomitsu to predict storm surges. This treated surges as oscillations in a dynamic sea. Nomitsu's theory was a development of Proudman and Doodson's 1924 work[90] on time relations in meteorologically induced oscillations in theoretical seas,[91]

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[86] Folder "Types of surges", dated 1939, Box 59, BA

[87] This is similar to the data and computer friction Edwards discusses, see Edwards, *A Vast Machine*, 83-84.

[88] Compare Latour, *Pandora's Hope*, 53.

[89] Corkan, *Storm Surges: Vol 1*, Section 2. This was based on theoretical work by Proudman and Doodson as well as other workers, for example Takaharu Nomitsu, Vagn Walfrid Ekman, Harold Jeffreys, Carl-Gustaf Rossby and Harald Sverdrup. A list of references written by Corkan can be found in Box 59, BA.

[90] Joseph Proudman, "The Effects on the Sea of Changes in Atmospheric Pressure," *Geophysical Journal International* 2, no. s4 (1929); Joseph Proudman and Arthur Thomas Doodson, "Time-Relations in Meteorological Effects on the Sea," *The Proceedings of the London Mathematical Society* 24(1924).

and fell within TI's work in the Laplacian tradition. From Nomitsu's formulae one could – in theory – predict the sea level resulting from wind or pressure some time beforehand, given empirical determinations of the period of the sea and the damping ration.[92] TI called this the six-point method as their version of it used pressure gradients at six locations over the North Sea.[93] The problem was then how to put this theory into practice.

Having chosen to experiment with the six-point method, Corkan also had to choose how to estimate the wind at sea level from the pressure gradients. He chose to use what is known as geostrophic wind, which is a theoretical wind assuming negligible friction and that the ground-level isobars are straight, so that the pressure gradient force equals the coriolis force. If the isobars are straight the geostrophic wind can be directly calculated from the pressure gradient and is of constant speed and parallel to the isobars.[94] Actual ground-level wind is affected by friction and other effects, like topography, and the relationship between wind estimated from pressure gradients and actual surface wind was not well established.[95] However, a choice had to be made and the choice of geostrophic winds simplified TI's calculations as it meant they could straightforwardly correlate surges with pressure gradients, which could be read off weather charts and were thus much easier to find than measurements of wind at sea level. How did this choice of a computationally easier formula influence the result of Corkan's work?

When Corkan summed up the work before he left TI during the war, he believed the six-point method could produce "satisfactory" and "accurate" predictions, but had realised the work on it required revision as "the method fails to give accurate results when meteorological conditions are complex". (The next section looks at how he and Doodson judged accuracy.) There were problems when the isobars "had large curvature

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[91] E.g. a directional canal of uniform depth and under various other simplifying assumptions
[92] Corkan, *Storm Surges: Vol 1*, 86-90.
[93] Note describing method in folder Theory, ca 1939, Box 59, BA
[94] It is theoretical as the pressure would not be the same on ground level as it would be where there is negligible friction, say at 1000m height, so as the geostrophic wind is calculated based on ground level pressure it would never in fact exist. Gradient wind is another possible way of measuring the wind, and is like the geostrophic wind but taking into account the curvature of the isobars, e.g. around the centre of a low. Sanderson, *Meteorology at Sea*, 32-37; Williams, Higginson, and Rohrburgh, *Sea & Air*, 142-145.
[95] For a later discussion of these difficulties see Roll H.U. Hull, *Physics of the Marine Atmosphere* (London: Academic Press, 1965), 176, 213-176.

as [the depression] centre is crossing [the] N Sea".[96] The choice of geostrophic wind – assuming straight isobars – was thus deemed to have produced the desired results in terms of forecasts when the isobars were straight,[97] but it did not produce the desired accuracy when the isobars were curved. TI could not necessarily have foreseen that this would become a problem when they were picking which formulae to use to calculate winds in their work.

However, before TI decided what to do about the curved isobar problem, something else happened which impacted on how they chose to analyse surges. As mentioned above Hydro sent TI a request for a forecasting formula for meteorological effects in May 1940 and by mid-September TI had produced one.[98] This quickly produced forecasting method combined statistical work with dynamical theory. The method forecast the meteorological effect at one port, Norderney, by calculating and combining statical and dynamical values, and then used statistically-derived coefficients to calculate and forecast the meteorological effect at other ports.[99] It was at least partly based on statistical relationships between wind and meteorological effects unlike the more theoretical ones from Nomitsu, which had been used in the six-point method. The work thus represented a shift away from the Laplacian tradition with which Corkan had experimented, and was starting to find problems with, towards statistical methods.

Later Doodson claimed the shift towards statistics had come in response to demands to quickly produce a forecasting formula.[100] However, to that should be added the problems TI were having with the six-point method and with using geostrophic winds, which Corkan thought did not work well when the isobars were too curved. Corkan's choices of particular ways of organising documents and picking formula, and the results achieved, were contingent on many issues, ranging from geostrophic winds being easy to calculate to Hydro's demand for a prediction formula. The shift towards statistical methods was also contingent on many other issues, such as Doodson's training and

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96 Draft "Report of present state of work on Thames Flood", unpublished note in Corkan's handwriting ca 1941, Box 59, BA

97 Estimate of surface traction, Note on final results, and also the untitled list of six points (and 12345 in bottom corner) in folder 'storm surges', Box 58, BA

98 TI to the Hydrographer, 12th Sep 1940, Box 62, BA

99 Instructions and remarks, TI to the Hydrographer, 12th Sep 1940, Box 62, BA

100 "Report on work done at the Liverpool Observatory and Tidal Institute," Part A, "General report", Doodson, Paper 5(iii) for 5th meeting of Advisory Committee on Oceanographic and Meteorological Research on 30th Apr 1956, Box 159, BA

experience in statistics. Corkan shifted from the Laplacian tradition within which much of TI's theoretical work was done, such as the work of Doodson and Proudman which Nomitsu had developed, towards the statistical tradition Doodson had earlier used in his attempts to forecast surges in Liverpool and after the 1928 event. Faced with an urgent demand for a forecasting formula, TI's researchers shifted from work using one set of practices they were used to, towards another set of practices they were also used to.

6.4.2 Success Judged as Small Residuals

What did Corkan mean when he wrote that the six-point method produced "satisfactory" and "accurate" prediction? Doodson made it clear their definition of satisfactory prediction looked at the size of residuals. For example, to LCC he claimed that a formula with a residual error of about 0.5 feet "very faithfully reproduce[d] the disturbances of sea level at Southend" during large surges – so 0.5 feet residual error was 'good'.[101] To Doodson and Corkan – who were the ones doing the defining, as the clients were not at this stage involved with the judgement of the fit of the formulae produced – a good formula produced a predicted residual that only differed a 'little' from the observed residual. How much 'little' was remained fuzzy. The difference by which the fit was measured was usually gauged by visually comparing graphs, and not by calculating formal statistical measures such as standard errors. For example, a prediction using the six-point method for the 1928 surge was declared 'poor' on the basis of the top graph in figure 6.1.[102]

TI were not interested in comparing observed and predicted *total sea levels* but instead measured the fit of their formula by comparing *residuals*, producing yet more second-order residuals in the form of the difference between observed and predicted residuals. Such second-order residuals could be used to further refine the predictions – a very similar method to that Doodson had used on the periodic tidal predictions in the 1920s. This can be seen in the second graph on figure 6.1, which shows a comparison of the earlier surge at Dunbar with the second-order residual between the observation and

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[101] Doodson to the Clerk to the Council, 29th Feb 1940, Box 128, BA
[102] "An investigation of surge Jan 1/3 1928", in folder 'Development of 6-point and oscillatory method', Box 59, BA

prediction at Southend. These two lines were deemed very similar. By constructing graphs such as this one TI decided that their method had to take into account external surges travelling past Dunbar but originating further afield. In other words, they decided that the error in their original forecasting formula (i.e. the second order residual) was not an error but another variable, represented by the surge at Dunbar. Later work included this as one of the variables to be accounted for in the forecasts. To TI a good forecast was one that took into account as many n-th order residuals (e.g. external surges) as they thought possible.

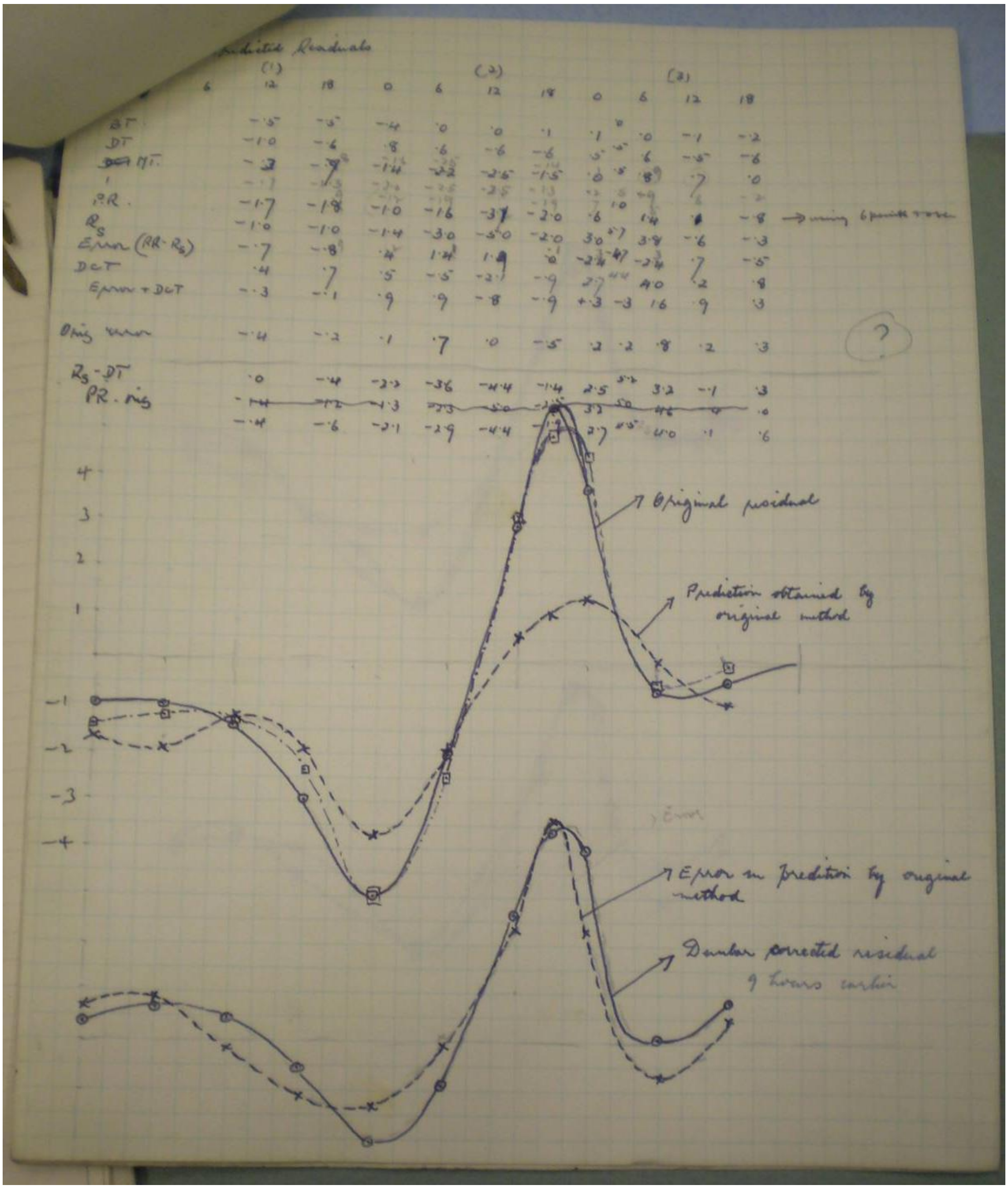

Figure 6.1: Graphs used by TI to identify external surges as a second-order residual. The top graph compares their first forecast with observations. The bottom graph compares the error in the forecast, or the difference between the lines in the top graph, with the residual at Dunbar.[103]

To TI forecasts of surges were forecasts of residuals. Successful forecasting in turn meant producing a small secondary-level residual, a small difference between observed

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[103] "An investigation of surge Jan 1/3 1928", in folder 'Development of 6-point and oscillatory method', Box 59, BA

and predicted residuals, and all these residuals were presented by TI in tables and graphs. This was linked to TI's definition of surges and their early work on the project, producing reams of tables of residuals. This work and these documents tied them to a particular kind of analysis of storm surges. These tables were the data they compared, their baseline, as well as their raw data. Bruno Latour emphasise how chains of documents connect[104] but they can also be chains that bind and limit, and paying attention to this develops his concept to take further account of how these chains are constructed. As Herbert Kalthoff discusses, the choice of a particular practices of calculation constitutes objects but makes them calculable in a particular way which to some extent 'sets' the next level of calculation.[105] TI's whole approach to surges relied on residuals, which in turn came from Doodson's work on tides and, further back, from his training in statistics. The choice of a particular definition of surges, as residuals constructed in a particular way, led to other choices. The choice of a particular formula to calculate winds led to particular results, also influencing other choices.

6.4.3 THE CONSTRUCTION OF A FORECASTING FORMULA: THE INFLUENCE OF EARLIER PRACTICE

After the war Corkan picked up the project again, producing a final report in 1947 which included a forecasting formula for surges at Southend. How did Corkan construct this forecasting formula? One way of answering this question is to say that he constructed a multiple regression formula which correlated the surge with gradients of barometric pressure over the North Sea, but what did this mean? This section will look at how he defined prediction as the addition of different effects and in particular how he constructed one of these effects through the use of multiple regression. I analyse his emphasis on a 'well conditioned' formula over any other possible emphasis and how this emphasis together with TI's constricted choice to use geostrophic winds influenced his results. Corkan's particular chains of documents resulted in particular results, and were linked to the traditions of practice Corkan and Doodson were operating in.

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104 Latour, *Pandora's Hope*, 70.
105 Kalthoff, "Practices of Calculation."

Corkan defined prediction as "computing the effects to be expected from the known meteorological situation".[106] However, while this definition emphasised the meteorological situation, he in fact included various other effects in his computations. Corkan constructed his forecasting formula by disaggregating the already calculated residuals into many separate effects through careful calculations of each effect, such as the external surges identified through graphs such as figure 6.1. He disaggregated surges into tidal effects not already accounted for, barometric pressure, external surges and wind effects. To 'forecast' (actually hindcast) a surge, each of the different effects were separately constructed using different practices of calculation ranging from addition to the application of a multiple regression forecasting formulae for the wind effect. His final formula for forecasting surges was an addition of the different effects, resulting in a number representing the predicted (hindcasted) residual or surge.[107] This addition of different effects was the last step, the end result of Corkan's construction of a forecasting formula. The result took no account of the predicted tide, so in order to know whether a particular predicted residual represented a potential flood threat it would then have to be combined with tidal predictions.

I will concentrate on how Corkan constructed his multiple regression formula for the wind effect, particularly the constants, which were constructed through the gradual application of the least square method to selected surges. For the final report Corkan had abandoned the six-point method and claimed to use theoretical work as "a guide to principles" only, instead relying more on empirical and statistical work.[108] He now claimed the "interesting results" he had earlier thought he had obtained using the six-point method were "largely spurious" because the method did not take into account external surges (i.e. the effect identified in figure 6.1).[109] On the basis of his chosen theory he had constructed a formula linking the wind effect, i.e. the tractive force of the wind on the surface of the sea, to east and north pressure gradients and empirical constants.[110] The earlier formula based on Nomitsu's work included variables based on the free period of the sea and the damping ratio for surges, both of which had to be approximated for the particular sea. Instead his final forecasting formula relied on

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[106] Robert Henry Corkan, *Storm Surges in the North Sea: Vol 2* (Washington D.C.: Hydrographic Office, H.O. Misc. 15072, 1948), 5.
[107] Ibid., Section 3.1.
[108] A list of references showing some of the theory Corkan read can be found in Box 59, BA.
[109] Corkan, *Storm Surges: Vol 1*, 90, 93.
[110] Ibid., Section 2.

pressure gradients and other variables directly measured and calculated (such as the barometric effect). The wind effect was calculated as the sum of contributions from different pressure gradients, one north (N and n) and one east (E and e) pressure gradients from two different points on weather charts of the North Sea, multiplied with coefficients.[111] His final formula for the wind effect took the form:

Wind effect=0.35N-0.55E-0.70n-0.95e.

Corkan determined the coefficients in this formula through multiple regression. He said the formula was built up by producing least square solutions for four carefully selected surges in the above form and then averaging the constants for these.[112]

How did Corkan choose which out of the many possible surges to base his coefficients on? Different surges would produce different coefficients which in turn would lead to different forecasts, so this was an important choice for Corkan. His choice was based on his and Doodson's belief in what made a 'good' formula: he made his choice by aiming for a particular type of formula, a 'well conditioned' one with consistent co-efficients. While not explicitly defined, to Corkan a well conditioned formula appears to have meant one which controlled for the effects of other causes of surges, such as external effects. This control of other variables was not through calculation, as is often done in a multiple regression model when mathematically controlling for other explanatory variables included in the model,[113] but by minimising other effects at the input stage. He tried to achieve control of other variables by choosing surges for which particular meteorological conditions held. He judged whether he had achieved his aim of a well conditioned formula by judging the numerical consistency of the coefficients.

To get a "well conditioned" formula, Corkan wanted to base his coefficient on surges of a particular type, with only small external effects and ones where all the pressure gradients showed a fairly large change. In addition he preferred ones with "fairly uniform meteorological conditions over the North Sea for then the pressure gradients

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[111] The values for the northern point (n and e), were taken six hours earlier than the values for southern point (N and E), to take into account the time it took for the wind effect to travel south. The practices of taking pressure gradients will be looked at further in the next chapter. Corkan, *Storm Surges: Vol 2*, Section 3.1.

[112] Corkan, *Storm Surges: Vol 1*, Part 2, section 2.

[113] Frederick E Croxton and Dudley J Cowden, *Applied General Statistics* (New York: Prentice-Hall, Inc, 1939; reprint, eighteenth printing, September 1950), 741.

will be well determined".[114] Here Corkan was looking for cases where the isobars were fairly straight, which would mean that the conditions for using geostrophic wind were fulfilled, so the problems previously encountered with using a definition of wind linked to straight isobars on curved ones would not recur. His choice to use geostrophic wind was involved in the choice of surges to calculate the correlation coefficients with, which in turn effected his final formula.

However, finding four surges to produce consistent coefficients that followed Corkan's ideal conditions was difficult, especially for those that raised sea level. None of the raising surges Corkan studied fulfilled all the conditions needed for a 'well-conditioned' equation. In the end one of the raising surges he used involved a fairly large external surge, but it was the other which required more 'fiddling' to achieve consistency. This one saw small changes in the two northern pressure gradients and the coefficients for these two were very different from those found for the other three surges. Corkan deemed the constants produced from the northern gradients in this case "badly conditioned".[115] He then replaced these coefficients with the averages from the other cases and re-fitted the eastern coefficients for that surge. For his final averaged formula he did not include the northern coefficients from this surge and used the refitted eastern ones in his calculation of the constants. He chose surges and made choices with the analysis of these surges to make the coefficients in his formula consistent.

While Corkan briefly discussed the possibility that different meteorological conditions or different types of surges (e.g. lowering and raising) would produce different coefficients, his own formula looked for consistency. He thought this consistency gave a "reasonably good representation" of wind effects, with the caveat "at least when conditions are fairly uniform".[116] In other words, he was less sure that his formula for wind effects worked well when the isobars were so curved that the assumptions for the calculation of geostrophic wind did not hold. His choice to use geostrophic winds to connect pressure gradients to the effect of wind on the sea influenced not only his definition of a well defined formula, as one whose coefficients were based on surges that met the conditions for geostrophic wind, but also the results of the work – as he was well aware. He did however think his formula produce 'good enough' results.

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114 Corkan, *Storm Surges: Vol 2*, 116.
115 Corkan, *Storm Surges: Vol 1*, 134.
116 Ibid., 141.

This again raises the issue of how Corkan judged the success of a forecast. Having constructed the predicted residual by adding the wind effect together with the various other effects, Corkan then displayed the closeness between the predicted and observed residuals graphically. He did not calculate formal statistical measures of the closeness and hardly even discussed it, instead leaving the judgement to the reader. The only case where Corkan calculated the mean square error – a formal statistical measure of closeness between observations and predictions – was for the first of his four surges. This was however done to decide the 'best' time interval between the surge and the time for which the northern gradients were taken, not to judge the fit of the forecasts. A very high correlation coefficient of 0.99 was achieved with a time interval of -6hrs, so this time interval was chosen. Otherwise Corkan tested his formula by hindcasting a large number of surges and presenting the results graphically, something which took up a large part of his final report.[117] In the end Doodson defined Corkan's work as a success and claimed that his forecasting formula was an "outstanding achievement in this kind of research".[118] To Doodson and Corkan just finding a solution, a forecasting formula, that produced predictions that were visually close to observations when graphed was success. They did not consider further analysis of the performance of the formula necessary.

