Normal and Extraordinary History? Thomas Kuhn and Historiography.

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Abstract: *The works of Thomas Kuhn have been hugely influential in a wide variety of disciplines over the last 50 years or so; yet the discipline of history is missing from the list. With a general consensus emerging that the 'postmodernist' era in historiography is over, and that we now have the task of developing a 'post-postmodernist' theory of history, it seems to me that the work of Kuhn could be extremely useful in this task. In this article I examine the ideas of 'normal and extraordinary science' and ask whether there is an equivalent 'normal and extraordinary history' within our own discipline. Is it the case that there are Kuhnian cycles of relative calm punctuated by periods of crisis and confusion? Are there such things as 'extraordinary' works of history? Finally, I conclude by briefly examining the past and the future of historical theory.*

Introduction

The writings of Thomas Kuhn have been extremely influential over the past 50 years or so in a multitude of disciplines other than science, particularly within the social sciences. His keynote work, *The Structure of Scientific Revolutions,* is regularly cited as one of the most influential books of the twentieth century.[1] Yet the furore that the publication of *Structure* caused in both its own and attendant disciplines seems to have almost entirely bypassed the historical profession.[2] Why is this? Whereas it was once probably fair to say historians were disinclined to enter into discussions over historical theory and methodology, such attitudes are less prevalent today as more and more historians have become increasingly literate with historical theory. A more plausible argument might be that historians and historical theorists have had

their hands full for the past half century trying to sort the wheat from the chaff with regards to various postmodernist arguments. If this is indeed the case, then the time is ripe for an assessment of Kuhn's work from the point of view of the historian There is a growing consensus that the era of postmodernism in history – and the very assertion that there *has* been an era of postmodernism in history is contentious in itself – has now come to end. As George Iggers has argued, we are now entering the 'post-postmodernist' phase of historical theory.[3] Much like Kuhn himself in the latter stage of his thinking, historical theorists have found themselves caught between two poles. As a result of some of the critiques of classical empiricism/ positivism, they are now inclined to be more constructivist than they once were.[4] That said, they do not feel obliged to embrace the full-blown relativist conclusions that postmodernist theory draws from said critiques – namely that the practice of history is nigh-on impossible and that history should therefore be reclassified as an entirely literary act.[5] They still hold to some form of realism in that there is, or was, a 'real' past-in-itself that has to be taken into account when writing history.[6]

It does not lie within the confines of this article to examine or enumerate those postmodern ideas that have not stood the test of time. Rather, my thoughts are grounded in the understanding that there is a growing consensus within the profession that the ideas of Michel Foucault, Hayden White and their followers are no longer always appropriate as theoretical cues. The road towards a 'post-postmodern' theory will be a long and arduous one, but it will be argued in this article that Thomas Kuhn and the ideas inspired by his work

have an important role to play in such a journey. Some of those ideas outlined in this piece concerns the notions of 'normal and revolutionary science' and their potential applicability to the discipline of history.[7]

Of course, not all historians practise the same type of history; the beliefs, values and techniques of an empiricist history will differ wildly from those of a postmodernist persuasion. Having said that, I would argue that the homogenisation of certain aspects of the historical profession can be justified. Martin L. Davies, for example, argues that 'the "culture war" between postmodernist academic history and traditional academic history is far less important than the fact that both exist, even flourish, *together,* in the academic environment.'[8] Similarly, Antoon De Baets states that 'scholarship is the decisive condition of the [historical] profession...Although operating under widely divergent conditions across countries, the organisational aspects are quite universal.'[9] Whatever their ontological persuasions, by and large historians, being part of what Kuhn called the 'disciplinary matrix', all undergo the same educational and professional initiations.[10] Reasons of space preclude a full description of the disciplinary matrix here; however, Kuhn's ideas centre upon the idea that all those within the discipline of science receive the same style of training, culminating in the granting of their membership into the 'club of scientists' via the awarding of the PhD. I feel his arguments correlate very closely with the historian's training. All academic historians in the West are inculcated with very similar values regarding what a piece of historical work should look like, particularly in terms of its form.[11] Furthermore, as Hayden White has illuminated, form directly impacts upon

content and therefore the content of the output of any institutionally trained scholar is universally constrained within certain accepted bounds.[12]

Normal History?