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[117] There is no indication whether problematic cases were left in or taken out by him to make the formula's performance look better.
[118] Doodson to Chief Engineer, 14th Feb 1947, Box 120, BA

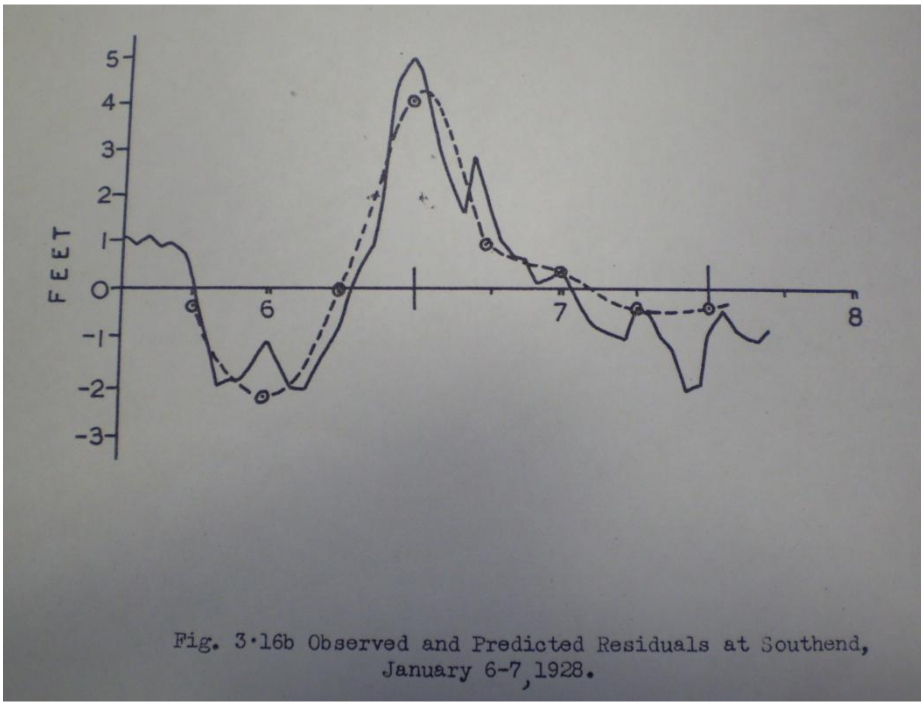

Figure 6.2: One of the graphs prepared by Corkan for his report to LCC showing the difference between the observed and predicted residual at Southend, near London, during the 1928 flood event. Many such graphs for different surges were prepared to illustrate the performance of Corkan's formula used to calculate the predicted residuals.[119]

As they had to, TI made choices, which were contingent on many things, in the work they did to find patterns in their data. These choices came to make up a tradition within which the researchers operated. To use statistics was a choice, influenced by the need for speed that came with the Hydrographer's request, which then led the way to other choices, such as which surges to choose to determine the constants. Corkan's choice in this was determined by his desire for a well-conditioned formula with consistent constants, which leads back to his and Doodson's training, influencing what they thought made a 'good' formula. For TI's researchers, operating within their own traditions, visual judgements of the 'goodness' of fit of their formula was sufficient. The next chapter turns to what happened with such judgements when TI's practice came into closer contact with other traditions.

[119] Corkan, *Storm Surges: Vol 2*, 102.

6.5 THE PATRONAGE OF POST-WAR BRITISH OCEANOGRAPHY

For the remainder of the chapter I shift back to debates regarding state patronage of oceanographic science, concentrating on the limits to the patronage TI received. While naval patronage of oceanography in Britain increased after the war, this only had a limited effect on TI and its storm surge work. Instead state actors supported the 'new' oceanography which had appeared during the war by establishing a new institute, the National Institute of Oceanography (NIO). Before discussing the establishment of NIO the section looks at the limits of the other patronage TI received, specifically that of the US Navy and LCC.

Firstly, there was a further sign of naval interest in storm surge science when Corkan's final report on the LCC project was appropriated by the Navy. It was sent to LCC in February 1947, when Doodson also mentioned that "the matter of publication is of some moment to" TI, presumably to ensure they got recognition for their work.[120] However, the publication of the report became a complicated affair. Out of the blue, the Royal Navy Scientific Service put in a request to LCC on behalf of the UK and US Navies to be allowed to copy the report, which was granted without consulting TI.[121] TI's only full copy of the report, which had been sent to LCC, was lent to the Royal Naval Scientific Service, which in turn gave it to the American Embassy. After it had been sent off both TI and LCC discovered errors in it, which meant that corrections and different versions of the report were sent between TI, LCC and the Navy representatives. It was eventually reproduced in Washington, DC, by the US Hydrographic Office with copies given to a range of naval, oceanographic and meteorological actors in the US (e.g. the US Weather Bureau, the Office of Naval Research, the Scripps and Woods Hole oceanographic institutes, and the US Coast and Geodetic Survey).[122] While the report gave Corkan as the author and mentioned where the work had been done and who had funded it, it received only limited circulation in the UK. The usage or readership of the report is unclear, but at this time a wider US-led exchange of scientific information between the UK and the US naval authorities was

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120 Doodson to the Deputy Chief Engineer, 1st Feb 1947; Doodson to Chief Engineer, 14th Feb 1947, Box 120, BA

121 "Storm Surges in the North Sea, Housing Committee, Report by the Chief Engineer", 15th May 1948, LCC.CL.MD.1.115, LMA

122 See correspondence between 1947 and 1949 in Box 120 and 130, BA, and also in folder LCC.CL.MD.1.115, LMA.

taking place.[123] The publication of the report seems to have been caught up in this early cold war military-scientific exchange, indicating that TI's storm surge science was to some extent part of the increased interest shown in oceanography by the UK and US Navies after the war. However, it was only a partial interest involving no funding, so this episode is a further example of the limits to naval patronage of, or interest in, TI's storm surge science.

In early 1948, while the publication of Corkan's report was being negotiated, Doodson was asked by the new LCC Chief Engineer, J Rawlinson, to give his thoughts on forecasting storm surges in the Thames Estuary and providing warnings of them.[124] In response he outlined a scheme to organise a predictive network of calculation, suggesting TI as a suitable central computing office for the potential warning system.[125] Following this exchange Rawlinson wrote a report to LCC's River and Drainage Committee, recommending that he himself do some further surge-related work on the height of flood defences and the impact of inland flood water. The committee accepted this recommendation and also asked him to look at increasing the time before a potential flood a warning was given, which led to a meeting with Doodson being arranged. However, the chief engineer's work primarily led to him again recommending he himself needed to do further work and not that Doodson's suggestions were implemented.[126] During this period the earlier warning system set up after the event in 1928 continued.[127] Despite TI claiming to have produced successful forecasting formulae, these were not taken up by the warning system. While also connected with the fact that the earlier supporters within LCC, such as Frank and Silkin, had moved on, this exemplifies another limit to the patronage TI's storm surge science received, as, unsurprisingly, patronage of research was insufficient to put the results of that research into use.

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[123] For example, the file ADM 116/5670, NA, contains lists of a large number of scientific reports, primarily produced by naval authorities, which were exchanged between the UK and the US via the Royal Navy Scientific Service 1947-9. TI's report does not appear in this list, probably because it was not produced by an Admiralty organisation.
[124] Doodson to Chief Engineer at L.C.C, 1st Mar 1948, Box 120, folder 7, BA
[125] "Note on the prediction of storm surges in Thames Estuary", Box 120, folder 7, BA
[126] Notes and memos in LCC/CL/MD/1/115, LMA. On the arrangement of a meeting with Doodson, see Rawlinson to Doodson, 23rd Nov 1950, Box 120, BA; and Rawlinson to Doodson, 4th Dec 1950, Box 120, BA
[127] See documents in LCC/CL/MD/1/116, LMA

The institutional landscape of British oceanography changed in 1949 with the establishment of NIO. While TI and Proudman were closely involved in the process of establishing NIO, it had little to do with storm surge science or the development of statistical forecasting formulae, so will only be very briefly summarised here, focusing on the results in terms of patronage and location. In the end these post-war changes also led to few changes at TI, something that contrasts with other case studies in the history of oceanography. As discussed in the introduction, several historians have argued that after the Second World War there was a dramatic shift towards military patronage for physical oceanography in the US.[128] This did not happen at TI. However, as in the US there was strong naval and state interest in oceanography in the UK following the war, but this interest and funding went to another institution favouring a different kind of oceanography: NIO.

During the Second World War, on the suggestion of Hydrographer Edgell and Proudman and with the blessing of the Scientific Advisory Committee to the War Cabinet, debates organised within the structure of the Royal Society began on how oceanography should be organised and funded after the war.[129] The eventual report by the Society committee, chaired by Hydrographer Edgell, favoured an Institute focused on physical oceanography, to be based at Liverpool and in receipt of central government funding from an unspecified Whitehall department. This was what Proudman had argued for. However, there had been strong arguments for a more integrated institution, incorporating more biological oceanography, during the committee's discussions. George ER Deacon (1906-1984), who was mentioned earlier while discussing the wave and swell forecasting network, was one of the key supporters of widening the remit of NIO. Deacon had done much biological oceanography before

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128 Hamblin, *Oceanographers and the Cold War*; Weir, *Ocean in Common*; Schlee, *A History of Oceanography*; Rainger, "Constructing a Landscape for Postwar Science."; Rainger, "Adaptation and the Importance of Local Culture."; Doel, "Constituting the Postwar Earth Sciences."; Oreskes and Rainger, "Science and Security before the Atomic Bomb."; Mills, *The Fluid Envelope of Our Planet*; Mukerji, *A Fragile Power*.
129 For more details on the establishment of NIO, see the work by George Deacon's daughter, Margaret Deacon, "Steps toward the Founding of NIO," in *Of Seas and Ships and Scientists : The Remarkable Story of the UK's National Institute of Oceanography 1949-1973*, ed. Anthony Laughton*, et al.* (Cambridge: The Lutterworth Press, 2010). My research on this was done before the publication of her work. While her work is more detailed than the version I provide here, it is also somewhat more celebratory and naturally focused on NIO's side of the story, while I am concentrating on TI's point of view.

the war and remained with the Navy after the war, leading the Admiralty Research Laboratory in Teddington.[130]

When the Royal Society report entered the Whitehall machinery in late 1944 the debates on what kind of institute NIO should be and who should fund it were revisited, and the much re-worked NIO that eventually came into being in 1949 included much more biological and naval work than the original Royal Society report had visualised, was funded primarily by the Admiralty and the Colonial Office, and had Deacon as its Director.[131] These changes were outside the influence of TI's circle, despite various attempts at influencing the debates. The location of the new NIO was left hanging for a few years, during which Proudman, Doodson and certain members of MDHB repeatedly argued for it to be located in Liverpool and merged with TI, implicitly providing TI with a secure future through increased state patronage, while Deacon and others argued for it to be located near London. In 1951 NIO finally acquired a headquarters, based in Wormley, in a rural location south of Guildford, which suited those arguing for a London location.[132]

There were any number of issues involved in the establishment of NIO, such as the kind of geographies that were most conducive for oceanographic research or which part of the state should support oceanographic research, but one is especially important for this thesis.[133] These debates were not simply about where the headquarters of the NIO should be, but about what kind of oceanography the NIO should be doing and in

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130 H. Charnock, "George Edward Raven Deacon. 21 March 1906-16 November 1984," *Biographical Memoirs of Fellows of the Royal Society* 31(1985).
131 The report suggesting NIO was sent to the Scientific Advisory Committee to the War Cabinet, whose members also met as the Colonial Research Committee, which amongst other things supported fisheries research and the Discovery Committee, see Clarke, "A Technocratic Imperial State? The Colonial Office and Scientific Research, 1940 - 1960," 467. This provided a high-level connection between the proposed NIO and the Discovery Committe and may be one reason why the Scientific Advisory Committee argued for the inclusion of the Discovery Committee's work into NIO.
132 Files in the National Archive on NIO used to prepare this summary are ADM 116/5715 and CO/927/39/2, NA. The files in the UKHO archive include much useful information, including copies of the Minutes of the Royal Society Committee not kept in London, see H 02455/43, H 6203/45, UKHO. While there are mentions of the establishment of NIO in Royal Society archives (primarily in relation to the Post-War needs committees) and the Churchill Archives Centre (Bullard papers), for example in minutes of meetings, there are few in-depth documents. In terms of the Royal Society documents, see for example "Reports from the sectional committees and the agricultural science committee upon the needs of special subjects in the balanced development of science in universities of the United Kingdom", p7, AE/1/9/7, RS. For minutes, agenda papers and correspondence regarding the National Committee of Geodesy and Geophysics (not discussing the oceanographic sub-committee in any detail), see AE 1/9/11, AE/1/9/4 and AE/1/11/21. For why it was not dealt with in detail, see AE 1/9/9.
133 I hope to develop this episode into a journal article after the PhD.

extension what kind of oceanography the state should be supporting. Should NIO focus on mathematically based oceanography, with an emphasis on the type of physical oceanography which Proudman and Doodson were doing, or should it instead have a more 'integrated' approach to oceanography, including biological oceanography and more of the 'new' naval oceanography that had been done during the war, such as the wave and swell work? In the end, NIO focused on the integrated approach, which more directly incorporated naval and state interests as well as personnel from those sectors. NIO's patronage structure, with its high dependence on naval funding and involvement of actors such as Deacon with close links to naval oceanographic work during the war, is thus an example of the trends identified by historians of US oceanography, while TI is something of a counter example. Despite its close links to the Admiralty and its role as the state's provider of tidal prediction, TI was excluded from the post-war increase in naval funding of oceanographic research. While its income from tidal predictions increased (see section 4.5, especially table 4.1), it received no further state funding.

NIO's eventual focus on 'integrated' oceanography represented a side-lining of the Proudman/Doodson kind of oceanography with its focus on a particular kind of mathematical practice. It also represented a potential crisis for TI in terms of patronage, as the process had earlier led to a frustrated MDHB applying to Parliament for permission to no longer run an Observatory, i.e. to no longer fund TI, and this application had been granted.[134] However, in the end MDHB changed its mind about relinquishing TI. The establishment of NIO in the south appears to have led to sufficiently sore feelings in Liverpool that TI's existing patrons now were determined not to see LOTI disappear or merged with NIO, even if this meant they had to part with more cash for research and allow a representative of NIO – Deacon – onto the governing committee. This especially held for Sir John Hobhouse, one of the MDHB members who had earlier argued for NIO to be located in Liverpool.[135]

In the end NIO's foundation thus re-affirmed TI's overall patronage structure, though the funding provided by the Dock Board and the University was in fact somewhat increased (see table 4.1). This came after Doodson had argued that in order to retain TI's prestige and expertise – so as not to eventually lose the financially lucrative analysis

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[134] Material on the NIO debate from the Liverpool perspective is held in D/BO/1/4/17, MMM - North Street, in Box 130, BA, and in P744/4, LUA.
[135] Hobhouse to Sir John Lang, Admiralty, 19th Nov 1948, D/BO/1/4/17, MMM - North Street

and prediction work – increased funding had to be given for research, including on storm surges.[136] The LOTI committee accepted this argument, with Hobhouse indeed emphasising TI's financial independence from NIO, and TI's funding from their Liverpool patrons was increased.[137] This meant that Liverpool's rejection as the site of NIO became not only a rejection of increased funding to TI by military and state actors, but also a re-affirmation of TI's independence by its Liverpool patrons. In turn this meant that TI could go on much as it had before, even increasing its research somewhat, without receiving state patronage. The institutional landscape as well as the patronage structure of British oceanography was diverse and contested in the post-war period.

6.6 Conclusion

This chapter has looked at a number of debates and tensions regarding civil and military state involvement with storm surge science. In the 1930s the Treasury refused to see storm surges as a national issue, despite repeated arguments from engineers, scientists and others that it was one, and in the end it remained a local government interest, though with some support from the Navy. While the war led to increased naval interest in TI's storm surge science, the UK and US Navies appears to have appropriated the work but not put it into use, instead concentrating on work on wave and swell. Finally when NIO was established TI's version of oceanography was side-lined for state support in favour of an 'integrated' oceanography in part descending from the wave and swell work during the war.

In this period there was thus considerable tension regarding the role of the state in relation to storm surge science. What science should the state support? Some actors, e.g. LCC's engineers and the tidal branch of Hydro, clearly believed storm surge science could help the state forecast flooding and help in naval battles, and that this was a valid reason to give it central government support. Others did not see this potential or the need for it, and did not consider this sort of science a high priority for central

[136] Memorandum concerning the future of Liverpool Observatory and Tidal Institute, Doodson, 6th Mar 1951, D/BO/1/4/17, MMM - North Street
[137] Minutes of meeting of LOTI committee, 16th Mar 1951, D/BO/1/4/17, MMM - North Street

government to use or support. These others generally won out. The increase in technological and scientific government of the sea was a slow and contested process.[138]

What effect did these patronage debates have on TI's work? The project was from the start aimed at producing a forecasting formula, which followed directly on the earlier local authority funding and concern with flooding. However, the debates between LCC, the Ministry of Health and the Treasury regarding whether storm surge science should be supported as a national issue or a local one had little impact on TI's subsequent work apart from eventually getting it started. During the project TI received little direction from LCC and indeed very little communication at all. This meant that TI was able to define the aims of the project and how to get there without much involvement from LCC. However, when the Navy got involved the work changed track. Both the work for LCC and the war-work for the Navy shared the same goal, defined by TI and not questioned by LCC: being able to forecast sea level by simple calculations and reference to meteorological charts. What changed were the methods TI used to attempt to achieve this. Before the Navy's request they had been experimenting with different methods of forecasting the surge, concentrating on the six-point method, but in order to provide the forecasting formula to the Navy they changed towards statistical methods. They then stayed on this track all the way through the rest of the LCC project. The change of method was not solely due to the request for a forecasting formula produced quickly, as they were also encountering problems with their earlier method, but it is clear that it was a key reason behind the change in methods.

Choices such as this change of method influenced later work by leading to TI's workers creating particular chains of documents, which bound as well as connected. While Corkan to some extent chose which chains to be bound by, for example by initially choosing the six-point method and later to aim for a 'well conditioned' multiple regression formula, his choices were influenced by contingencies such as Hydro's request. His choices were also limited by earlier choices, such as the definition and construction of surges as residuals or the choice to use geostrophic winds. TI's researchers did not necessarily know which choices would result in a 'supplement' for

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138 Armytage, *The Rise of the Technocrats*; Edgerton, *Warfare State*. Also compare Whitehead, *State, Science, and the Skies*.

them, so their choices cannot be reduced to a will-to-power but were instead contingent on a range of issues.

CHAPTER 7 (1953-60), THE EAST COAST FLOOD: THE IMPACT OF STATE PATRONAGE ON STORM SURGE RESEARCH

In early 1953 over three hundred people died in a storm surge flooding event on the East Coast. Like the event in 1928 this one led to various changes for the Tidal Institute (TI) and its work on storm surges. For the first time central civil government, as opposed to local government or the Navy, became a patron of the work. This central government patronage came with a new framework, through which a range of actors were brought together to co-ordinate and conduct the research. Within this framework TI constructed statistical forecasting formulae that were put into use by the newly created East Coast flood warning system. While these formulae were deemed as good as was possible with their statistical methods, TI's practices – especially their choice of formula to connect wind and sea – were questioned both by themselves and others. After this questioning TI both argued for change and began to change their practices. Both the intensification and the questioning of TI's statistical practices for creating forecasting formulae came together with the changed patronage structure. Throughout I emphasise the contested, complicated and partial nature of the patronage of TI's storm surge science and, more generally, British oceanography. The key question for this chapter is how the contested response to the flooding event impacted on TI's work, both leading to its 'fruition' and to it being questioned.

The chapter starts by analysing why there was a change towards state patronage, arguing that a combination of belief in science as a problem solver and political pressure from the opposition were key in this development. While much of the secondary literature argues that there was little contemporary debate about responsibility after the event, I will emphasise that political pressure on the government from parts of the opposition was strong. This pressure impacted on policy, leading to relative government generosity in terms of grants and also to the creation of an investigation into the event, called the Waverley Committee. Belief in planned science and technology as a problem solver also had a role, with the technical members of the Waverley Committee being important in formulating its recommendations. The technical members formed a sub-committee which recommended further research, arguing this would improve the warning system. However, this belief in science and technology as a problem solver was not uncontested. The Cabinet does not seem to have been motivated by such beliefs, and when the sub-committee's report did the rounds of various government departments the worth of the proposed research was discussed. It was eventually picked up by research-friendly officers within the Ministry of Agriculture and Fisheries (MAF, from 1955 MAFF)[1] who set about implementing it well before the publication of the Waverley Committee's final report. They established the Advisory Committee of Oceanographical and Meteorological Research, which then formed the framework for TI's work on storm surges, including forecasting formulae.

TI's work initially retained its emphasis on statistics, leading to the publication in 1959 by one of its workers of a set of statistical forecasting formulae. I will discuss how these formulae were deemed as good as possible using these particular practices of calculation. However, by the mid 1950s both TI's researchers and others at the Advisory Committee were finding problems with the Institute's methods, especially with TI's practices of calculating winds, and a debate broke out about whether TI's formulae should be based on statistical correlations or on more theoretical work. TI suggested that other, non-statistical, practices of calculation had to be attempted to improve prediction, but the Advisory Committee in the end prioritised statistical formulae. I analyse TI's practices of calculating winds, looking at the various choices involved in this and the consequences of these. Following both the debates on TI's practice and the provision of formulae by them, by the end of the 1950s TI was beginning to shift towards new practices of calculation, by employing digital computers for the calculations of one particular aspect of their research for the Advisory Committee.

7.1 The East Coast Flood and the political response

On Saturday 31 January a cyclone passed north of Scotland before turning south. Its unusually intense pressure gradient led to extremely strong winds and a major storm surge together with high waves.[2] The surge travelled down the East Coast, starting at

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[1]The Ministry of Agriculture and Fisheries, MAF, was in 1955 merged with the Ministry of Food, becoming the Ministry of Agriculture, Fisheries and Food, MAFF.

[2] This summary is primarily based on Peter J. Baxter, "The East Coast Big Flood, 31 January-1 February 1953: A Summary of the Human Disaster," *Philosophical Transactions of the Royal Society A: Mathematical,*

Spurn Head at 4pm, reaching King's Lynn and maximum height at 7.20pm and Canvey Island at 1.10am, Sunday morning. It caused major flooding, as sea defences were breached in over 1,200 places, either by direct wave action or by overtopping and erosion. The timing of the surge, arriving after dark and late in the evening, especially further south, increased the deadliness of the event, and in England 307 people died, either of drowning or exposure. Most of those who died were over sixty and lived in post-war wooden or prefabricated single-storey buildings, some intended only for summer inhabitation. The impact was much worse in the Netherlands where 1795 people died. No public warnings were given, though the Met Office, as part of the Thames surge warning system, had given some warning to at least some River Boards.[3] Most of the immediate search and rescue operation was organised locally with the help of the US and UK army which had a strong presence in the area.[4]

On Monday a major government response was initiated. The repair of sea defences before high spring tides in mid-February to stop further flooding was a major logistical challenge, involving 30,000 workers, half from the UK and US military, and masses of material and machines, but was deemed successful. The cost of the damages to the defences was estimated at about £30 million by the Home Secretary at the time, Maxwell Fyfe. In addition, telephone lines, electricity, gas, water, sewage and drainage had been disrupted, 24,000 houses and over 200 industrial premises needed repairs and

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Physical and Engineering Sciences 363, no. 1831 (2005): 1302. Some historical work has been done on the event, the most thorough but also oldest and geographically restricted is that written by County Council of Essex's senior assistant archivist, Hilda Grieve, *The Great Tide : The Story of the 1953 Flood Disaster in Essex* (Chelmsford: County Council of Essex, 1959). Shorter, more popular accounts are given in HJ Harland and MG Harland, *The Flooding of Eastern England* (Peterborough: Minimax Books, 1980); Michael Pollard, *North Sea Surge: The Story of the East Coast Floods of 1953* (Lavenham: Terence Dalton Ltd, 1978); Dorothy Summers, *The East Coast Floods* (London: David & Charles, 1978). Until recently academic historians have paid little attention to the event. For this recent work see Frank Furedi, "From the Narrative of the Blitz to the Rhetoric of Vulnerability," *Cultural Sociology* 1, no. 2 (2007); Clare L. Johnson, Sylvia M. Tunstall, and Edmund C. Penning-Rowsell, "Crises as Catalysts for Adaptation: Human Response to Major Floods," in *Flood Hazard Research Centre, Publications* (Middlesex: Flood Hazard Research Centre, 2004); Alexander Hall, "The Rise of Blame and Recreancy in the United Kingdom - a Cultural, Political and Scientific Autopsy of the North Sea Flood of 1953," *Environment and History journal* (forthcoming spring 2011).

[3] See e.g. "Flood Warnings," *The Manchester Guardian*, Feb 5 1953, 4. The Met Office warnings are often not noted in secondary sources.

[4] Secondary sources have portrayed central government as not getting involved until Monday, leaving local people to fend for themselves during the weekend (often described as doing so heroically and successfully), but the Guardian reported that Prime Minister Churchill, as well as his Ministers of Housing and Local Government (Harold Macmillan) and of Health (Iain MacLeod) had been kept informed throughout Sunday and had been involved in organising the response via the government's regional offices. "Minister Acts," *The Manchester Guardian*, Feb 12 1953; "Statement Today by Mr Churchill," *The Manchester Guardian*, Feb 12 1953. In addition, the involvement of the army in the rescue officers implies state involvement, even if the response was organised on the ground.

32,000 people were evacuated. 160,000 acres of land were flooded by salt water and 46,000 livestock died.[5]

How and why did the government become a patron of storm surge science after this event? This change in patronage impacted on TI's continued surge research by establishing a particular framework. Secondary sources discussing the event have not analysed why the Waverley Committee was set up. For example, Clare Johnson, Edmund Penning-Rowsell and Sylvia Tunstall, who have compared the policy response to four major English flood events in 1947, 1953, 1998 and 2000, seem to assume that inquiries, like the Waverley Committee or the Bye Report after the flood in 1998, are a natural and good policy response after a major flood. They do not analyse why such inquiries were established after events in 1953, 1998 and 2000, but analyse in considerable detail the lack of such an inquiry after the one in 1947.[6] However, the lack of an inquiry after the flood in 1947 and the delay of many months in setting up such an inquiry after the 1952 London smog disaster in which around 4000 died highlights that the creation of an inquiry was not necessarily a straightforward or obvious response to a weather-related disaster at the time.[7] The quick creation of it, and, in extension the creation of the mechanisms through which TI's research was done after the event, requires just as much an explanation as does the lack of an inquiry after 1947. In addition, secondary sources have deemphasised the allocation of blame and responsibility as a reason for the government response to the event.[8] For example, Peter Baxter argues there was a lack of public apportioning of blame to government and individuals "[d]espite the absence of warnings and the deficiencies of defences".[9] While Johnson, Penning-Rowsell and Tunstall link controversy to the creation of the new warning system, they do not link this to the creation of the Waverley Committee.[10]

The scale of the event clearly had a key role in the government's financial response to the event, but I will argue that political pressure also played an important role in the

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[5] Baxter, "The East Coast Big Flood." Baxter's figures and the estimate of cost are taken from the Waverley Committee's report, *Report of the Departmental Committee on Coastal Flooding [Waverley Committee]*, Cmd. 9165 (London: HMSO, 1954), para 22.
[6] Johnson, Tunstall, and Penning-Rowsell, "Crises as Catalysts for Adaptation," 88-93. On 1947 see section 4.4.1, p 88-93.
[7] On the smog, see Whitehead, *State, Science, and the Skies*, 142.
[8] Baxter, "The East Coast Big Flood," 1306 & 1311; Furedi, "From the Narrative of the Blitz to the Rhetoric of Vulnerability."; Frank Furedi, "The Changing Meaning of Disaster," *Area* 39, no. 4 (2007).
[9] Baxter, "The East Coast Big Flood," 1310.
[10] Johnson, Tunstall, and Penning-Rowsell, "Crises as Catalysts for Adaptation," 79-82.

creation of the Waverley Committee and in extension the giving of grants to TI's research. There was in fact a substantial amount of discussion regarding the responsibility of the government after the event. While the leader of the Opposition, Clement Attlee, was supportive of the government's response effort and assisted it,[11] more leftist members of the Labour Opposition, led by Attlee's rival Herbert Morrison[12] and Aneurin Bevan, did not hesitate to raise issues of blame. They questioned whether a government circular from 1952 limiting the use of steel for flood defences had caused weaknesses in them and whether the government was willing to incur expenses by requisitioning houses to use for the evacuees. In response the government defended its actions as sufficient.[13] Throughout February and March issues like this bubbled up.[14]

In late March the Opposition's questioning of the government's actions came to a crescendo. On the Monday after the event Prime Minister Churchill had declared "that the catastrophe is one which will require to be treated upon a national basis and, broadly, as a national responsibility", a statement frequently discussed in the political debates following the event.[15] On 18 March a statement to Parliament by Fyfe, Home Secretary and co-ordinator of the government's response, led to a lively debate, with questions and comments from several Labour members, saying that Fyfe's statement breached Churchill's earlier promise by not providing sufficient financial assistance.[16] After a few days the Opposition backed down, with Mr Edward Evans, the MP for flood-affected Lowestoft, declaring the government's actions "timely, decent, and not ungenerous".[17] Despite the end result, this and the earlier exchanges show that there were substantial debates regarding blame and responsibility, both past and future and in

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[11] E.g. by 'pairing' MPs. See also Attlee's positive statement on the response of the public: Hansard, "Flood Disasters", HC Deb 19 February 1953, vol 511, c1458, http://hansard.millbanksystems.com/commons/1953/feb/19/flood-disasters#S5CV0511P0_19530219_HOC_297, accessed 23 July 2010

[12] He had been the Leader of LCC in the 1930s during the discussions regarding the funding of further surge research after 1928.

[13] "Refugees' Hosts to Get State Pay," *The Manchester Guardian*, Feb 7 1953; "Requisitioning to Receive 'Most Urgent Consideration'," *The Manchester Guardian*, Feb 4 1953.

[14] See for example "Flood Losses," *The Times*, Mar 11 1953.

[15] Hansard, "East Coast Flood Disaster", HC Deb 02 February 1953, vol 510, c1481, http://hansard.millbanksystems.com/commons/1953/feb/02/east-coast-flood-disaster#S5CV0510P0_19530202_HOC_231, accessed 23 July 2010

[16] "Mr Ede's Dismay," *The Times*, Mar 19 1953; "Labour's Challenge on Flood Relief," *The Manchester Guardian*, Mar 19 1953.

[17] "Labour Withdraws Charge against Premier," *The Manchester Guardian*, Mar 24 1953.

particular about the role of the government in the response, in the wake of the 1953 event.

The political pressure from these debates about blame was keenly felt by ministers and impacted on its financial policy regarding the event, making the Treasury seemingly more generous than it had previously been towards storm surge science, such as during the interwar debate with LCC. The impact of the opposition's noises on senior figures in government can be clearly seen in a secret memo to the Chancellor from the Financial Secretary, John A Boyd-Carpenter. Early on the Treasury had agreed to double whatever the public contributed to the so-called Lord Mayor's Fund, the key channel for distributing charitable aid to those affected by the flood. Initially the Chancellor, Richard Austen Butler, had wanted to stop these payments when the government's contribution had reached £2m. These contributions were now well beyond this sum, as while the Chancellor had been away Boyd-Carpenter and Second Secretary B Gilbert had decided any such stop would have been unwise, and Boyd-Carpenter argued this was still the case: "any attempt to do so would both provoke a major row and stimulate even greater demands that the Exchequer should accept liability for various forms of loss". It was "politically impossible" to do such a thing.[18] They argued that providing more money to start with would lead to the Treasury's bill being lower in the end. The Treasury appear to have adopted the same attitude towards the implementation of the Waverley Committee's recommendations, such as the research TI did.

Another example of the impact the political pressure had on funding issues is the Cabinet discussion of Fyfe's draft statement to Parliament, mentioned above, on 17 March. The Chancellor questioned the high Exchequer liability: "Is it really a national disaster"? If it was, he agreed they must pay up. In response Fyfe argued that the event was "unprecedented in our history", adding that the political pressure was "v. strong".[19] This exemplifies how the scale and framing of the event as a 'national' unprecedented disaster together with political pressure impacted on government funding. The main issue during the Cabinet debate on Fyfe's March statement was two possible paragraphs regarding reimbursements to be given to local authorities. Though the Chancellor

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[18] "Floods", memo by JA Boyd-Carpenter to Chancellor of the Exchequer, 14th Mar 1953, T 227/312, NA

[19] CC 20(53), Notebooks, Cabinet meeting held on 17th Mar 1953, minute no 10, CAB 195/11, NA

claimed to be willing to consider paying more than was implied by the less generous version, he did not want to promise it in advance. The Cabinet in the end chose the less generous of the two paragraphs, leading to the criticism from the opposition of Fyfe's statement mentioned above.[20] The Treasury tried to limit the government's contributions, but felt there were stricter than normal limits imposed by the political difficulties such reductions could cause.[21] This meant that much of the Treasury debates were more about allocation and organising of money than about putting severe limits on spending.[22] The political pressure thus impacted on funding decisions, such as whether TI should get state funding for their work.

These debates about funding were however only background to the Waverley Committee. Johnson, Penning-Rowsell and Tunstall have argued that a belief in the ability of science and technology to solve problems and inform policy, fostered during the recent war, meant that there was no debate regarding the funding of research or the capability of research to produce the sought-after answers after the 1953 event. They argue that the Waverley committee's recommendations, over which they say scientific members had a strong influence, were not openly debated or questioned but instead accepted because of this belief in science.[23] This chimes with others' portrayal of the post-war period, with a substantial literature arguing for an increase in state patronage of research during the 1950s.[24] Jon Agar has summarised this literature: "The increased funding [of science] reflected the post-war regard for 'boffins' and for rational planning, both popularly and within government", also arguing that this depended on and continued networks between government, military and academic science developed

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[20] CC (53), 20th Conclusions, Cabinet meeting held on 17th Mar 1953, minute no 11, CAB 128/26, NA. See also C.(53) 104, "Flood damage," Memorandum by the Secretary of State for the Home Department and Minister for Welsh Affairs, 16th Mar 1953, CAB 129/60, NA, which includes the alternative paragraphs.

[21] See also Minute on "Flood Damage" by JG Owen to Mr Jenkyns, 16th Mar 1953, T 227/312, NA

[22] For examples, see documents in T 277/311, NA. These deal primarily with contributions towards the rebuilding effort and compensation to individuals and small businesses, and do not mention payments related to the warning system or the research. I have not been able to locate any Treasury files on these latter topics, but the costs for these were much smaller and there is nothing to indicate the overall argument did not apply to them too.

[23] Johnson, Tunstall, and Penning-Rowsell, "Crises as Catalysts for Adaptation," 84-85 & 115-120.

[24] Armytage, *The Rise of the Technocrats*, ch 1, 16 & 21; Soraya de Chadarevian, *Designs for Life : Molecular Biology after World War II* (Cambridge: Cambridge University Press, 2002), ch 2; Whitehead, *State, Science, and the Skies*, esp. ch 6; Edgerton, *Warfare State*, 103-107; Philip Gummett, *Scientists in Whitehall* (Manchester: Manchester University Press, 1980), 37-40.

during the Second World War.[25] It is thus well documented that belief in science and planning as problem solvers was strong in Britain in the 1950s and that government investment in science and technology increased dramatically in this decade.

However, the government's response to the 1953 flooding was not that of a believer in science as a problem solver. Instead the Cabinet used the supposed objectivity of science as a support mechanism to defend and protect itself from its political opponents.[26] During a Cabinet meeting in mid-February, discussing the size of government contributions toward the rebuilding effort, Fyfe mentioned that he was going to announce the appointment of what became the Waverley Committee "to consider what long-term measures should be taken to guard against a recurrence of flooding on the rare occasions when tide and wind conditions were the same" as during the event.[27] Fyfe used the rarity of the event, as having happened "3 times in 1,000 yrs", to argue that they "must be careful to see how far we shd. go".[28] Fyfe wanted to appoint an expert committee to ensure that the costs of the response to the event did not exceed the benefits by going too far in the building of further flood defences or the creation of a warning system, given the rarity of these events. The announcing of the expert Waverley Committee, which later suggested that TI should be contracted to do surge science work, did not reflect a Cabinet belief in science, but was instead a cost-limitation exercise in response to the political pressure for high government assistance following the event.

This section has introduced the impact of the event and argued that political pressure impacted on the political response to the event, producing a seemingly generous Treasury attitude and the creation of an expert inquiry, both in fact thought to be long-term cost saving measures. I have downplayed the importance of beliefs in science and technology as problem solver at the Cabinet level. However, while the political pressure appears to have provided an unusually receptive Treasury response to funding requests, this does not explain the form of the framework within which TI did its work. This was instead contingent upon debates within the Waverley Committee and different

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[25] Jon Agar, *Science and Spectacle: The Work of Jodrell Bank in Post-War British Culture*, vol. 5, Studies in the History of Science, Technology and Medicine (Amsterdam: Harwood Academic, 1998).
[26] Compare Ted Porter's argument that threatened or weak organisations (or scientific disciplines) are more likely to refer to objectivity and scientific rules than less threatened ones. Porter, *Trust in Numbers*.
[27] CC (53), 12th Conclusions, Cabinet meeting held on 17th Feb 1953, minute no 7, CAB 128/26, NA
[28] CC 12(53), Notebooks, Cabinet meeting held on 17th Feb 1953, minute no 7, CAB 195/11, NA

government departments, in which different beliefs in the value of science were an important consideration. The next section will look at the committee Fyfe was announcing, which eventually created a framework for further research into forecasting storm surges.

7.2.1 The creation of the Advisory Committee on Oceanographical and Meteorological research

This section explains how the government's patronage of TI's surge science came to take the form it took and how a new framework for storm surge science was set up. It does this by outlining the creation of the organisation that commissioned the work, placing the Advisory Committee within a wider framework of departmental discussions regarding whose responsibility its research should be and what the value of it was. The end result was an Advisory Committee on Oceanographical and Meteorological Research under MAF. While the setting up of such a committee was one of the recommendations of the Waverley Committee, the creation of it actually came about slightly differently, with the report of a scientific sub-committee of the main Waverley Committee doing the rounds of various departments, eventually being picked up and implemented by MAF. The main effect of the Waverley Committee's final recommendation was the relatively minor one of making a scientist the chair of the proposed committee. The entire process exemplifies the contested nature of central government support of storm surge science and how the work later done depended on personal networks.

The Departmental Committee on Coastal Flooding, the committee established by Fyfe, is usually called the Waverley Committee after its chair Viscount Waverley. Initially when the Hydrographic Department (Hydro) was consulted on the draft membership of the Waverley Committee it did not contain a "tidal expert", something which Hydrographer Day and Commander WI Farquharson, the current Tidal Super-Intendent at Hydro, were unhappy with. Arguing that this lack was likely to lead to questions of the legitimacy of the committee in the eyes of other scientists, the Hydrographer successfully suggested the addition of Joseph Proudman, whom he already knew.[29] While Proudman had stepped down as Director of TI in 1946, when Arthur Doodson took over, he had remained involved with TI.[30] The final committee consisted of a mix of Lords, former politicians and civil servants, as well as engineers and four scientists.[31] The Waverley Committee's terms of reference included examining the causes of the flooding, what lessons could be learned, in particular regarding physical sea defences, and whether a further warning system should be set up.[32] These questions concentrated on scientific and technological matters, and also included planning-related matters such as how to organise the management of flood defence. The intervention of scientifically-inclined Departments such as Hydro, who ensured Proudman's place on the Committee, increased the scientific membership of the final version of the committee.

The Waverley Committee issued two reports: first, an interim report in the summer of 1953 on the development of a warning system, and, second, a final report in the spring of 1954, which included the report of a Sub-Committee suggesting the establishment of the Advisory Committee. These reports together with departmental discussions led to the creation of the framework that TI did its surge research within following this event. What was the reason for the research suggested by the Waverley Committee, what did they suggest should be done and how was the framework for this work then actually established?

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29 Documents in H 01041.53, UKHO. The phrase tidal expert was used repeatedly, see e.g. Minute by Hydrographer Day to Secretary, 13th Mar 1953 and Proudman to Farquharson, 13th mar 1953, both in H 01041.53, UKHO.

30 Proudman to Hobhouse, 19th Nov 1945, P744/4, LUA; and Hobhouse to Proudman, 21st Nov 1945, P744/4, LUA. LOTI, *Annual Report 1946*, 3.

31 The names, titles and affiliations of the members of the Waverley Committee were Right Honourable Viscount Waverley GCB GCSI GCIE FRS, Dr GMB Dobson CBE FRS (Reader in Meteorology in the University of Oxford), Sir Donald Fergusson GCB (former Permanent Secretary of the Ministry of Fuel and Power, and Ministry of Agriculture and Fisheries), Mr RD Gwyther CBE MC (partner in Messrs. Coode and Partners, chartered civil engineer), Sir Claude Inglis CIE FRS (Director, Hydraulics Research Station, Department of Scientific and Industrial Research), Mr RG Leach CBE (partner in Peat, Marwick, Mitchell and Company, former Deputy Finance Secretary of Ministry of Food), Major Sir Basil Neven-Spence (Lord Lieutenant of Zetland), Proudman, Mr AS Quartermaine CBE MC (President of the Institution of Civil Engineers and lately Chief Engineer of the Great Western Railway), Lord De Ramsey TD (Lord Lieutenant of Huntingdonshire), Professor JA Steers (Professor of Geography at the University of Cambridge), Sir Miles Thomas DFC (Chairman of British Overseas Airways Corporation), Sir John Wrigley KBE CB (former Joint Deputy-Secretary of the Ministry of Housing and Local Government), Mr T Yates CBE (General Secretary of the National Union of Seamen). Affiliations taken from Hansard extract "Flood and Storm Damage" from 23rd Mar 1953, in MAF 135/341 and Hansard, "Flood Disaster Committee (Membership)", HC Deb 23 April 1953, vol 514, c1395, http://hansard.millbanksystems.com/commons/1953/apr/23/flood-disaster-committee-membership, accessed 16th Dec 2010.

32 *Report of the Departmental Committee on Coastal Flooding [Waverley Committee]*, para 1.

7.2.2 The reason for research: The warning system

The Waverley Committee argued that the research it recommended would make forecasts done by the newly established warning system more accurate, i.e make them more like observed surges, and reduce the number of costly false alarms.[33] An emergency warning system had been set up soon after the event.[34] The Waverley Committee was asked by the government to produce an interim report regarding the continuation of the warning system, so that this could be organised for the following winter.[35] The warning system suggested by the Waverley Committee was fairly similar to the emergency one. Hydro staff would be based at the Met Office forecasting office in Dunstable receiving special hourly tidal predictions from TI, tidal observations from gauges and meteorological information from the Met Office. Hydro's staff would then calculate a forecast of the surge using a version of Robert Henry Corkan's forecasting formula further developed by Hydro's staff, and put out warnings if necessary. The interim report with these recommendations was published in July 1953.[36]

As can be seen in a memo to the River Boards, which would receive warnings under the system and needed to prepare, the government very quickly accepted the recommendations. In mid-August 1953, even before the government had officially accepted the report, MAF sent the River Boards a confidential memorandum telling them that the Government had accepted the interim report "in toto".[37]

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[33] The Hydrographer estimated that each cancelled warning or false alarm cost at least £1,000 for local authorities, the police etc. To him the aim of the research was to improve the 'accuracy' of forecasts which in turn would enable him "with confidence, to lower the margin of safety which he now applies," so that there would be fewer costly false alarms. "Waverley Committee on Coastal Flooding, Report on Oceanographic Sub-Committee", Note for [the departmental] meeting on 3rd Feb 1954, HW 287.54, UKHO

[34] Minutes of Cabinet Emergency Committee, H 01041.53, UKHO

[35] Cabinet, Official Committee on Emergencies, Sub Committee on the Setting Up of Flood Warning System, Minutes from third meeting, 24th March 1953, H 01041.53, UKHO

[36] *Report of the Departmental Committee on Coastal Flooding [Waverley Committee]*, Appendix A,, Interim Report - July 1953. Flood warning system.

[37] Memorandum to river boards, 17th Aug 1953, "Flood Warning System", MAF 222/306, NA. The lack of a warning system had been a common complaint voiced both by media actors and by others whose views were covered by the newspapers, for example, East Suffolk County Council and the jury at the coroners' inquests in King's Lynn and Canvey Island.[37] While this controversy was one of the reasons why the warning system was implemented fast and as suggested by the Waverley Committee, as Johnson, Penning-Rowsell and Tunstall has argued, I would add that the government did this to avoid further political controversy and demands for funding. Johnson, Tunstall, and Penning-Rowsell, "Crises as Catalysts for Adaptation," 79-82.

7.2.3 Formulation of a research plan: The Oceanographic Sub-Committee

The framework for TI's research came out of an Oceanographical Sub-Committee set up by the Waverley Committee on the suggestion of Proudman and the meteorologist on the committee, GMB Dobson. The Waverley Committee's original terms of reference did not allow it to make recommendations on future research, but the creation of a sub-committee made this possible.[38] This sub-committee provides a good example of the Waverley Committee favouring planned research, especially as the sub-committee's recommendations made it into the final recommendations of the Waverley Committee. By suggesting the sub-committee Proudman was able to 'piggy-back' a research agenda for storm surge science onto the Waverley Committee's report. This suited the Waverley Committee, Proudman and TI, which was recommended as a research contractor to the state as a result of the close connections between the Institute, Proudman and Hydro.[39]

The Sub-Committee's terms of reference included reporting on what research was needed to improve the recommended warning system and who should do this work.[40] Proudman chaired while the other members were the other scientists from the main committee, Dobson (Reader in Meteorology at the University of Oxford), Claude Inglis (Director, Hydraulics Research Station, Department of Scientific and Industrial Research) and JA Steers (Professor of Geography at the University of Cambridge). They also consulted George Deacon and Farquharson.[41] The scientists recommended further research and the creation of an Advisory Committee to co-ordinate the work. They identified seven specific problems and allocated these to TI and NIO, both represented directly or indirectly at the meeting.[42] The focus of this chapter is on two of the problems TI were allocated: "[t]he search for empirical formulae" to predict surges at

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[38] Minute by JE Maher (secretary of the Waverley Committee) to Chairman, 22nd May 1953, MAF 135/344, NA

[39] Correspondence between Proudman, Rossiter and Doodson, spring and summer 1953. Folder: Waverley Committee, Box 120, BA, especially Proudman to Rossiter, 24th Apr 1953, Box 120, BA.

[40] It was also "to make recommendations on the location of tidal gauges on the coast of Great Britain" and consider whether statistics should be prepared from tidal gauge records on the frequencies of certain high water levels. I will not discuss this side of the work, as it is less relevant to the focus of the thesis.

[41] Departmental Committee on Coastal Flooding, Minutes of third meeting, 8th June 1953, MAF 135/341, NA and *Report of the Departmental Committee on Coastal Flooding [Waverley Committee]*, Appendix B. The Sub-Committee's one meeting was held at the Royal Society in June. Maher to Steers, 17th Jun 1953, MAF 135/344, NA.

[42] Proudman represented TI's interests.

seven named ports along the East Coast as early as possible and "[t]he search for formulae which will enable external surges, as recorded at Aberdeen, to be forecast from meteorological data".[43] TI was then asked whether it was able and willing to do the suggested work, which it said it was, subject to finance of at least £2000 per year and time to fulfil its other responsibilities regarding tidal predictions.[44]

In its report the Sub-Committee formulated a specific research programme aimed at improving the warning system, to be undertaken by TI and NIO. This specification was heavily dependent on existing networks and contacts, as well as on existing ideas about what research would be useful. For example, Proudman communicated with TI informally regarding the proposed research, suggesting they stress the need for research on statistical formulae in their submission of evidence to the Waverley Committee.[45] In a supporting circle this was one the ideas then allocated to TI by the Sub-Committee. A noticeable exclusion from all this networking was the Met Office.

Having shown that the suggested research was framed as necessary to decrease the costs of the warning system and improve its performance, and that the suggested research plan was formulated by a group of scientists including Proudman in close collaboration with the research institute's they suggested should do the work, we now turn to the establishment of the framework for the suggested research.

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[43] Oceanographic Sub-Committee Report, undated, ca July 1953, document nr 18 in MAF 135/344, NA and *Report of the Departmental Committee on Coastal Flooding [Waverley Committee]*, Appendix B. The recommended research allocated to NIO was studies of free and forced oscillations in the North Sea, the reaction of the sea surface to winds of different strengths and how surges and long waves were modified in shallow water. In the final version TI was allocated the tide-surge interaction problem (which is somewhat similar to the last of NIO's tasks), the production of analysis and hourly tidal predictions for the ports involved in the warning system, investigations of certain kinds of surge oscillations and doing similar work as that done for the North Sea for the other seas surrounding Britain. The interaction issue appears to have been added to this list before the final Waverley report, as it is not in the version of the sub-committee's report prepared in 1953.

[44] Doodson to Ministry of Housing & Local Government, 23rd Jul 1953, folder Waverley Committee, Box 120, BA. See also correspondence in MAF 135/344, NA

[45] Correspondence between Proudman, Rossiter and Doodson, spring and summer 1953. Folder 'Waverley Committee', Box 120, BA, especially Proudman to Rossiter, 24th Apr 1953, Box 120, BA.

7.2.4 Establishing the Advisory Committee: Which department would lead?

The Sub-Committee's recommended research programme turned into the Advisory Committee through a process of departmental negotiations, during which the value of the proposed research was debated and different views on the value of this kind of science were aired. After the sub-committee's report had been accepted by the main Waverley Committee, the chairman asked the Committee's Secretary, Maher, to bring it to the notice of the government.[46]

The sub-committee's report then did the rounds of Whitehall, bouncing between different departments until MAF picked it up. The Home Office co-ordinated the report's Whitehall travels, but officials there said it did not have any other departmental interests in the proposed research.[47] Indeed an Assistant Secretary in the Civil Defence Department, RF Wood, quickly pushed the report away from the Home Office by suggesting that the matter was not something that the Home Office could play a "useful part" in. He implied that the report was overly academic and technical, and that the work would only possibly lead to indirect benefits to the warning system over the long term.[48] The Admiralty was then asked to lead on the report. They responded that while they were "flattered" to be asked and were "competent to advise on the technical aspects of the report" they would not take the lead as they were not the right Department to decide whether the potential increase in protection to life and property was worth the expense and effort of the suggested work.[49] They claimed to be *too* technical to lead – no doubt there were also financial or personnel considerations involved. Following this, the Home Office bounced the report to others, asking MAF, the Ministry of Housing and Local Government (MHLG) and the Scottish Office to take over the lead on it .[50]

At this point the Permanent Secretary Alan Hitchman at MAF said they would lead on the implementation of the report. MAF seems to have decided to take the lead on the report with only little discussion. Assistant Secretary CHA Duke's first minute in the file

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[46] Correspondence in MAF 135/344, NA, and Departmental Committee on Coastal Flooding, Minutes of fifth meeting, 6th Jul 1953, MAF 135/341, NA
[47] Minute by A Booth, 24th Sep 1953, HO 325/13, NA
[48] Minutes by RF W[ood], 14th Sep 1953 and 25th Sep 1953, HO 325/13, NA
[49] Jarrett to Allen, 28th Oct 1953, HO 325/13, NA
[50] Newsam to Milne, 18th Nov 1953, HO 325/13, NA

stated that "since this Department is responsible for the sea defences of low-lying land and therefore has a major interest in seeing that information about tidal surges is as complete as possible I think we should take the initiative in this matter".[51] In the interwar period the Treasury had argued that if the state became the patron of surge science this could lead to demands for funding of sea defences. Duke now argued that as the state, specifically his department, were responsible for sea defences, they should support the research – a symmetrical argument, but stated in the opposite direction to what the Treasury had feared.

There were briefly some discussions whether MAF or the MHLG should lead, with the latter arguing for joint leadership, but Duke pressed for MAF to lead as he thought MHLG's attitude to research left much to be desired, implying they were not keen on supporting research.[52] He also ascertained that the Chief Engineer thought an increase in the reliability of the warning system was worth "the relatively modest sum of £25,000", and argued for a meeting of concerned Departments.[53] By mid January Duke confirmed to the Home Office that MAF would lead on the report.[54]

It is clear that while the 1950s may have been a decade of strong belief in planning and scientific research as a problem solver, some departments and some officers believed this more strongly than others. In this case Duke at MAF was keener on scientific research than Wood at the Home Office, and Duke also thought MAF was 'better' at research than the Ministry of Housing and Local Government. The implementation of the sub-committee's research plan relied on arguments by officers such as Duke at MAF, who due to their established interests in sea defence were favourably inclined towards research in that area and also saw themselves as generally research friendly.

7.2.5 Establishing the Advisory Committee: Funding

Once MAF had adopted the report, they set about organising its implementation by calling a departmental meeting. Representatives of the Admiralty (including the

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51 Minute by Duke, 28th Nov 1953, MAF 135/324, NA

52 Minute by Duke, 28th Nov 1953, MAF 135/324, and Sheepshank to Newsam, 8th Dec 1953, HO 325/13, NA

53 Minute by Duke, 11th Jan 1954, MAF 135/324, NA

54 Duke to Wood, 13th Jan 1954, HO 325/13, NA

Hydrographer and Farquharson), MHLG, the Ministry of Transport and Civil Aviation, the Met Office, the Scottish Home Department, and MAF attended.[55] The representatives discussed the merits of the proposals "from a technical point of view" and quickly appear to have agreed to advise Ministers to accept the Sub-Committe's recommendations.[56] The report now had supporters who believed its arguments that research would improve forecasts and reduce costs, but funding was still an issue.

At the meeting none of the Departments present claimed to be able to fund the work in its entirety, though all, especially the Met Office and Hydro, agreed to contribute.[57] Following the departmental meeting one of MAF's finance officers contacted the Treasury to discuss the matter, finding that they were "more or less reconciled to the fact that any money for this purpose will have to be provided on this Ministry's [MAF's] vote". While MAF would need to seek Treasury authority for the grants, providing further details of the work proposed and the finances needed, they could assume that the principle of funding storm surge science had been accepted by the Treasury.[58] Formal Treasury approval was given after minor formalities had been sorted.[59] For the rest of the 1950s MAF and Treasury financial support for TI's research on surge forecasting was not questioned. For example, while there were critical debates between a number of Departments regarding the funding of further tidal gauges in 1958-59, this debate did not affect the funding of the rest of TI's work.[60] Central government was now a direct patron of storm surge science, especially TI's work on forecasting formulae for the warning system.

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55 The names, affiliations and titles (where known) of the representatives were: from the Admiralty SP Osmond (Principal) and Farquharson, the Ministry of Housing and Local Government HH Browne (Principal), the Ministry of Transport and Civil Aviation Mr FE Page, the Met Office Dr JM (James Martin) Stagg and Mr SB Peters (both Deputy Chief Scientific Officers), the Scottish Home Department Mr NJP Hutchison, and from MAF the chair CHA Duke (Assistant Secretary of Land Drainage and Water Supply Division), A Sparks (Assistant Secretary in the Finance and Accounts Division), EAG Johnson (Chief Drainage Engineer) and JE Maher (Principal in the Land Drainage Division).
56 Documents in HW 287.54, UKHO, quote are from Departmental Committee on Coastal Flooding, Report of the Oceanographic Sub-Committee, Minutes of meeting held at 3rd Feb 1954.
57 Departmental Committee on Coastal Flooding, Report of the Oceanographic Sub-Committee, Minutes of meeting held at 3rd Feb 1954, HW 287.54, UKHO
58 Minute by Assistant Secretary, Finance and Accounts Division Sparks, 3rd Feb 1954, MAF 135/324, NA. See also documents in HW 287.54, UKHO. There were some discussions regarding how to pay for the installation of additional tidal gauges, primarily between MAF and the Ministry of Transport. The latter in the end agreed to pay with Treasury approval, see documents in MAF 135/324, NA
59 Such as which sub-head the grants should be charged to and the provision of a minimal budget from TI outlining how much of the grant would be spent on different costs. Minutes and correspondence, October to December 1954, esp. Minute by Treadway, 8th Dec 1954, MAF 135/324, NA
60 Documents from 1958 and 1959 in MAF 135/324, NA

The Advisory Committee on Oceanographic and Meteorological Research met for the first time on 8 October 1954, in MAF's offices. Initially it had been suggested that the Advisory Committee should be chaired by MAF's Chief Engineer,[61] but "in deference to the recommendation in the Waverley Report" Duke suggested Proudman should be asked to chair it.[62] This was agreed by the Departmental representatives and Proudman thus became the chair of the Advisory Committee. George Deacon represented NIO, Doodson TI, Commander CT Suthons Hydro and Dr RC Sutcliffe the Met Office where he was Deputy Director (Research). Suthons, who had been involved with the swell forecasting system during the war, was Principal Scientific Officer at Hydro and now in charge of the warning system.[63] The interests of government departments were also represented by officers from the engineering departments at MAF, MHLG and the Ministry of Transport and Civil Aviation. Inglis represented DSIR.[64] The terms of reference for the Advisory Committee were to co-ordinate oceanographic and meteorological research work in relation to sea defence, as recommended by the Waverley Committee's technical sub-committee.[65]

With this the Advisory Committee was established and the Treasury had agreed to pay for TI's storm surge work; the state becoming a major patron of storm surge science. Once the overall responsibility for the report had been settled, the process of establishing the Advisory Committee appears not to have been contested, with little or no questioning of whether it was worthwhile or not. After MAF had taken it on, the arguments that the scientific research would reduce costs by reducing false alarms[66] and increase the 'reliability' of the warning system[67] were given space and were effective in

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[61] Minute by Hugh Gardner, 25th Mar 1954, MAF 135/324, NA
[62] Duke to Browne, 22nd Apr 1954, MAF 135/324, NA
[63] Documents from 1954 in H 01041.53, UKHO
[64] The departmental representatives were from MAF MR EAG Johnson, Chief Drainage Engineer, from Ministry of Housing and Local Government Mr AAL Lane, Senior Engineering Inspector, and from the Ministry of Transport and Civil Aviation Mr RE O'Malley, Harbour Engineer. The Committee's initial Secretary was Miss JD Duncombe from the Land Drainage and Water Supply Division of MAF.
[65] Advisory Committee on Oceanographic and Meteorological Research, Minutes of 1st meeting held on Friday, 8th October, 1954, and agenda paper no 1 "Advisory Committee on Oceanographic and Meteorological Research", Box 160, BA.
[66] "Waverley Committee on Coastal Flooding, Report on Oceanographic Sub-Committee", Note for meeting on 3rd Feb 1954, HW 287.54, UKHO
[67] In addition to the Hydrographer's comments this had been how Proudman argued for the original research, see e.g. Minute by Maher, 22nd May 1953, MAF 135/344. For the Hydrographer's comments, see note 33, this chapter.
[67] Minutes of Cabinet Emergency Committee, H 01041.53, UKHO

securing support and funding.[68] This was due more to Duke's eagerness for MAF to support such work than to the Waverley Committee's pressure.[69]

7.3.1 Questioning of TI's practices

What happened to TI's use of their particular practices when these were developed and judged within the new framework of the Advisory Committee? This section discusses the research TI did for the Advisory Committee on statistical forecasting formulae for the East Coast. It concentrates on how TI's practices were questioned, focusing on a debate between TI, Hydro and the Met Office which broke out in the mid 1950s regarding the calculation of winds. This follows on the discussion in the previous chapter of TI's choice to use geostrophic winds to do this calculation and analyses what happened when their particular practices encountered other, different, practices within the new framework of the Advisory Committee. TI's emphasis on correlation was criticised by the Met Office which favoured causal analysis. However, despite TI then suggesting that theoretically based research would lead to more 'improvements' in forecasts than continued statistical work, its emphasis on statistical formulae remained at the end of this dispute, as the Advisory Committee asked them to prioritise such work. In 1957 TI presented their statistical formulae as finished, and as being as good as possible using this particular practice. After a couple of years usage the prime user, the flood warning system represented by Hydro's Suthons, agreed with TI that they had taken statistical work on forecasting as far as it could go.

I will first look at the early continuation of TI's statistical practices until 1955, before turning to the contestations of it in 1955-6, and how TI then produced a set of forecasting formula which they and, after a couple of years, Suthons judged as the best

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68 This of course did not mean all agreed. Once Wood had been told of the outcome of the Departmental Committee he wrote "This seems like a happy [illegible - route? minute?] out of one of our afflictions at least." Minute by RF W[ood], 22nd Feb 1954, HO 325/13, NA

69 Minute by Duke, 28th Nov 1953, MAF 135/324, NA. When the Waverley Committee at its last meeting in March 1954 were mistakenly told Treasury authority had not already been given for the suggested research work it threatened to ask the Home Secretary "for an assurance that very early steps would be taken to authorise the research." This provides an example of the committee's support of research. However, as this was a case of miscommunication, with the authority already given, it also clarifies that issues other than the Waverley Committee's pressure on the government were decisive for the Treasury's provision of funds to TI's research (such as avoiding a political backlash if they refused funding). Departmental Committee on Coastal Flooding, Minutes of twenty-sixth meeting, 22nd Mar 1954, MAF 135/341, NA

possible *statistical* formulae. However, I will first introduce TI's key worker on the forecasting formulae during the 1950s, Jack Rossiter (1919-1972, full first name John Reginald). When the event took place Doodson, who had become Director of TI in 1946,[70] was severely ill with heart disease and not to be disturbed.[71] Corkan, who had been Doodson's second in command, had died unexpectedly the previous year, leaving Rossiter to take control of TI's response to the flooding event.[72] Rossiter had joined as junior assistant in 1937, after studying for his Higher School Certificate as a scholarship student at Liverpool Collegiate College.[73] His father was a barber and illegal bookmaker and his brother the actor Leonard Rossiter.[74] As money was short he was unable to attend university but while working for TI he studied part-time at the University of London, gaining an Intermediate BSc in 1939. During the war, between 1939 and 46, he served in the Royal Artillery, as an Instructor in Anti-Aircraft Gunnery and as a REME (Royal Electrical and Mechanical Engineering) Education Officer.[75] Before 1953 he was closely involved with mean sea level work at TI and his research work involved determining tidal charts using relaxation methods, i.e. work not directly related to storm surges. In 1947 he obtained a BSc Degree (first class honours), followed by an MSc in 1956, with his thesis being on surges, and a DSc in 1961, all as an external London University part time student.[76]

Rossiter, Proudman and Farquharson, at the Tidal Branch, initially assumed the empirical formulae Oceanographic Sub-Committee had asked for would be statistical ones, along the lines of Corkan's earlier work. In their discussions both Hydro and TI discussed statistical work, as did Proudman, who emphasised statistical formulae in correspondence with Rossiter: "it would be valuable to have formulae like Corkan's for any pair of stations out of half a dozen on the east coast of Scotland and England".[77] In their memos to the Waverley Committee Rossiter wrote about "extensions of previous

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[70] Proudman to Hobhouse, 19th Nov 1945, P744/4, LUA; and Hobhouse to Proudman, 21st Nov 1945, P744/4, LUA

[71] Scoffield, *Bidston Observatory*, 208 & 213.

[72] LOTI, *Annual Report 1952*, 5-6.

[73] Scoffield, *Bidston Observatory*, 182.

[74] "Rossiter, Leonard," in *Oxford Dictionary of National Biography* (Oxford University Press, 2009); Jones, "From Astronomy to Oceanography - a Brief History of Bidston Observatory."

[75] Scoffield, *Bidston Observatory*, 188.

[76] Ibid., 216; NERC, *Institute of Coastal Oceanography and Tides, 1971-72*, Annual Report (Liverpool: NERC, 1972), 6.

[77] Proudman to Rossiter, 24th Apr 1953, Box 120, BA

work"[78] while Farquharson wrote that Corkan's "method should *obviously* be developed".[79] This group of researchers, who knew each other well, assumed that TI's earlier practices should be continued and indeed wrote the continuation of these practices into the programme of work TI was then contracted to do.

By these researchers the continuation of TI's practice was taken for granted as it was said to have previously 'worked'. In TI's submission to the Waverley Committee, Rossiter claimed that Corkan's formula produced "satisfactory predictions" for most surges. A satisfactory prediction was defined as one in which "the general trend of the residuals was calculated in a way which could have been done six hours in advance, and the general magnitude of the effect reproduced". Having defined Corkan's statistical forecasting formulae as satisfactory, Rossiter went on to assume that further such work could produce equally satisfactory results: "There is no reason to doubt that investigations such as that for Southend ... will give correspondingly satisfactory results".[80]

There is no indication that this assumption that forecasting formulae should be statistical, like Corkan's, was questioned by other members of the Oceanographical Sub-Committee or the Advisory Committee at this point.[81] However, other aspects of TI's practices changed, in particular how they calculated winds. As discussed in the previous chapter, before 1953 Doodson and Corkan had used geostrophic winds, which assumed the isobars were straight, to easily calculate the winds they correlated the surge with. In response to comments from Proudman and others, TI changed to using gradient winds which were somewhat more difficult to calculate but take the curvature of isobars into account. There is little material on this decision, but when recalling this initial questioning of TI's practices, Doodson claimed he had not felt that the refinement was necessary for work on statistical forecasting formulae based on correlation, but had accepted it to meet the criticisms from those like Proudman who Doodson argued were

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[78] "North Sea Storm Surges", Memorandum [for Waverley Committee] by the Liverpool Observatory and Tidal Institute, April 1953, Rossiter, Box 120, BA

[79] My emphasis. "Waverly Committee: Terms of Reference Item No. iii, A Permanent Flood-Warning System", April 1953, H 01041.53, UKHO

[80] "North Sea Storm Surges", Memorandum [for Waverley Committee] by the Liverpool Observatory and Tidal Institute, April 1953, Rossiter, p 5-7, Box 120, BA

[81] Advisory Committee on Oceanographic and Meteorological Research, Minutes of 1st meeting, 8th Oct 1954; Minutes of 2nd meeting, 14th Dec 1954, Box 159, BA. There are also no indications of any discussions of such matters in MAF's files of correspondence regarding the meetings, MAF 135/356 (1st meeting) and MAF 135/357 (2nd meeting), NA

concerned with "mechanisms of action", i.e. causation instead of correlation.[82] Rossiter had incorporated the use of gradient winds in TI's practices for constructing surge forecasting formulae. Judgements on TI's work by the scientists involved in the establishment of the Advisory Committee had thus led to TI changing their practices of calculation.

Overall TI's researchers continued their statistical work along previous lines with some changes.[83] For example, they worked on a forecasting formula for Aberdeen,[84] something which Suthons repeatedly argued for.[85] The members of the Advisory Committee were happy with the progress, "not[ing] with satisfaction the progress made" with the research on the forecasting formulae at the spring 1955 meeting.[86]

7.3.2 Contested practices: Causation vs correlation

However, by the autumn of 1955 problems were starting to appear. At a meeting of the Advisory Committee the apparent calm was shattered, when a debate regarding TI's practices of calculating winds broke out. Different approaches and practices collided, with the Met Office criticising TI's work for not taking into account causal relationships when estimating winds. Doodson instead claimed that had never been their intention, and that they had 'only' ever attempted to produce forecasting formulae as fast as possible by correlating pressure gradients with the surge. This section will look in more detail at this debate between the different approaches to the problem.

This debate was preceded by a refusal by the Met Office to do work on the stress of wind on water, i.e. the theoretical relationship which TI was said to be neglecting by the

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[82] Doodson to Farquharson, undated, ca Nov 1955, HW 287.54, UKHO

[83] They also changed how they reduced data to construct curves of residuals, replacing Doodson's graphical method described in chapter five with a new "simple computative method," which was said to be in a form already familiar to TI's computers, so making it easier for them to work with it, and did not require the use of a tide-predicting machine. This continued their emphasis on reducing the time and work involved in the calculations. "Liverpool Observatory and Tidal Institute, Progress Report on Storm-Surge Research (to March 31, 1955)," Paper 3(v) for 3rd meeting of Advisory Committee on Oceanographic and Meteorological Research on 28th Apr 1955, p1, Box 159, BA.

[84] Advisory Committee on Oceanographic and Meteorological Research, Minutes of 3rd meeting, 28th Apr 1955, para 4, Box 159, BA

[85] CT Suthons, "Report on the Warning System during 1953/4", 28th Sep 1954, Agenda paper for 1st meeting of Advisory Committee on Oceanographic and Meteorological Research, 8th Oct 1954, and Agenda papers for 3rd meeting, 28th Apr 1955, Box 159, BA.

[86] Advisory Committee on Oceanographic and Meteorological Research, Minutes of 3rd meeting, 28th Apr 1955, para 3, Box 159, BA

Met Office. In late 1954 the Advisory Committee asked the Met Office to help the Advisory Committee by researching the stress of wind on water. The Deputy Director (Research) there, OG Sutton, claimed that while the issue had been put on the Meteorological Research Committee's Programme, they were unable to conduct research into the problem currently due to the pressure of other work.[87] In other words, while it had been put on the Met Office's agenda and would be treated in due course it was not an issue given high priority by the Met Office. While this was in part due to different priorities and emphasis on different practices, with the meteorological community at this time moving away from statistics towards computer-based numerical weather prediction, it may also have been linked to departmental politics.[88] During the Departmental Meeting organised by MAF to establish the Advisory Committee Dr James Martin Stagg, one of the representatives of the Met Office and one of the key meteorologists involved in D-Day[89], had "expressed surprise" that the Office had not been consulted by the Oceanographic Sub-Committee while writing its report. At the time this was smoothed over by Johnson from MAF saying he thought the Met Office's involvement in the research had been assumed.[90] While seemingly petty, it may be that the Met Office had negative feelings towards the Advisory Committee, based on this early lack of consultation, which influenced how it related to the Committee.[91]

The debate regarding how TI should calculate winds began at the autumn 1955 meeting, when Rossiter asked for Met Office help with this as he felt he was not making the best use of the meteorological data.[92] At the meeting Doodson also raised technical concerns regarding how TI's formulae were constructed, which brought the issues to light. He criticised the provisional formula for Aberdeen Rossiter had presented at this meeting, arguing that despite achieving a relatively high correlation coefficient of 0.84 it only explained about half of the storm surge. He attributed this partly to the inclusion of non-meteorological effects in the calculations of standard deviations and partly to

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[87] Sutton to unnamed at Ministry of Agriculture and Fisheries [probably the secretary for the Advisory Committee], 4th Jan 1955, HW 287.54, UKHO. See also documents in MAF 135/357 and MAF 135/358, NA

[88] Edwards, *A Vast Machine*; Harper, *Weather by the Numbers*.

[89] James Rodger Fleming, "Sverre Petterssen, the Bergen School, and the Forecasts for D-Day, " *Proceedings of the International Commission on History of Meteorology* 1, no.1 (2004).

[90] Documents in HW 287.54, UKHO, quotes from "Departmental Committee on Coastal Flooding, Report of the Oceanographic Sub-Committee", Minutes of meeting held at 3rd Feb 1954.

[91] I have not been able to find any Met Office files to confirm this.

[92] Farquharson, minute to ADMO (FR), undated, ca Nov 1955, HW 287.54, UKHO. Farquharson claimed the Minutes of the meeting provided a "garbled account" of Rossiter's question.

difficulties in producing a good statistical formulae. Standard multiple regression techniques assume that the different explanatory variables, e.g. winds in different places, that are used to explain the dependent variable, e.g. the storm surge, are independent from each other so that one variable can be changed while the others remain the same.[93] According to Doodson, the correlation coefficients between winds at the different calculation points in the North Sea and the Atlantic and the surge in Aberdeen were low (below 0.6) but there was high inter-correlation between the different points, causing "uncertainties in the solutions".[94] Doodson argued that as wind in one area tended to mean wind in another – the variables were not independent – the statistical approach did not produce good results. Doodson was arguing that more, different, research was needed to produce 'better' formulae, which TI needed more funding and time to do. Doodson brought up the issues to argue for further support.

However, raising these issues appears to have led to critiques of TI's practice. At this meeting even what TI's practices *were* seems to have been debated, especially whether they and Hydro were using similar enough practices in regards to the meteorological data. As one outcome of the autumn 1955 meeting, FM Berncastle, one of Hydro's officer involved in the warning system, visited TI and was shown how TI calculated the tractive force of the wind from isobars, in order that the warning system would be able to use a similar method.[95] After this visit Berncastle produced a memo, giving details of TI's practices, and suggested that TI's methods be checked for their validity by the Met Office:

> The very great importance of this work to the nation, in saving life and securing property, which justifies the research now being carried out by other bodies would seem to justify further research on the part of the Meteorological Office

[93] Croxton and Cowden, *Applied General Statistics*, 741-742. As put by Johnson and Tetley: "When the ranges of variable are mutually dependent … the correlation which arises is not of the simple type … and it is doubtful whether the correlation coefficient by itself is a valid measure of association in such cases."N L Johnson and H Tetley, *Statistics: An Intermediate Text Book* (Cambridge: Cambridge University Press, 1950), 165.

[94] "Report on the work done at the Liverpool Observatory and Tidal Institute", Doodson, Paper 4(ii), Advisory Committee on Oceanographic and Meteorological Research, Minutes of 4th meeting, 4th Oct 1955, Box 159, BA

[95] Rossiter to Farquharson, 15th Oct 1955, HW 287.54, UKHO

> if the mechanics of storm surges are to be fully understood and the best method of forecasting then derived.[96]

Berncastle was implying that the Met Office ought to do more work for the Advisory Committee, for example by giving the issue higher priority in its research programme. He justified his pressure to divert Met Office resources towards Advisory Committee work by arguing such work was in the national interest.

Put under pressure from both Rossiter asking for help and Berncastle, the Met Office appears to have taken umbrage at these attempts to get them to do the research they had politely but firmly refused to do. In response to Berncastle's suggestion that the Met Office evaluate TI's methods, JS Sawyer, the Senior Principal Scientific Officer in charge of forecasting research at the Met Office,[97] explained he did not want to do any of TI's work for them. He did admit that if, but only if, "a fundamental approach to the tidal problem based on theoretical relations between surface wind and stress on the sea is successful", then the Met Office would consider doing some research.[98] Despite this refusal, a colleague of Sawyer's, JM Craddock, did in the end produce a two page memo giving the Met Office's thoughts on how to estimate the effect of wind stress on the sea, while Saywer was on sick leave. This memo listed a number of factors impacting on the wind effect that he thought TI ought to take into consideration in their work but provided little or no quantitative directions for how to take them into account.[99] Sawyers and others at the Met Office seem to have wanted TI to produce a theoretical rather than statistical model before the Met Office did research. They thought the research should be about causes, not statistical correlations.

In response to Craddock's memo Doodson explained to Farquharson that they were not doing what the Met Office seemed to think they were doing. They were not estimating the wind on the basis of all possible causes, like the ones Craddock had listed

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[96] "Use of Meteorological Data for Storm Surge Research by the Liverpool Tidal Institute", Berncastle, undated, late Oct or early Nov 1955, HW 287.54, UKHO

[97] John Mason, "John Stanley Sawyer 19 June 1916--19 September 2000," *Biographical Memoirs of Fellows of the Royal Society* 48(2002).

[98] JS Sawyer to Farquharson, 14th Nov 1955, HW 287.54, UKHO

[99] These factors included issues such as the speed of change in pressure gradient and that the relationship between the pressure gradient and surface wind was different for easterly compared to a westerly wind. "Comments on correspondence between Dr Doodson, [LOTI] and Cmdr. Farquharson, Hydrographic Officer, Dunstable", J M Craddock, undated, ca Nov 1955, HW 287.54, UKHO

in his memo, but instead correlating pressure gradients and surges, without considering the intermediate causes:

> I have very carefully avoided estimating the wind. I passed direct from the barometric pressure system to the surge. This letter is written by me and received by you but you do not speculate as to the intricate procedure by which it reaches you. So in many physical problems we have to pass by the stages, though we would always like to know these in detail, and in due course we shall do so.[100]

Doodson thus emphasised that TI's work on storm surge forecasting was part of a set of statistical practices of calculation, producing correlations and not causal descriptions. While Doodson would like a more fundamental understanding, as far as TI's current work went, he saw it as just about statistical correlations. This was in opposition to the Met Office staff who clearly were much more keen on causal analysis.

The correspondence stopped at this point, with TI firmly placing their formulae in the realm of statistical correlations, away from hydrodynamical causal physics. The new Advisory Committee framework brought together two different approaches to surges: one focused on correlation, statistics and calculations, the other on causal and synoptic analysis. These different approaches also represented institutional investments in different practices and research programmes, both financially and epistemologically. The Met Office's researchers refused to have its own resources diverted, instead arguing TI should do work of a different kind than it was currently doing. TI's researchers on their hand defended their existing practices, arguing that changing these would take much resources which TI did not have and that they could not causally analyse every surge: "we have a hundred surges at each place and we cannot investigate all in detail".[101]

At the following meeting in spring 1956 Doodson raised whether the Advisory Committee wished them to continue their statistical work or whether they should go down a more theoretical route. He questioned whether TI's approach, based on

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[100] Doodson to Farquharson, 29th Nov 1955, HW 287.54, UKHO

[101] Doodson to Farquharson, 29th Nov 1955, HW 287.54, UKHO. The debate took place while Rossiter was working on his new formulae and at one point he wrote to Farquharson, asking him if TI's practices would have to change, as he did not want to do too much work on the Immingham formulae if changes were imminent. Rossiter to Berncastle, 10th Nov 1955, HW 287.54, UKHO

historical demands for quick forecasting formulae, was the best long-term approach, and suggested that research on surges had been "hampered" by the emphasis on empirical formulae for immediate use. Instead he suggested that Rossiter should continue some work he had done on the six-point method (which Corkan experimented with in the previous chapter), arguing this showed promising results and had a "better scientific basis". Doodson said this work had been put aside, both by Corkan and Rossiter, in order to provide simpler formulae for forecasters, and he called for the Committee to consider whether further such experimental work should be done by TI, suggesting it would lead to better forecasts more in the causal style favoured by the Met Office.

To support his argument for investment in new practices, Doodson developed his technical critique of the statistical approach, discussing how the formulae depended on inter-correlated winds, thus being "ill-conditioned". According to Doodson, formulae that were simple enough to use in forecasting were "somewhat unstable", fitting some samples well but other samples not so well while at the same time it was possible to vary the constants without much changing the fit. He argued this was not a problem if the meteorological data used by forecasters was "sufficiently accurate" but even "under the best conditions the uncertainties regarding wind velocity and direction are adverse to results which satisfy the research workers".[102] He thus admitted there were issues with the calculation of winds but put the onus not on TI's choice of gradient winds but on the quality of meteorological data, which could not easily be improved.[103]

At the meeting Rossiter outlined his work on using the six-point-method and its promising results, and reiterated that it had been "reluctantly put aside" to concentrate on statistical formulae. He argued that these statistical formulae were reaching the limits in terms of accuracy and ability to predict: the "least squares solutions ... would appear to mark the boundary line beyond which the 'stationary conditions' type of formula cannot be improved". In other words, the statistical formulae were as good as one could get, without taking into account non-stationary, dynamical aspects of surges, especially

[102] "Report on work done at the Liverpool Observatory and Tidal Institute," Part A, "General report", Doodson, Paper 5(iii) for 5th meeting of Advisory Committee on Oceanographic and Meteorological Research on 30th Apr 1956, Box 159, BA

[103] This does not appear to have reignited any debates with the Met Office, whose data Doodson was criticizing, though Proudman was vary of the language in minutes leading to offense. Proudman to Enticott, 10th May 1956, MAF 135/363, NA and see note 106.

"the response of the water according to the time rate of change of the meteorological variables".[104] Rossiter argued that new practices of calculation were needed to take this into account.

Doodson's concerns regarding inter-correlated variables was real, but it was also set within the context of the Met Office's criticisms. Doodson's questioning of TI's practices were thus both a response to the technical difficulties with inter-correlated variables and to the Met Office's criticisms of their methods as insufficiently theoretical. With his question to the Advisory Committee, Doodson was asking the Committee to confirm whether they wanted TI to continue their statistical work or whether they wanted them to attempt more theoretical work, which would take more time and money. He was offering to do work on causes – more to the liking of the Met Office – instead of correlation, but also implying there were costs in terms of time and funding to do such theoretical work. In the face of this need for investment the Advisory Committee gave statistical forecasting formulae priority, but tried to have it both ways by suggesting that TI should also continue working on the six-point method if possible.[105] There is no indication that this decision was contentious and TI continued their statistical forecasting work to produce forecasting formulae.[106]

One thing the new framework of the Advisory Committee did was to bring together different actors who had different practices, leading to debates between the practitioners. How these debates went was partly shaped by the political and personal history of interactions, such as between the Met Office and the Advisory Committee, by historical choices, such as that of TI to use geostrophic and later gradient wind, and also by the different methodological approaches at TI and the Met Office, such as TI's emphasis on statistics versus the Met Office's emphasis on causal relationships. This last was the most fundamental issue at stake, but it was set within the context of many other

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104 "Report on work done at the Liverpool Observatory and Tidal Institute," Part B, "Report on North Sea surge research", Rossiter, Paper 5(iii) for 5th meeting of Advisory Committee on Oceanographic and Meteorological Research on 30th Apr 1956, Box 159, BA

105 Advisory Committee on Oceanographic and Meteorological Research, Minutes of 5th meeting, 30th Apr 1956, para 6, Box 159, BA

106 Proudman suggested to the Secretary of the Advisory Committee that use of the term 'inaccuracy' could be offensive to the providers of the data, i.e. the Met Office, and suggested she use other terms (such as deficiency and gap) in the Minutes instead, but no other corrections were made to the minutes regarding the aspects discussed here by any of the reviewers (which also included the MAF staff who were on the committee, Spalding, Maher and Johnson). Proudman to Enticott, 10th May 1956, MAF 135/363, NA

issues, particularly the investment of time and money necessary to change the practices used in the production of forecasting formulae.

7.3.3 TESTING THE STATISTICAL FORMULAE

During and after the debate with the Met Office Rossiter constructed statistical formulae for Aberdeen and the four East Coast ports that were part of the flood warning system. He presented these in 1957 and for the next couple of years they were used and tested by the warning system organisation. In the Interim Report of the Advisory Committee,[107] prepared in 1957, TI claimed to have fulfilled the request for empirical formulae that the Oceanographic Sub-Committee had made, to the extent it was possible and useful. Following up on their earlier comments, they stated that "it is improbable that further work on these lines would give better results", instead suggesting research of a more concentrated, regional nature, using the theory of decaying oscillations, i.e. the six-point method.[108] TI thus declared their statistical formulae as good as possible and that it was necessary to change their practices to get further improvements, i.e. to further reduce the gap between predictions and observations. For the next couple of years TI were working on other research for the Advisory Committee.[109] No further work was done on the statistical forecasting formulae for the East Coast, instead permission was granted to publish the results in 1957.[110]

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[107] As Proudman pointed out one reason for the report was to convince MAF that the Committee had "justified [its] existence." (Proudman to Enticott, 18th Feb 1957, MAF 135/381, NA). It seemed to serve this purpose, with Under Secretary Basil Engholm deeming the progress of the work "satisfactory." (Minute by Engholm, 11th Jul 1957, MAF 135/381, NA)

[108] Interim report, attached to Advisory Committee on Oceanographic and Meteorological Research, Minutes of 7th meeting, 15th Apr 1957, Box 159, BA. The report was drafted by the Secretary from MAFF, with help from Proudman, Doodson, Deacon and Suthons. The drafting and finalising of the interim report appears to have been uncontroversial. The only interesting change to the sections discussed here was when Proudman changed the title of the section which discussed the performance of TI's new formulae from "Accuracy of Surge Prediction Formulae" to "Testing of New Surge Prediction Formulae" – an indication that issues of accuracy were sensitive to members of the committee, compare note 1066, this chapter. Draft interim report, amendments in Proudman's handwriting, MAF 135/381, NA

[109] For example investigating surges in the English Channel and on the West Coast, periodic oscillations following the main surge and tide-surge interaction in the Thames.

[110] J. R. Rossiter, "Research on Methods of Forecasting Storm Surges on the East and South Coasts of Great Britain," *Quarterly Journal of the Royal Meteorological Society* 85, no. 365 (1959). Advisory Committee on Oceanographic and Meteorological Research, Minutes of 8th and 9th meeting, 11th Dec 1957 and 19th May 1958, minute 9 (at 8th meeting) and 2(ii) (at 9th meeting), Box 159 and 160, BA

Between 1956/7 and 1958/9 Suthons tested TI's formula and their claims for it. Having compared the results of the old and new formulae during the season 1956/7 Suthons was cautiously optimistic, saying the new formulae had shown "some improvement on the old for a few hours ahead for the ports on the open sea", but remarked the season had been quiet with no really large surges. Having calculated standard errors for the new prediction formulae, he concluded that for the southern ports the predicted and observed residuals agreed "reasonably well", with standard errors of 0.5ft at Tyne, 0.7ft at Immingham and 0.8ft at Lowestoft, using 90 comparisons. However, the result for the Aberdeen formula he argued "was not so good" with a standard error of nearly 1 foot.[111] Of course these judgements were not only about the numbers. Take for example the small difference in standard error at Lowestoft and Aberdeen (less than 0.2ft). There was a very fine line between 'reasonably well' and 'not so good', that was not just in the numbers, which were not that different. Instead this was also about supporting his view that more research was needed on a new forecasting formulae for Aberdeen, which he repeatedly argued for.[112] He was continuing to argue for particular types of research to be done by TI.

Over the next couple of years Suthons incorporated TI's formulae into the warning system and tested them further, using statistical techniques to produce formal numerical measures of the fit of TI's formulae.[113] While he adopted TI's emphasis on comparing observed and predicted residuals, he added numerical, statistical estimates of this. This was a change of practice away from TI's visual judgements of the performance of their formula towards more formal numerical judgements. This was also a change from how Suthons had judged the performance of the early warning system by counting the number of false alarms in 1953-4.[114] Both TI's and Hydro's earlier practices of judging

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111 CT Suthons, "Report on the working of the Flood Warning Organization during the season 1956/7", 2nd Apr 1957, p 2-3, Agenda paper for 7th meeting of Advisory Committee on Oceanographic and Meteorological Research, 15th Apr 1957, Box 159, BA.

112 For example in his report in spring 1956, CT Suthons, "Report on the working of the Flood Warning Organization during the season 1955/6", 14th Apr 1956, p2, Agenda paper for 5th meeting of Advisory Committee on Oceanographic and Meteorological Research, 30th Apr 1956, Box 159, BA.

113 CT Suthons, "Report on the working of the Flood Warning Organisation for the Season 1958/9", 27th Apr 1959, p 2, Agenda paper for 11th meeting of Advisory Committee on Oceanographic and Meteorological Research, 4th May 1959, Box 160, BA.

114 CT Suthons, "Report on the Warning System during 1953/4", 28th Sep 1954, Agenda paper for 1st meeting of Advisory Committee on Oceanographic and Meteorological Research, 8th Oct 1954, Box 159, BA.

the success of statistical formulae were thus amended when they were brought together through the Advisory Committee.

In spring 1959 Suthons declared he had tested TI's formulae and agreed with TI on their performance. At the spring 1959 meeting of the Advisory Committee Suthons "said that the degree of accuracy achieved was probably about the highest that could be hoped for".[115] At this point the person in charge of the warning system was by all accounts satisfied with the accuracy of TI's forecasting formulae, though he also supported work on other types of formulae. This is why 1959 is the endpoint for the thesis: the construction of statistical storm surge formulae at TI were said to have reached a high point.[116]

7.4 Practices of calculation at TI: Calculating the wind effect

The debate about how to calculate the wind effect provides material for a final discussion about the practices of calculation TI used in their work on forecasting formulae. Berncastle's memo to his colleagues at Hydro (mentioned above) explained in detail how TI went about constructing the crucial estimates of wind drag. It was meant to enable those running the warning system to follow or amend TI's practice as suited them and consisted of a range of material: tables, scales, theory sheet, instructions both specifically for Hydro and for TI's computers and a summary bringing it all together.[117] For us this memo provides a window into some of TI's practices of calculating the contested winds.

What did TI do to calculate their gradient winds? Firstly, they acquired the Met Office's Daily Weather Report charts and prepared empty data tables, with a number of labelled columns. A computer then began filling in this data table for a particular port, say Immingham, by reading off the barometric pressure at the specified port from the weather chart, by checking the isobars and if necessary interpolating between different isobars. The next step for the computer was to read off two pressure gradients at each

[115] Advisory Committee on Oceanographic and Meteorological Research, Minutes of 11th meeting, 4th May 1959, para 4, Box 160, BA

[116] After this point they were being deconstructed instead of constructed, which brought in issues of a different kind to those this thesis has focused on. I hope to look further at these issues of controversy and debates after the conclusion of the thesis, see section 8.4.

[117] Berncastle memorandum, undated, late Oct or early Nov 1955, HW 287.54, UKHO

of nine points over the North Sea. This was done by placing a glass scale with the points and gradients marked on it on top of the Daily Weather Report, which as they were of a standardised format allowed standardisation of the scale. A paper version of the scale can be seen in figure 7.1. Having made sure the scale and the chart were oriented according to the written instructions (e.g. point E on the scale should coincide with 60 degrees north on the chart), the computer then calculated the difference between the pressure at the ends of the two lines that met at the point in question. These two differences, one east-west and one north-south gradient, were put into columns in the data tables. At this point the computer was told be "especially careful to put in the correct signs". The last task for the assistant computer was to use another glass scale marked with arcs of circles with varying radii, selecting the scale curve that most closely matched the curvature of the isobar nearest each of the lettered points in figure 7.1, and write these radii down in another column.[118]

The computer's work involved learnt skills of interpreting and following the instructions given to them, interpolating and calculating. A key skill was that of 'reading off' values using different techniques and tools, such as the scale in figure 7.1 or the radii-scale. This involved carefully identifying the numbers by taking care to position the scales as instructed and then seeing, locating, and reading them. As part of this process there were decisions regarding which value to use, potentially out of several possible. Just like in measurements in laboratory science, there were many different skills and much work involved in measuring and deciding values in TI's work.[119]

[118] "Instructions for reading-off barometric pressurs and pressure gradients, and isobar curvatures", sheet D of the memo "Use of Meteorological Data for Storm Surge Research by the Liverpool Tidal Institute", Berncastle, undated, late Oct or early Nov 1955, HW 287.54, UKHO
[119] For more on such work in laboratory science, see e.g. Gooday, *The Morals of Measurement*.

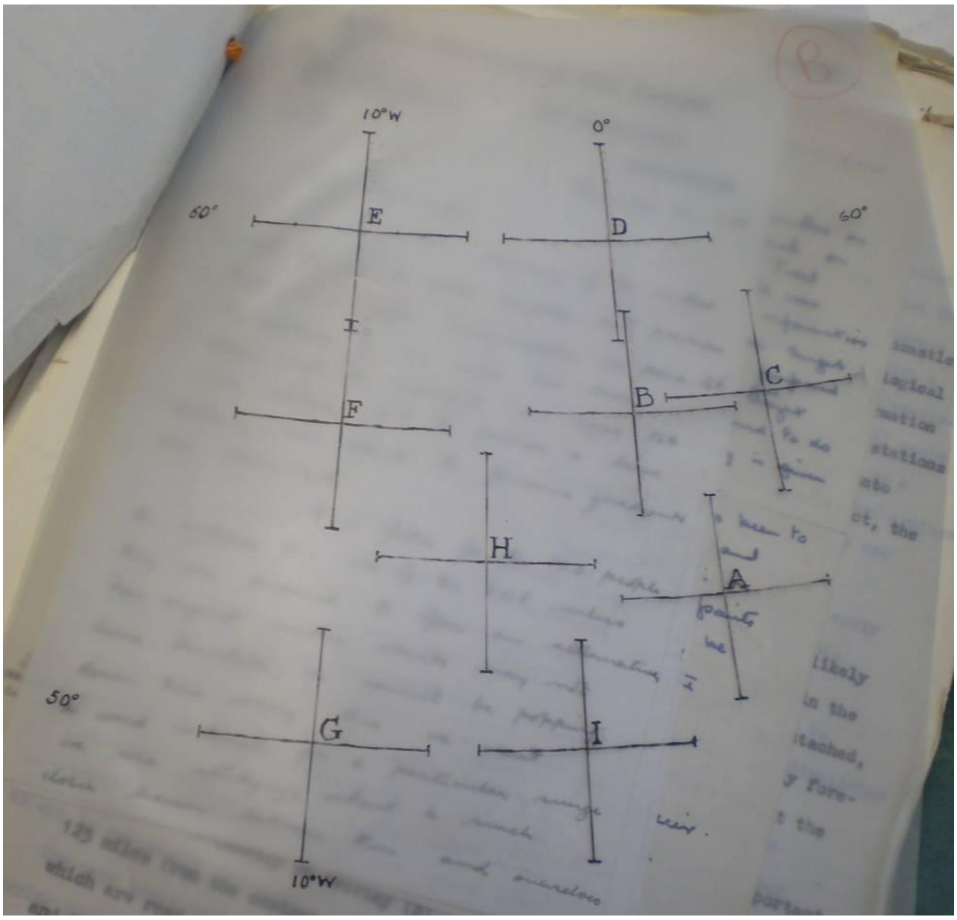


Figure 7.1: The paper version of TI's scale for reading pressure gradients off the Daily Weather Report included in Berncastle's memo[120]

The data tables begun by the computer were then passed onto a more senior computer or to Rossiter, who converted the values tabulated by the assistant computer into two values E and N. This was done via intermediate values which were not necessarily written down and came from reading off numbers from three tables. These different tables represented component parts of a formula for calculating gradient winds. As the tables were pre-prepared, no actual computations were involved in converting the barometric gradients into E and N. Instead the numbers could be gradually converted, in three steps, by reading straight off the three tables. While this task could have been done by a junior computer with less mathematical training, the absence of an instruction sheet for them implies that the person at TI worked of the theory sheet and thus that it

[120] Sheet B of the memo "Use of Meteorological Data for Storm Surge Research by the Liverpool Tidal Institute", Berncastle, undated, late Oct or early Nov 1955, HW 287.54, UKHO

was more senior member of staff who did the work. Perhaps it was not deemed worthwhile in terms of time to make this part of the research project into a routine procedure.

The person doing these conversions probably sat with the three tables in front of them, converting the different numbers by finding the right location in the first table, moving straight to the next table to read off the next figure and then using the third table twice, once for each pressure gradient. Between the tables the numbers might have been kept solely in the head or by keeping a finger or pen at the location in the previous table. Not writing the figures down is likely to have produced fewer errors as there would be no transcription errors and it would certainly have been much faster. The end product, the numbers for E and N, could then be inserted into the formula for gradient winds, which were then calculated, probably using a calculating machine.

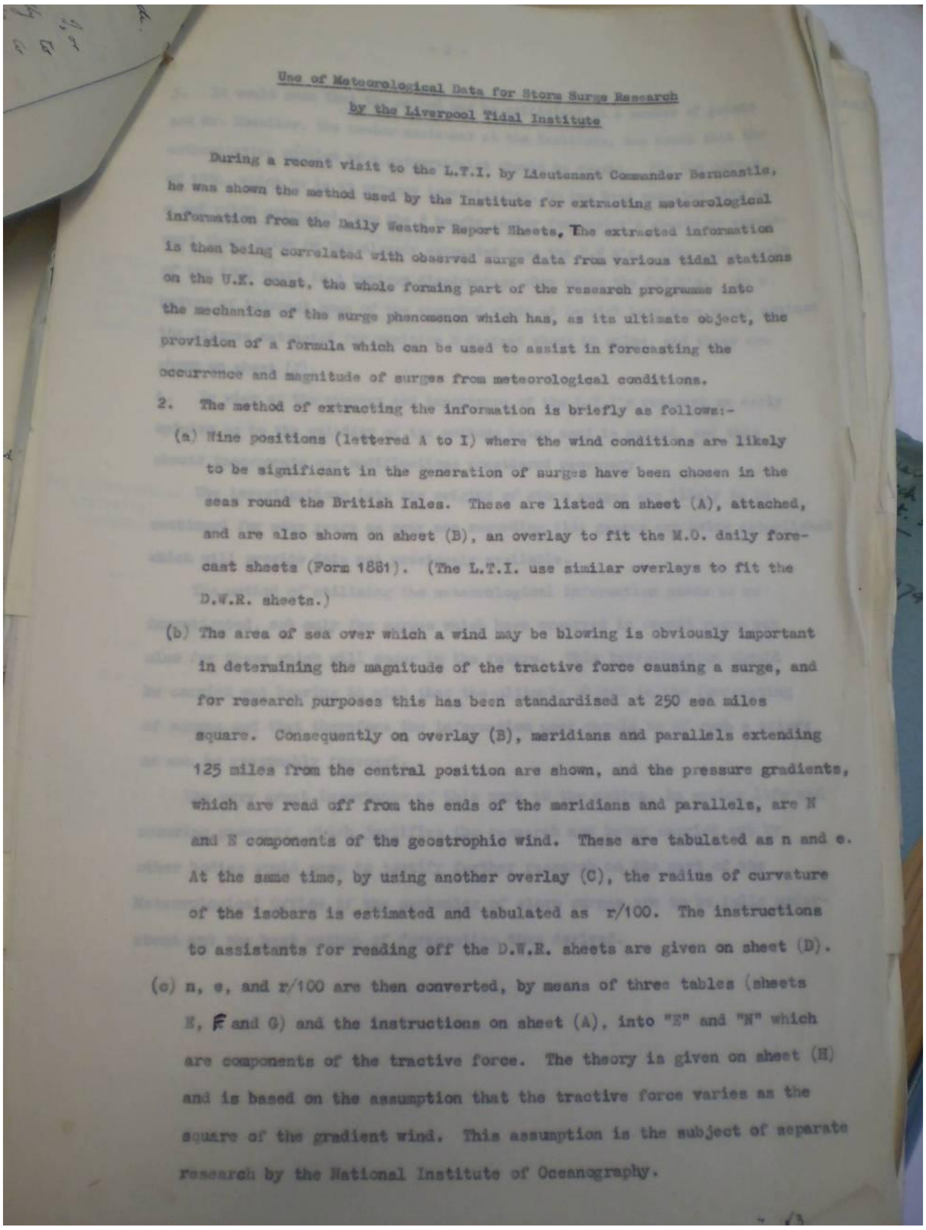
The components of TI's practices of constructing statistical forecasting formulae are by now familiar. The work done at TI consisted of calculating or reading off numbers, for example from graphs such as tidal gauge records, meteorological charts or tables, and then writing these numbers down on particular documents. Finding the numbers for the data tables was done using various aids, in this case glass scales and the formulae tables, but often also including various calculating machines, including the tide-predictors. As pointed out earlier the skill of reading off values was key. Checking the figures produced was another important part of the job, both for the initial computer, as seen in the instruction for the computer to check they put the correct sign for the pressure gradients, and also by others at a later stage. The end result was more numbers in more tables or in graphs. This kind of work was at the core of what TI did to try to make surges more predictable.

An important part of the work of the researchers at TI was to work out how to use the theory, e.g. of gradient winds, and then to organise the data tables and calculation aids, such as the scales and the conversion tables in this case, for the computers to do the calculations involved. In order to calculate winds for their work on storm surges they not only had to work out how to calculate gradient wind in theory but also how to do it in practice, and then organise this work. This organisation was in itself work, for example figuring out which documents to use how and when, preparing these documents and materials such as the scales, and explaining the work to the junior computers. They also had to decide if it was worth making the calculations into such a routine, as was decided *not* to do with the conversion of values into E and N above. Berncastle's memo provides an example of the work involved in organising chains of documents, as he wrote down instructions for which document to use when and how, see figure 7.2. To construct these instructions Berncastle first had to collect and organise the information and then write it up clearly, i.e. do work on the organisation of chains of documents. His memo made explicit the often implicit organisation TI's researchers did as a key part of their work.

As was discussed in the previous chapter, the creation of chains of documents involved choices. Here I have emphasised the work involved in organising chains of documents, which involved many choices as well as time and effort. As a centre of calculation TI created and organised documents, but these documents had specific content, which had to be organised in specific ways to produce results (or supplements) and this organisation took work. Their documents did not just appear but were constructed with time, experimentation and effort.[121] The ability to organise documents was of course not a special cognitive ability, but a learnt skill, as was emphasised when discussing Doodson's early career.

In addition, as the debate regarding how to calculate wind shows, the choices and practices involved in constructing and organising documents could be contested. This was also discussed in the 1928 chapter, when the construction of documents and storm surges were contested between PLA and TI. The inevitable choice of particular practices of calculation, of particular ways of organising the work, for example of how to calculate winds, led to differences in approaches between different institutions. When such different practices were brought together, as they were in the framework of the Advisory Committee, such differences led to friction and contests between different approaches. There was competition between different centres of calculation, in this case the Met Office and TI, regarding what documents to organise and how to organise them, with TI being criticised for and made to justify their choice of gradient wind.

[121] It was similar in businesses, see JoAnne Yates, *Control through Communication : The Rise of System in American Management* (London: The Johns Hopkins Press Ltd, 1989), 80-85.



Use of Meteorological Data for Storm Surge Research
by the Liverpool Tidal Institute

During a recent visit to the L.T.I. by Lieutenant Commander Berncastle, he was shown the method used by the Institute for extracting meteorological information from the Daily Weather Report Sheets. The extracted information is then being correlated with observed surge data from various tidal stations on the U.K. coast, the whole forming part of the research programme into the mechanics of the surge phenomenon which has, as its ultimate object, the provision of a formula which can be used to assist in forecasting the occurrence and magnitude of surges from meteorological conditions.

2. The method of extracting the information is briefly as follows:-

(a) Nine positions (lettered A to I) where the wind conditions are likely to be significant in the generation of surges have been chosen in the seas round the British Isles. These are listed on sheet (A), attached, and are also shown on sheet (B), an overlay to fit the M.O. daily forecast sheets (Form 1881). (The L.T.I. use similar overlays to fit the D.W.R. sheets.)

(b) The area of sea over which a wind may be blowing is obviously important in determining the magnitude of the tractive force causing a surge, and for research purposes this has been standardised at 250 sea miles square. Consequently on overlay (B), meridians and parallels extending 125 miles from the central position are shown, and the pressure gradients, which are read off from the ends of the meridians and parallels, are N and E components of the geostrophic wind. These are tabulated as n and e. At the same time, by using another overlay (C), the radius of curvature of the isobars is estimated and tabulated as r/100. The instructions to assistants for reading off the D.W.R. sheets are given on sheet (D).

(c) n, e, and r/100 are then converted, by means of three tables (sheets E, F and G) and the instructions on sheet (A), into "E" and "N" which are components of the tractive force. The theory is given on sheet (H) and is based on the assumption that the tractive force varies as the square of the gradient wind. This assumption is the subject of separate research by the National Institute of Oceanography.

Figure 7.2: Berncastle's overview form/instructions[122]

In the construction of TI's chains of documents, such competition between different centres of calculation was important as one of the many contingencies discussed throughout the thesis that influenced the shape of the chains. Another example of such contingencies is the role of Hydro's request during the Second World War influencing TI's use of statistical methods. The shape of the chains in turn influenced the

122 "Use of Meteorological Data for Storm Surge Research by the Liverpool Tidal Institute", Berncastle, undated, late Oct or early Nov 1955, HW 287.54, UKHO

supplement or result achieved by the researchers organising the chain. For example, by the mid-1950s TI argued that the choice of statistics limited the results it was possible to achieve when forecasting surges and that new chains of documents would be necessary to get further supplements from the results. Such further supplements were defined as further reduction of the differences between predicted and observed residuals.

7.5 CHANGES AT TI

Around 1960 several things changed at TI. Rossiter introduced digital computers into storm surge work, Doodson retired and TI was taken over by the University. These developments form a suitable endpoint to the thesis and will be discussed in this section.

TI started using new practices of calculation involving electronic computers in late 1959 for work on tide-surge interaction. TI had worked on this problem of the highest possible combination of tides and surges given the potential interaction between the height of the water, tide and surge since 1928. Various work had been done since 1953 using human computers, since when the issue had also been emphasised by Suthons as important for the warning system. Rossiter now introduced digital computers to the work and after he presented his initial findings in spring 1960 the Advisory Committee asked him to continue with the work.[123] Doodson was generally not in favour of digital computers, repeatedly arguing against their use in periodic tidal prediction, saying they would be more expensive while producing less accurate results than the tidal predictor machines produced.[124] However, when Rossiter first suggested using digital computers for this problem he reported that "at long last" TI had found a computational problem (that of storm-surge interaction) that they considered worth using a digital computer

[123] Advisory Committee on Oceanographic and Meteorological Research, Minutes of thirteenth meeting held on Monday, 16th May, 1960, and attached report 13(v) "Interaction between tide and surge" by Rossiter, Box 160, BA

[124] This had to do with the identification of high and low waters, which Doodson claimed was easy to do by eye from a curve produced by a predictor machine but less easy to programme a digital computer to do. For example, when asked during the BAAS meeting in Liverpool in 1953, see LOTI, *Annual Report 1953*, 9.

for. This suggests that even Doodson had been convinced that new practices of calculation along these lines should be attempted.[125]

In terms of this thesis, the implication of this work on interaction was that Rossiter was introducing new practices of calculation to TI's storm surge work. Digital computers were taking over from hand calculations; computer modelling from statistics. The availability of digital computers offered a new way to attack this particular aspect of forecasts, with TI's researchers again arguing that they could 'improve' the surge forecasts by introducing new practices of calculation, though they needed funding and time to do this. While aspects of the statistically based work continued into the 1960s, TI's preferred method of investigation and its preferred practice of calculations changed, away from statistics.[126]

This was not the only change taking place at TI. In May 1959, at the instigation of MDHB, discussions were begun regarding the future of the Liverpool Observatory and Tidal Institute's (LOTI's) future. MDHB wanted to divest their responsibilities, claiming this was for the good of the increasingly wide-ranging scientific work TI was doing. They wanted to ensure the "valuable work" continued and increased, and felt the University would be better able to ensure this than MDHB after Doodson's eminent retirement at 70 years of age. In addition there were financial reasons: MDHB felt they should not be the only Port Authority to "be financing research for the maritime nations of the world", i.e., they no longer wanted to pay for oceanographic research they felt was the interest of the state.[127] While MDHB's decreased interest in LOTI was also linked to changes to the port and the local shipping industry in the 1950s, with containerisation beginning and the number of employed in these industries declining in Liverpool,[128] these changes were not yet strong enough for finance to be the main motive for MDHB's decision to divest LOTI, especially as TI's income continued to

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125 Advisory Committee on Oceanographic and Meteorological Research, Minutes of twelfth meeting held on Monday, 9th November, 1959, Report 12(v), "A further report on the interaction between tide and Surge", Rossiter, p 2, Box 160, BA

126 This did not only happen at TI, of course, and in 1960 Rossiter received a request for co-operation from Professor Hansen at Hamburg University and some French workers, to do research on surges using electronic computers. This was also a sign of the increased international co-operation in the field of storm surge science, which was another change taking place. Advisory Committee on Oceanographic and Meteorological Research, Minutes of fourteenth meeting held on Monday, 24th October, 1960, Box 160, BA.

127 Draft memorandum, 1st Jun 1959, P744/5, LUA

128 Tony Lane, *Liverpool: City of the Sea* (Liverpool: Liverpool University Press, 1997), 22.

increase in this period (see table 4.1).[129] Instead their motive was linked to the types of work LOTI was now doing, as well as to the changing landscape of funding of research, e.g. increasing state funding and involvement in research.[130] One part of this was TI's work on storm surges for MAFF. When NIO was established, MDHB had re-affirmed it wanted to stay a patron of LOTI, but they had now changed their mind, considering this patronage as providing benefit primarily to the nation, with the implication that it should be the responsibility of the state, not industry.

However, while direct industrial patronage decreased, state patronage of TI did not increase with this move. Instead the University took over TI and the Observatory in December 1960, complete with the profitable but routine tidal prediction work. The existence of this work meant the University thought taking over TI would not have serious financial implications[131] and that they did not need to apply for a grant from the University Grants Committee for LOTI in the foreseeable future.[132] It was expected that the tidal prediction and analysis work, together with limited aid from the university, would pay for LOTI. TI's patronage structure thus remained dependent on indirect military and industrial patronage through the purchase of predictions, but this was combined with increased dependence on the university. In terms of governance, the new governing committee had only one member from the Mersey Docks and Harbour Board, compared to five representatives before. The University contingent increased to seven from four while the NIO and Admiralty representatives remained one and two respectively.[133] While the shipping industry retained some representation and patronage,[134] the governance and patronage structure of LOTI which had lasted for thirty years changed, with the balance swinging away from industry, not towards the state but towards the university. The role of the Admiralty remained unchanged.

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129 Rates and dues received by MDHB increased steadily during the 1950s and into the 1960s. Mountfield, *Western Gateway*, 205-206.

130 See e.g. de Chadarevian, *Designs for Life*, ch 2; Edgerton, *Warfare State*.

131 The decision was taken by a committee consisting of Vice-Chancellor Mountford, the Dean of Faculty of Science (Professor of Oceanography Bowden) and five other professors. Mountford to Bowden, 24th Mar 1960, D498/2/4/3, LUA

132 Correspondence in UGC 7/726, NA

133 LOTI, *Annual Report 1960* (Liverpool: C. Tinling and Co. Ltd., 1960). See also Mountford to Bowden, 24th Mar 1960, D498/2/4/3, LUA. The Admiralty membership of the governing committee had increased from one to two in 1939. This had been compensated for by a reduction in representatives from the university, see LOTI, *Annual Report 1939* (Liverpool: University of Liverpool, 1939), 5.

134 Particularly spatial patronage, as MDHB continued to provide the Observatory building for the use of TI's successors until 2004.

The takeover followed discussions and consultations which provide examples of how TI and especially its patronage and governance structure were viewed by different actors. The institutional geography of oceanography in Britain was again debated, as it had been when NIO was being established a decade or so earlier. All of those who were consulted by the University, the University Grants Committee and MDHB regarded the take-over as sensible, and indeed desirable. Many in fact expressed a sense that LOTI's current set-up was old-fashioned. For example, Proudman thought the previous model of governance, a scientific institution owned and run by businessmen, was "a very fine one, but it belonged to a world that has gone".[135]

Two Cambridge-based scientists were highly critical of TI's existing set-up. Edward Bullard, of the Department of Geodesy and Geophysics at Cambridge University, thought the Bidston site should be abandoned and that close co-operation with other Departments, especially Oceanography, was needed as "work on tides is a somewhat narrow field".[136] The chairman of University Grants Committee Sir Keith A H Murray, consulted his friend,[137] the physicist Sir George Thomson, master of Corpus Christi College, Cambridge, who despite not knowing either Doodson or TI's work suspected the Institute was "completely fossilised" and would become more so if it was run by the Government. He thus suggested supporting the take-over by the University.[138]

While all approved of the University taking over, Deacon and Doodson both emphasised the need to retain work on predictions and links to industry. Deacon, who was very positive regarding TI,[139] argued against merging LOTI with other departments and for maintaining close contacts with "sailors and engineers", arguing that subjects like marine science needed the goodwill of industry as well as of science.[140] When Doodson was asked to outline what research needs he saw in tidal science that could be filled by an Institute like TI, he emphasised theoretical work but also repeatedly underlined the "intimate association" between the commercial work and the research. For example, in relation to surges he claimed that it was not even possible to know the height of a surge without precise hour-by-hour predictions (this was of course linked to

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135 Proudman to Mountford, 29th Oct 1959, P744/5, LUA
136 Bullard to Mountford, 26th Oct 1959, P744/5, LUA
137 Judging from the friendly greetings and informal tone of the letters.
138 Thomson to Murray, 4th Jan 1959, UGC 7/726, NA
139 The working relationship between NIO and TI, and between Deacon and Doodson, appears to have been good, despite the issues regarding NIO's establishment.
140 Deacon to Mountford, 9th Nov 1959, P744/5, LUA

his definition of a surge as the residual of observation minus prediction). As such predictions were not produced during routine prediction work they could not be bought from elsewhere, meaning it was necessary to have constant access to a tidal predictor machine for TI's work on storm surges. In addition he argued the warning system's forecasters needed "very trustworthy" predictions for their work. He concluded: "Research is thus intimately associated with [tidal] prediction".[141] By linking the two Doodson was also providing an academic rationale to keep the important income stream of predictions going.

Three state actors were involved: the University Grants Committee, the Ministry of Transport and the Admiralty. After Murray consulted Thomson, as discussed above, and also PMS Blackett, Professor of Physics at Imperial College, the University Grants Committee asked the University for some clarification of the reasons why it was proposed to keep TI independent of other departments. Having received such clarification, the University Grants Committee then supported the University taking over TI.[142] The Admiralty emphasised predictions. Initially Hydrographer Collins was positive towards the merger, as long as there was no break in the provision of predictions, and indeed suggested nationalising the Institute.[143] Later on in the take-over process, a new Hydrographer, Irving, was worried the university would de-emphasise predictions and asked them to re-affirm the 1923 agreement of three year's notice before ceasing to provide predictions.[144] The Ministry of Transport's officers, who had consulted Irving, internally called the proposal "entirely reasonable" and did "not see any grounds for objection", but did not give the necessary permission from the Mersey Conservancy for the take-over until Irving had been satisfied.[145] Only after the Vice-Chancellor re-assured the Hydrographer that the 1923 agreement would remain valid and emphasised the close connection between the research work and the predictions

141 "Research on tides appropriate to a Tidal Institute", attached to [Mountfield? Illegible signature] to Mountford, 14th Jul 1959, P744/5, LUA
142 Blackett supported the University proposal, but also raised some question marks regarding why LOTI was not being merged with the Department of Oceanography. In response to Murray's question to clarify this, Mountford said there was lack of space on campus and that one person could not manage the two sites, and also that LOTI was more likely to get a "first class" Director if it was maintained separately. Murray to Mountford, 25th Jan 1959, Mountford to Murray, 29th Jan 1960, & Blackett to Murray, 21st Jan 1959, UGC 7/726, NA
143 Hydrographer Collins to Mountford, 16th Nov 1959, P744/5, LUA
144 Hydrographer Irving to Vice-Chancellor Mountford, 10th Aug 1960, P744/5, LUA
145 Law to Mountfield, 10th Aug 1960, MT 76/57, NA

(using Doodson's arguments discussed above)[146] did the Admiralty give its support for the take-over. In turn the Minister of Transport gave his formal consent for the plan.[147]

What these discussions show is that state patronage of oceanography was not obvious, even in this period of increasingly high state involvement in science, and neither did scientists agree on how to organise oceanography. The only state actor which showed any interest in increasing state patronage of TI's work was the Hydrographer, and that only briefly, which shows that the state did not see TI's oceanographic work as something that it was an obvious patron of any more now than it had previously been. As long as it continued to provide predictions to Hydro, the different government departments were not particularly concerned about TI's patronage or its governance structure, and were happy for the university to take over. Neither did academics ask for further state involvement. Instead arguments for increased state patronage came from industry, with MDHB wanting to reduce its involvement.

In terms of the views of scientists, some outsiders, especially at Cambridge, were somewhat dubious of the quality of work being done at TI. Though they knew little of it they seemed to think that a small, relatively isolated institution with heavy involvement from industry *must* be "fossilised" and was unlikely to be producing good work. They were arguing for a different kind of science, being done in larger institutions more closely connected with universities, not with the state or industry. Even Proudman thought LOTI's governance structure was outdated and limited the work the Institute was able to do. As chairman of the Advisory Committee he wrote that "[b]ecause of shortness of scientific staff the T.I. just cannot find time to do what we ask".[148] However, he and others more closely involved with the work, like Deacon and the two Hydrographers, were much more positive of LOTI's work and clearly did not have a problem with the quality of science done at TI. Deacon's support of TI and its work is interesting, as his own NIO represented the type of larger institute with many connections to the state that have become seen as typical of oceanography, but he still clearly saw a role for an institute run like TI. This represents a belief in the science produced at other types of set-ups than those favoured by the Cambridge-based

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146 Mountford to Irving, 5th Sep 1960, P744/5, LUA
147 Draper to Roberts, 21st Sep 1960, and Law (Acting Conservator of the River Mersey) to The General Manager and Secretary, MDHB, 28th Sep 1960, MT 76/57, NA
148 Proudman to Mountford, 29th Oct 1959, P744/5, LUA

scientists. The most desirable governance and patronage structure of oceanography was not self-evident or uncontested in the UK in 1960, which will be further discussed below and in the thesis conclusion.

7.6 Conclusion

Rather than there being little or no political debate about blame and responsibility after the 1953 East Coast Flood there was substantial political pressure on the government, which responded to this by providing relatively generous funding and also by setting up the expert Waverley Committee. Enlisting experts had two benefits for the government: it meant the opposition could not complain it was not doing enough as long as the government did what the experts said, while also ensuring that it was not doing 'too much'. For example, the Waverley Committee emphasised cost-benefit analysis regarding flood defences, limiting the expense on these compared to what other political actors had argued for.[149] While the politicians were using the experts, the Waverley Committee of course also had its own agenda, parts of which were markedly shaped by one of its scientific members, Proudman. The research programme his Oceanographical Sub-Committee designed was picked up by one section of MAF that claimed to be more inclined towards scientific research than other government departments. MAF provided a supportive base within central government for storm surge science and the Advisory Committee, which became the framework for TI's work on this subject throughout the 1950s.

The new framework led to an intensification of TI's work and increased scrutiny of their practices, both by themselves and others. Together with issues of departmental politics this led to TI's practices being questioned. The questioning focused on how to measure and calculate winds and also how formulae could best be found – through statistical or other approaches. This debate also provides another example of how TI's choices of organising documents into particular chains bound them just as much as they connected them. Eventually, after the Advisory Committee had asked TI to prioritise statistical formulae, they constructed formulae which they proclaimed were as good as the statistical approach allowed. They also stated that other chains of documents needed

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149 *Report of the Departmental Committee on Coastal Flooding [Waverley Committee]*, para 4.

to be constructed to get further improvements. These claims of 'success' were of course contextual and contingent, but as this thesis focuses on TI's attempts at making surges predictable through statistical formulae, which they and Suthons at this point thought they had done, this together with TI's move away from statistical practice marks the end of this thesis.

This chapter has discussed the role of state patronage of both storm surge science and of TI generally. Most history of oceanography has emphasised a large increase in government patronage of oceanographic research in the 1950s and particularly focused on naval patronage. Similarly it has been argued by David Edgerton that the increased research funding in agricultural departments such as MAF in Britain in the 1950s had military over-tones of ensuring agricultural self-sufficiency.[150] However, while storm surge science saw a radical increase in state funding after the 1953 event, I have found no evidence this was linked to military needs. It was instead linked to political pressure. The Treasury's unusually generous response after the event and, by implication, towards the research, was shaped by this pressure from opposition politicians. This response by the Treasury was in stark contrast to the interwar situation. Then the Treasury consistently saw storm surge research as a local concern which it did not want to pay for, fearing that paying for the research would lead to requests to pay for flood defences. Now the issue was turned on its head, with opposition pressure leading to increased state funding for flood defences, and the Waverley Committee aimed at balancing or mitigating these costs. As a recommendation of that Committee, and framed as hopefully decreasing the costs of false alarms, TI's research received central government patronage, not to fulfil military needs but within a context of political contests and discussions between the government and the opposition and between different government departments.

While Hydro was involved in the warning system, they were in a sense as much clients of the same patrons as TI, running the warning system for other state actors. On the other hand, through their involvement in the Advisory Committee, where Suthons suggested lines of research to TI and NIO, they returned to the kind of patronage they had had of TI's very early work where they influenced the lines of research without paying for it. The patronage of storm surge science after the East Coast Flood was a

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150 Gummett, *Scientists in Whitehall*, 37-40; Edgerton, *Warfare State*, 103-107.

complicated mix without clear borders between civil, military, academic and, to a lesser extent, industrial patronage.[151] It involved not only monetary support but also other kinds of patronage, such as influence over the content of research projects and the relationships between Hydro, Proudman and TI through which the Institute got involved with the Advisory Committee.

Apart from the increase in funding from MAF for storm surge work, TI's general patronage structure remained as before until the late 1950s. With the take-over in 1960 TI's overall patronage structure moved towards university patronage, not state patronage, which again contrasts with the existing literature on the history of oceanography.[152] Case studies such as this one and that of Ronald Doel *et al*'s provides nuances both to the history of oceanography and to Edgerton's idea of oceanography as a state science.[153] It was not self-evident or uncontested that oceanography should be a state or military concern in the UK in 1960, and when it was a state concern, as it became in the case of storm surge science, the links between central government, military, academia and industry were complex.

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151 Contrast with Doel, Levin, and Marker, "Extending Modern Cartography to the Ocean Depths: Military Patronage, Cold War Priorities, and the Heezen-Tharp Mapping Project, 1952-1959."
152 Weir, *Ocean in Common*; Hamblin, *Oceanographers and the Cold War*; Doel, "Constituting the Postwar Earth Sciences."
153 Doel, Levin, and Marker, "Extending Modern Cartography to the Ocean Depths: Military Patronage, Cold War Priorities, and the Heezen-Tharp Mapping Project, 1952-1959."; Edgerton, *Warfare State*, 112.

Chapter 8, Conclusion

This thesis has argued that TI attempted to make storm surges more predictable by contingently introducing particular statistical practices of calculation which produced chains of documents. These chains connected changing sea levels to TI's mathematical analysis, enabling TI's researchers to attempt forecasts of surges. These chains also bound TI's researchers, limiting their future choices and work. TI's researchers constructed such chains of documents to forecast surges to fulfil promises to or contracts with patrons. Such promises and contracts were in turn linked to flooding events as well as other events and processes, such as wars, wider patronage and political disputes. This conclusion will summarise and analyse my key findings, discuss alternative approaches to the thesis and suggest some further work.

8.1 History of oceanography

8.1.1 The contested patronage of oceanographic science

I have repeatedly discussed the patronage structure of TI and of storm surges science at TI during this thesis. Both were complex, involving a range of academic, industrial, military and government, both local and central, patrons. TI's patronage settled into a particular pattern in 1929, which though it was debated around the time of NIO's establishment remained similar until 1960. This was a triangular patronage structure: Liverpool University provided connections to academia, a room and a small grant, MDHB provided a more substantial grant, a building and connections to the shipping industry, and Hydro provided a secure, long-term source of income by purchasing tidal analysis and predictions and influenced the running of TI and its research programme by being on the governing committee. In addition TI received income from other sources, such as the sale of predictions to almanacs and other clients, and project work, such as what they did on storm surges for LCC and MAF. Both naval and industrial patronage was clearly important for TI, not only for income but also for the governance of the institute. In addition, while the thesis has not focused on this after chapter four, a key part of TI's research programme was to develop methods of tidal predictions and analysis, so there were obvious links between TI's programme of work and the patronage it received from naval and merchant shipping sources.

From 1928 onwards the development of patronage of storm surge science was somewhat different to that of TI. The Institute's work on storm surges then went from having local industrial patronage and a focus on correcting tidal predictions for meteorological effects, to having local government patronage and a focus on forecasting floods. In the 1930s the civil state repeatedly refused to become a patron of surge science. However, during and after the Second World War both the UK and US Navies appropriated TI's surge work in different ways. While Hydro may not have provided any further resources, its interest in TI's work during the war led to a shift in TI's practices of calculation towards statistics. This interest was part of the wider patronage relationship between Hydro and TI. Following the 1953 storm surge event, the patronage structure of storm surge science in the UK, including at TI, became focused on the state, especially MAF, but a large number of actors – state, military and scientific – were involved in the work. While the funding came from MAF and the Treasury, the requests that shaped the research programme came from Hydro, which was in turn running the warning system primarily for the benefit of local and central government. Patronage of storm surge science was a complex mix of military, industrial and state support. Like in the rest of TI's work there were substantial links between the patronage and the work. This was not only in response to Hydro's request during the war but also, for example, when the initial interest in both positive and negative storm surges, prompted by the needs of TI's shipping patrons, was retained when the patronage shifted towards local authorities interested in the positive surges that can cause flooding.

In many ways TI was an untypical scientific institute, for example in its small size and its triangular patronage structure with a high dependency on contract work, both repeat contracts and short-term ones. Especially initially, Proudman and Doodson tried to get support wherever they could think of for their Institute, applying to BAAS, the Booths, DSIR, LSOA and Hydro. Their sometimes planned, sometimes improvised, institution building strategy resulted in an organisation balanced between the industrial, academic and military worlds. In terms of comparators, TI was somewhat like the Co-operative Research Associations set up by DSIR, but unlike many of them it soon stopped receiving grants from that organisation. It was also focused on a topic of research as opposed to the needs of an industry more generally.[1] Other organisations that received limited grants from DSIR may be potential comparators. Studying institutions like TI provides insights into which patrons were sufficiently interested in a particular topic to support research work on it, how this changed over time and how such patronage influenced the work done by the researchers. In addition, one can study how the interests of different patrons and researchers were negotiated, what the balance of power between patrons and researchers were and what kind of rhetoric TI used to gain patronage.

While I for convenience have sometimes talked about state, military and industrial patronage, I have also tried to break these categories down as much as possible, as TI's patronage was not from 'the state' or 'the military' but from specific departments or even specific people within such departments. Both the patronage of TI and that of its storm surge science depended on personal networks and institutional politics. This was not only between TI and its patrons, but also between different patrons whether actual or potential, as between the potential patrons LCC and the Treasury, or MAF and the Home Office, or actual patrons, as between MDHB and the University. In addition institutional politics existed within patrons, as within Hydro when they discussed whether to give TI their contract in 1923. To understand the patronage structure of a discipline, an institute, or one part of that institute's work, it is necessary to trace out the complicated dynamics at play. For example, while oceanography as a whole in Britain saw a rise in naval patronage after the Second World War, this went primarily to NIO, while TI remained much as before.

While TI was a small and perhaps unusual organisation, the story told about it in this thesis suggests that other sources of patronage of physical oceanography could fruitfully be considered in addition to the existing emphasis on naval patronage, especially in the 1940s and 50s. While civil and military patronage of TI and storm surge work increased, particularly in the 1950s when MAF became the main patron of storm surge science, a range of actors were constantly involved. Between 1919 and 1960 different kinds of patrons and patronage interacted with TI's storm surge work in different ways. This was sometimes obvious, as during the creation of the research inquiry after the flooding in

[1] Rose and Rose, *Science and Society*, 45; Varcoe, *Organizing for Science in Britain*; Hull, "War of Words."; Clarke, "Pure Science with a Practical Aim."; Ian Varcoe, "Co-Operative Research Associations in British Industry, 1918–34," *Minerva* 19, no. 3 (1981).

1928 by the Sub-Committee of the Conference when the Admiralty got TI involved. At other times such interactions between patronage and science were more subtle, such as the shift to gradient winds during the early stages of the Advisory Committee's work, or sometimes very little, such as the small role industrial actors played following the flood event in 1953. In order to understand how patronage affected oceanographic science overall it is necessary to look at different kinds of detailed case studies, including all sources of patronage.[2]

8.1.2 THE CONTESTED NATURE OF OCEANOGRAPHY

As discussed in the introduction, Eric Mills has recently written a history of physical oceanography, concentrating on the study of ocean currents using a particular dynamical mathematical framework first developed in Scandinavia at the end of the nineteenth century. While he emphasises contingencies in the uptake of this approach, he argues that the dynamical approach made oceanography a quantitative science and formed an important part of the origin of modern oceanography through its influence on the textbook *The Oceans* published in 1942.[3] The history of TI, as described in this thesis, adds another dimension to Mills's narrative of the origins of modern oceanography. In Britain, which Mills does not discuss, there appears to have existed different traditions of mathematical oceanographic work in the interwar and post war period. Doodson and Proudman were part of a mathematical tradition stemming from the adaptation of Laplace's work in Cambridge. TI came out of this tradition coupled with the context of the First World War. NIO instead came out of the context of the Second World War, with George Deacon also closer to the tradition of European dynamical oceanography.[4] Deacon was keen on *The Oceans* and thus represents the

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[2] In part the difference between my findings, emphasising a range of patrons, and those historians of oceanography that focus on naval patronage, comes down to differences in approach. For example, Weir's book concentrated on the links between the Navy and oceanography, which obviously made him pick out naval patronage, while Hamblin concentrated on international co-operation, thus focusing on larger scale research programmes. My approach has instead focused on the detailed interactions between patrons at a small institute and for a particular strand of their work, which has meant that I have concentrated on the complications and multiplicities in their patronage. The different approaches produce complementary pictures of the history of oceanography.
[3] Mills, *The Fluid Envelope of Our Planet*.
[4] Deacon and Laughton, "Of Seas and Ships and Scientists," 34.

tradition which Mills portrays as the foundation of modern oceanography.[5] However, TI continued in existence and Proudman and Doodson contested the NIO's concentration on Deacon's kind of oceanography.

In Britain in the twentieth century there has been several different approaches to physical oceanography, such as dynamical oceanography from Scandinavia, the tradition stemming from Laplace which much of Proudman's and Doodson's theoretical work fell into, and TI's statistical approach to storm surge forecasting. This complicates the story of the origin of modern oceanography, at least for Britain. If the assumption that dynamical oceanography was the eventually successful variety of oceanography holds, what made that version 'win' and TI's approach 'lose'? Both during the establishment of NIO and the university takeover of TI the nature of oceanography in Britain was one of the issues at stake. The type of oceanography that should be practiced at NIO was a key aspect of the debate regarding its establishment and during the University takeover of TI the kind of institution that would produce 'good' oceanographic research was also raised. These debates illustrate that the nature of oceanography was not obvious to those who were involved in deciding the future of UK oceanography in the 1940s and 50s. The 'ascendancy' of modern, dynamically-inspired, oceanography was contested and contingent in Britain. In addition, even Deacon, whose NIO belonged to the 'winning' variety of oceanography, argued for the continuation of TI as a different kind of institution that could and did produce good oceanography.

This also raises the issue of the definition of 'modern' British oceanography. Did dynamical oceanography in fact 'win' – was it the origin of modern oceanography? I would argue that British oceanography is less monolithic than a singular origin in dynamical oceanography implies. Jacob Darwin Hamblin has discussed the importance of contestations between different oceanographers in relation to international co-operation in oceanography in the 1950s, describing it as a debate between physical oceanographers (such as Proudman, Doodson and, according to Hamblin, Deacon) and those arguing for a wider definition of oceanography.[6] The debates about what

[5] Mills, *The Fluid Envelope of Our Planet*, 273. In a memo to the 1944 Royal Society discussions on the establishment of NIO, Deacon claims he is making full use of "opinions expressed" in Sverdrup's *The Ocean,* as well as in Bigelow's *Oceanography* and Kemp's *Oceanography in relation to the fluctuations of Marine Animals.* Appendix by Deacon, prepared for meeting on 1st Mar 1944, labeled Appendix to 67, H 02455/43, UKHO.
[6] Hamblin, *Oceanographers and the Cold War*, 100-116.

oceanography should be, how it should be organised and who should pay for it did not end in 1960 in Britain – in fact, they are ongoing to this day. A number of different set-ups involving versions of TI and NIO have been tried, the most recent being a merger of the National Oceanography Centre in Southampton (NIO's successor) and the Proudman Oceanography Laboratory (TI's successor) in April 2010. While both share a title they remain in their previous physical locations.[7] The post-war institutional geography of British oceanography has remained unresolved – the settlement of NIO near London did not settle the debates. To understand the history of British oceanography I believe historians need to pay further attention to the wide range of approaches Harald Sverdrup included within oceanography and *The Oceans*.[8] Paying attention to contestations regarding the meaning of oceanography may not only help us understand the development of international oceanography, as discussed by Hamblin, but also that of 'national' oceanography.

8.1.3 Further work: The history of British twentieth century oceanography

Both these two sources of complexity – non-naval patronage of oceanography and the contested nature of oceanography – could be further addressed by a study of the history of oceanography in the UK in the twentieth century, including TI, NIO and other institutions such as the Department of Oceanography at Liverpool, the Department of Geodesy and Geophysics at Cambridge and research stations involved in biological oceanography. It may also want to include institutions like the National Physical Laboratory and the Hydraulics Research Station, as well as naval research stations. Such

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[7] Focusing on Bidston-based oceanographic work, in summary the development between 1960 and 2010 looks like this: the set-up with the University, created in 1960, only lasted until 1969, when the *Institute of Coastal Oceanography and Tides* was created through a transfer of TI to the Natural Environment Research Council (NERC). That lasted until 1973, when the *Institute of Oceanographic Sciences* replaced it, being formed through a merger of existing institutions including the former TI and NIO. In 1987 the Wormley (former NIO) and Bidston (former TI) laboratories were de-merged, with the Bidston one named *Proudman Oceanographic Laboratory* (POL), remaining under NERC. In 1994 POL's place within NERC was reconsidered, with it falling under the *Centre for Coastal and Marine Sciences* (CCMS), together with some other oceanographic laboratories in Plymouth and Dunstaffnage, but not the Southampton successor of NIO. In 1997 CCMS completely absorbed POL, but in 2001 CCMS was disbanded and POL re-emerged, still funded by NERC but independent. In 2004 POL moved from Bidston to the campus of Liverpool University. Jones, "From Astronomy to Oceanography - a Brief History of Bidston Observatory." Also various Annual Reports and NOC's website, http://noc.ac.uk/, accessed 08 June 2010.

[8] Compare Mills, *The Fluid Envelope of Our Planet*, 264.

a study could analyse the debates between practitioners of different types of oceanography (e.g. Proudman, Deacon and Bullard) and analyse in more detail the tensions involved in establishing the institutional geography of contemporary oceanography in Britain. In addition, such a history could provide further insight into how oceanography came to be defined as it is, through disputes and discussions, and the role of military and state patronage of oceanographic science, providing more details on exactly why and how the state's role increased. This would further develop the history of British oceanography in the twentieth century and extend the existing US-focused debates regarding patronage in oceanography.

8.2.1 PRACTICES OF CALCULATION

The thesis has followed the attempts of a small, specialised Institute to make previously unpredictable water movements calculable, reducing it to (what was thought to be) predictable patterns on paper. TI attempted to make more predictable previously unpredictable aspects of the sea, be they surges or shallow water effects, by using new practices of calculation to produce specific chains of documents. The practices of calculations that TI used were at least to some extent new to tidal science. For example, from his work experience in London and elsewhere Doodson brought with him habits, skills and techniques, such as the use of new types of mechanical calculators and ways of organising calculations, which he used to increase the amount of calculations and thus residuals and constituents that could be managed when analysing tides. Later TI was able to use this work to secure patronage in different ways, so the introduction of new practices of calculation had tangible results in terms of institution building.

The thesis has shown in some detail how TI's workers used practices of calculation to attempt to make storm surges predictable, for example describing, in chapter five, how TI's workers put together data, re-inscribed it into tables and graphs and constructed surges. Through this use of particular practices of calculation they constituted surges as something calculable – something that could be made into numbers and calculated with. This work constituted storm surges, as defined by TI as residuals, into entities. Before this they had been unpredictable (according to George Howard Darwin) meteorological effects; now they had been constituted as storm surges that could be calculated. However, they were not yet deemed predictable – something that could be forecast through calculations. In 1928 TI's forecasts of surges were deemed to need further development to be deemed satisfactory, but their definition and construction of surges were accepted by others involved in the project (e.g. LCC, Hydro and Met Office). There is thus a difference between making something calculable and making it predictable, and more work was needed for the latter.

TI's use of particular statistical practices of calculation eventually made surges more predictable, but in a specific way: through a multiple regression correlation formula. When TI returned to the project of making surges predictable in the late 1930s they experimented with various methods. In the end they chose one particular way, statistical correlations, as it was felt this way would produce forecasting formulae fast. This need for speed was linked to a naval request for a way of forecasting meteorological effects on the German North Sea coast. This request led to TI using particular statistical practices, as opposed to continuing the development of the six-point method, so influenced the work and the forecasting formulae in a particular way. TI's researchers understood prediction of surges as the calculation of a correction to tidal predictions using a statistical formula. Only after sustained use of the formulae and intensive research in the 1950s did TI question this use of statistical practices, and then as much in response to criticism from others.

To produce statistical formulae TI constructed chains of forms. Throughout the thesis I have emphasised how the organisation of documents into such chains involved choices and work by TI's researchers. Both the organisation and the choices sometimes limited future choices and the potential shape of future chains of documents. It also influenced what 'supplements' – results with which they could achieve something – the researchers found, if any. The choices made in constructing chains of documents could also be disputed. For example, TI's choice to calculate winds in particular ways became disputed in the 1950s. Their choices were contingent on issues such as the labour involved in the calculations and criticisms from others involved in the establishment of the Advisory Committee. The initial choice of geostrophic winds for these calculations limited the choices of surges Corkan could use to calculate the coefficients in his formula for the wind effect, as in order to be 'well-conditioned' the surges he picked needed to meet the criteria for geostrophic wind (of course Corkan's emphasis on producing a 'well-conditioned' formula was another choice). This meant that the choice of using geostrophic winds, which appears primarily to have been based on ease of calculation, impacted on the results TI produced by influencing the results his formula produced. The documents TI produced appeared only with much work and many constrained choices which influenced the results they got in unpredictable ways.

To analyse chains of documents it is necessary to find documents that combine intelligibility with 'rawness'. The Bidston archive contains a reasonable amount of such material, at least from after 1929 when TI moved to the Observatory, in part perhaps because it has not yet been weeded by archivists.[9] However, locating and interpreting such material was still a major challenge during this project, especially given the mathematical nature of much of the material and the inevitably partial nature of archival traces. Completely 'raw' material can be difficult to make sense of, necessitating the use of documents such as Berncastle's memo to aid in this, but the difficulty in doing such analysis is offset by the potential insights it can provide. Through this thesis I hope to have showed that an attention to practices of calculation and the practices of making chains of documents within centres of calculation can give a detailed understanding of how a phenomenon such as storm surges was made more predictable. TI's researchers contingently produced statistical formulae through particular chains of documents that made surges predictable in particular ways, as a residual to be added to tidal predictions and found through correlation with pressure gradients. Such studies of the contents and organisation of documents help historians to understand in more detail how scientists have tried to order the world, using different practices of calculation.[10] In addition, by paying attention to the choices and work involved in creating chains of documents I have attempted to extend Bruno Latour's ideas about such chains, by emphasising that such chains are constituted through contingent practice which influences the shape of the chain produced. Much more work could be done in this area upon location of suitable archival material.

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[9] The archive was donated to Liverpool World Museum in 2004when TI's successor moved from Bidston to the University campus. Only minimal reorganisation, such as numbering of the archive boxes and creation of a basic finding aid, has taken place since then.

[10] A similar point is made in Campbell-Kelly *et al.*, *The History of Mathematical Tables*, 13., where the authors argue that "the internal structure of tables … tell us much about how people have selected, classified, and manipulated quantitative data at different times and places."

8.2.2 Further work: The 'diffusion' of statistics

Studying TI's work has analysed how statistics was used in the twentieth century on a particular phenomenon and how this changed over time. Change in practices of calculation were contingent on many events and processes, such as wars, the politics of patronage and earlier practices, e.g. what Doodson had learnt in London or TI's definition of surges as residual. Such events and processes influenced how TI used statistics, and thus how statistics travelled to the Institute's work, but this topic needs further work.

An area of potential for further work highlighted by this thesis is thus twentieth century history of statistics, in particular how this technology of calculation travelled from Karl Pearson's UCL into the natural sciences. Much of the history of statistics tends to finish with the establishment of mathematical statistics of Francis Galton, Karl Pearson or R A Fisher, claiming that their ideas and techniques were then stripped of their context and became expertise that was exported to almost 'everywhere'.[11] While there has been some work on the use of statistics in social sciences and select natural sciences, e.g. biology and physics, there does not seem to have been substantial work on the actual 'diffusion' of these ideas – the diffusion and 'stripping' of context (e.g. of eugenic concerns) has not been problematised.[12] However, as much work in history of science has shown, the 'diffusion' of scientific ideas is not straightforward.[13]

While some work has been done on the impact of Pearson's work abroad, little seems to have been done on the travel of statistics within the UK.[14] Doodson's usage of statistical work is one example of how Pearson's 'disciples' took up and used his ideas in other fields. LJ Comrie would be another example, who like Doodson emphasised the

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[11] Gigerenzer *et al.*, *The Empire of Chance*, 69, 272-274; Desrosières, *The Politics of Large Numbers*, 104. Francisco Louçã, "Emancipation through Interaction – How Eugenics and Statistics Converged and Diverged," *Journal of the History of Biology* 42, no. 4 (2009).

[12] Stigler, *The History of Statistics*; MacKenzie, *Statistics in Britain*; Porter, *The Rise of Statistical Thinking, 1820-1900*; Desrosières, *The Politics of Large Numbers*; Gigerenzer *et al.*, *The Empire of Chance*; Hacking, *The Taming of Chance*; Magnello, "Karl Pearson and the Establishment of Mathematical Statistics."; Magnello, "The Non-Correlation of Biometrics and Eugenics: Rival Forms of Laboratory Work in Karl Pearson's Career at University College London, Parts 1 and 2."

[13] For an example from mathematics and physics, see Warwick, *Masters of Theory*.

[14] See e.g. E. Seneta, "Karl Pearson in Russian Contexts," *International Statistical Review* 77, no. 1 (2009); C. G. Borroni, "Understanding Karl Pearson's Influence on Italian Statistics in the Early 20th Century," *International Statistical Review* 77, no. 1 (2009); D. R. Bellhouse, "Karl Pearson's Influence in the United States," *International Statistical Review* 77, no. 1 (2009).

organisation of large scale computation and the use of calculation machines.[15] However, this emphasis may not be the same for Pearson's other students. While some work has been done to trace the initial destination of some of his students – the way Doodson went to UCL, picked up the mathematics and then took it elsewhere was a common pattern – further work along these lines could be a way at looking at the diffusion and transformation of mathematical ideas.[16] What did Pearson's disciples do, especially those who did not stay in mathematical statistics? How did they use the techniques they had learnt, if at all? What fields did they go into?

As many social scientists are taught at least some statistics, this is an area of mathematical practice that ought to be more accessible to those who wish to study practices of mathematics than, for example, the construction of proofs. This type of mathematics is also obviously material through the production of tables and documents and use of machinery, such as calculation machines. At the same time it is an example of 'esoteric' expert knowledge that seemingly spreads without effort and has become much used. As such it would be a suitable project to study how a particular set of mathematical ideas have been universalised, to the extent that they have.

8.3 Alternative approaches to the thesis

This thesis has focused very closely at storm surge science at TI and how they made surges predictable in a particular way. While this has allowed attention to details of TI's practices of calculation and their complex patronage, it has limited my view to how TI and to some extent their closest allies understood storm surges. In order to understand how the British have interacted with storm surges more generally a wider view would be necessary as TI's work was one, but only one, aspect of this. Another aspect, with a much longer history than TI's, is the history of engineered coastal defences and research into such defences. The Waverley Committee treated this as a complementary but separate aspect of storm surge science.[17] A different committee was set up to manage research into this area, which the Advisory Committee on Oceanographical and Meteorological Research was not in close contact with. However, for a fuller

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[15] Croarken, *Early Scientific Computing in Britain*.
[16] MacKenzie, *Statistics in Britain*, ch 5.
[17] *Report of the Departmental Committee on Coastal Flooding [Waverley Committee]*, esp. para 4.

understanding of the interactions between the British and storm surges, and how the first tried to govern the latter, historical work on the science of flood defence and the links between this science and policy is necessary. Work in this area would be particularly interesting as there has been a marked shift away from 'hard' defences following the 1953 event towards the current policy of 'managed retreat'.

I would also have liked to look further at reception issues, for example what other scientists outside TI's 'set' thought of their work. If time had allowed I would have explored this by attempting to locate further sources in this area. Two particular groups of interest are BAAS and the Royal Society, which published various work by TI's storm surge researchers.[18] Another aspect of reception is how TI's formulae were taken up and adapted by forecasters, and how these warnings were in turn taken up by those who received them. TI's work produced forecasting formulae, not forecasts. Their formulae needed further work to result in forecasts. Due to word count constraints it has not been possible to discuss how TI's formulae were taken up by the storm surge warning system. They adapted and developed them during the winter of 1953-4, before the Advisory Committee had been formally established, as well as later. While the warning system used TI's formulae, these formulae were transformed by that use, thus providing a case study of travelling formulae. In addition, the warnings produced by this system did not travel unhindered or unchanged, and how they were communicated and used by experts, such as the river boards the warnings was sent to, and non-experts, such as politicians or residents, would be important issues for a broader history of storm surge forecasting.[19] It would also have been very useful to know more about the potential use of TI's formulae during the Second World War. If more information could

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[18] For example, Robert Henry Corkan, "The Levels in the North Sea Associated with the Storm Disturbance of 8 January 1949," *Philosophical Transactions of the Royal Society of London. Series A* 242, no. 853 (1950); Arthur Thomas Doodson, "Tides and Storm Surges in a Long Uniform Gulf," *Proceedings of the Royal Society of London. Series A* 237, no. 1210 (1956); J. R. Rossiter, "The North Sea Storm Surge of 31 January and 1 February 1953," *Philosophical Transactions of the Royal Society of London. Series A* 246, no. 915 (1954).

[19] Compare Hooke and Pielke Jr., "Short-Term Weather Prediction: An Orchestra in Need of a Conductor."; Pielke Jr., Sarewitz, and Byerly Jr., "Decision Making and the Future of Nature: Understanding and Using Predictions."

be found on whether TI's formulae were used this could put a different light on the extent of naval patronage of storm surge science.[20]

Another issue that deserves further exploration is how TI judged formulae as successful or not. For example, how was a residual curve judged to be close enough or too far from the observation curve to be a 'good' prediction? The researchers at TI presumably discussed this face to face as such discussions have left few archival traces. While figure 6.1 is an exception of sorts, the graph in this picture was used by TI as much as evidence of external surges as of a judgement regarding success – the finding of new types of residuals was as much a result for TI as the sufficient closeness of the final formula was. That discussions regarding judgements of the distance between curves, or success versus failure, were made in person rather than on paper also points to the importance of conversation in mathematical practice and research, which is another potential further topic of investigation.

8.4 FURTHER WORK: CHANGING CHAINS OF DOCUMENTS

In addition to the suggestions regarding further work discussed above, an obvious topic for further work coming out of this thesis would be to look at developments in the 1960s and beyond regarding storm surge forecasting. In the 1960s the tripartite system involved in the warning system, consisting of TI, Hydro and the Met Office, suffered severe strains. An argument broke out between TI and the other two regarding the warning formulae. Who should develop them, the scientists at the Institute or the forecasters based at the Met Office? How should they be developed, for scientific quality or practical use? What was a 'trustworthy' prediction in the 1960s and how could it be produced? The argument was intensely acrimonious and eventually TI lost control over the development of statistical storm surge forecasting. In the 1970s they regained control by developing digital computer models to forecast surges (i.e. introducing new practices of calculation) and this computer-based warning system was put into

[20] In this case the document trail went cold immediately, as the only UKHO reference to the work that could be found in the BA archive could not be located in UKHO's archive and was presumed lost by the staff there.

operation in 1978.[21] It has since continued to be developed, often by researchers at TI's successors, to become the system that is running today.

I would like to follow up on this episode, which would involve further sets of theoretical literature than those used for this thesis. Historians of science have focused on disputes like this as fruitful sources for understanding how science functions. Controversies are interesting to historians and STS scholars as they make visible assumptions and relationships that are normally invisible. In this case this includes how findings could or could not be replicated by different actors and the status of the different actors involved with the warning system, such as the role of 'the scientists' and 'the forecasters'. The controversy raised issues of how storm surge science should be conducted, as well as what it was. Further developing this episode would also follow up on issues raised in this thesis, such as the role of patronage in storm surge science, and would also add to the existing literature on scientific controversies, often focused on disputes regarding laboratory experiments, as this dispute focused on the role of field measurements and calculations. This controversy brought to the fore practices of calculation instead of laboratory practices. Studying it would further develop my argument that storm surge scientists constructed particular chains of documents making consequential and contingent choices, by analysing how their chains changed.

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[21] The narrative in this paragraph is based on material in the Bidston Archive, especially correspondence in Box 120 and 137.

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APPENDIX

TI's financial ledger provides an overview of the organisation's patrons in the 1920s and how these changed. The table below illustrate this development, while the figure displays it graphically.[1] Initially TI relied heavily on donations from the Booths and other shipping men, with grants from DSIR also playing a prominent role. When the Booth's funding ran out in 1923, other shipping men from LSOA took over. In 1923-24 DSIR's grants stopped, donations increased further and commercial income started coming in. This was also the time TI bought a tidal predictor machine for £1541, with funds donated mainly from different shipping men and companies (£1200) but also BAAS (£300). The predictor expenses and donations are not included in the table and graph below. In the years following this purchase and the 1923 agreement with Hydro to supply them with tidal predictions the income from commercial work continued to increase, overtaking donations in 1927.[2]

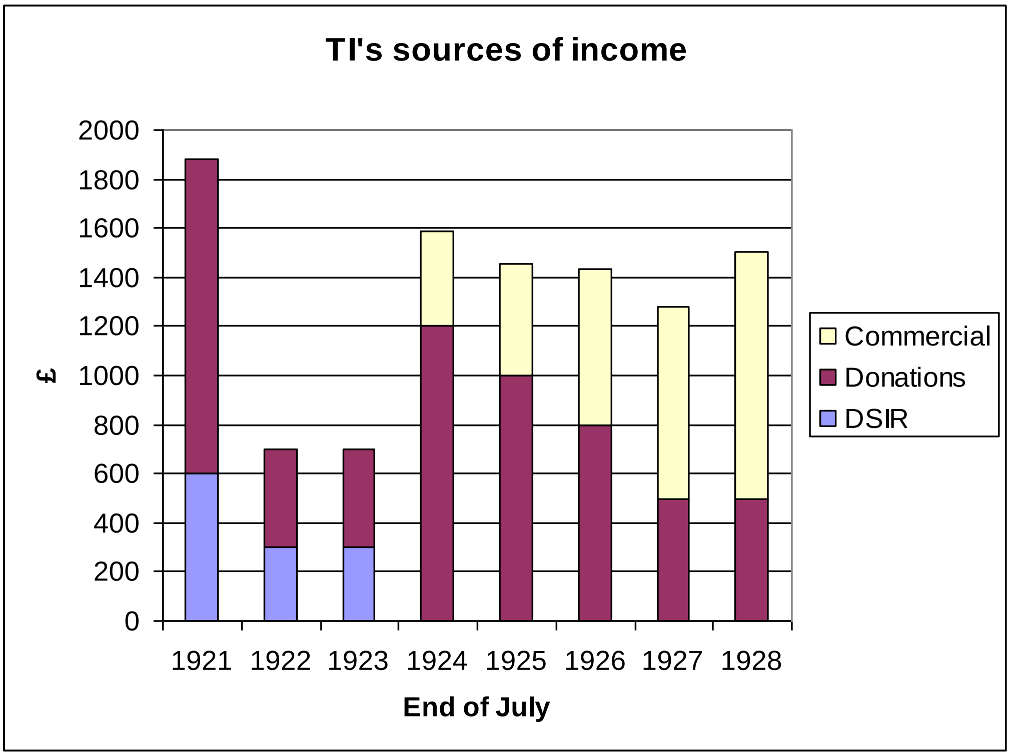
End of July	DSIR	Moss Steamship Co	Booth brothers	LSOA	Commercial work
1921	600	250	633		
1922	300		400		
1923	300		400		
1924				1200	390
1925				1000	453
1926				800	632
1927				500	778
1928				500	1003

Appendix: Table of TI's income 1921-28 from various sources, in £[3]

[1] The ledger begins in 1921, not when TI was established in 1919. While there are some mistakes and discrepancies in it, there are no signs of bias or systematic errors. For example, in 1924 the individual donations add up to £1200, while the summary gives total donations as £1250. The latter figure has been corrected.

[2] Tidal Institute Ledger, S2147, LUA

[3] Tidal Institute Ledger, S2147, LUA



Appendix: Graph of TI's income 1921-28 from various sources, in £[4]

[4] Tidal Institute Ledger, S2147, LUA