In trying to examine whether there is a 'normal history' which plays a role in historiography akin to Kuhn's 'normal science', we run into the problem that it is impossible to 'boilerplate' Kuhn's model of science onto the process of historical writing.[13] During my own research I have come to the conclusion that, despite its wholesale approbation by the social science community - for historians the idea of the 'paradigm' is a red herring.[14] However, there does appear to be a role in any Kuhn-based theory of history for what we might call 'normal' and 'extraordinary' history. The figure of the historian provides a good starting point. Kuhn argued that the normal scientist harbours no desire to split the atom, discover the genetic code or become the next Lavoisier but is quite happy to work in a highly specialised area; this approach Kuhn referred to as 'puzzle solving'.[15] Similarly, it could be argued that there is a similar lack of hubris to be found in most historians. Most of us instinctively know that we won't be the next A.J.P. Taylor, Edward Gibbon or E.P. Thompson.[16] Of all the papers that historians collectively publish, the majority of them are never cited, and will probably be read by only a handful of fellow professionals: this paper being no exception.[17] It may well be asked that if the historian's ambition is not to write a work that parallels Braudel's *The Mediterranean* or Thompsons' *The Making of the English Working-Class* in terms of its innovativeness and scope, then why become a historian in the first place? Kuhn provides us with the answer: 'Few people who are not actually

practitioners of a mature science realise...how fascinating such work [normal science] can prove in the execution.'[18] The historian finds him/ herself in a similar situation. Take for example a random thesis title plucked from the shelves of the University of York library - *Quaker and Political Influences in the Writing of Charles Brockden Brown*. It is easy to dismiss such a work as antiquarianism or mere minutiae – yet the fact is that this work, in its own small way, fills a gap in the knowledge of the area in question, as well as making a limited contribution to its historiography.[19] As Kuhn puts it, the 'areas investigated by normal science are, of course, miniscule; the enterprise now under discussion has drastically restricted vision. But those restrictions…turn out to be essential to the success of normal science.'[20]

Kuhn is at pains to dismiss the idea that normal science is little more than dull, plodding hack work. It is harder to perhaps make such a case for history. Any historian who has laboured in the archives for any length of time can testify as to the tedium of such an enterprise. However, as spirit-crushing as it might be at times, every serious work of history needs a solid amount of Rankean spadework to back it up.[21] In the words of Frederick Maitland, we must leave no thicket unbeaten and no stone unturned: 'out of the thicket may fly a bird worth powder and shot. Under the stone may lurk a toad with a jewel in its head.'[22] Indeed, amongst the 'normal historians' we may classify many of the greats in the field – such as Theodor Mommsen, for instance, whose corpus of Roman inscriptions, while not impressing E.H. Carr, 'laid the solid foundations of Latin epigraphy.'[23] Perhaps another term for 'normal history' might be 'excavating.' In a review of the works of A.G. Little, Dr Moorman

included him amongst the 'excavators' who make possible the work of others.[24] Developing this theme further and utilising Emmanuel Le Roy Ladurie's famous aphorism, historians can be split broadly into two categories - 'truffle hunters' and 'parachutists.' [25] The former:

> grub about with their noses in the dirt, searching for some minute and precious fact; the second float down from the clouds, surveying the whole panorama of the countryside, but from too great a height to see anything clearly.'[26]

It would seem that the normal historian is by and large a truffle hunter, as only a select few are able to provide such a panoramic view.

The idea of 'truffle hunting' also ties in with Kuhn's notion of fact gathering, or 'significant determination of fact'. In science, this consists of 'the experiments and observations described in the technical journals through which scientists inform their professional colleagues of the results of their continuing research.'[27] On what aspects of nature do scientists normally report? Among other things, 'that class of facts that the paradigm has shown to be particularly revealing in the nature of things.'[28] This sounds like E.H. Carr's much maligned 'club of historical facts'.[29] Carr sketched out a process whereby certain significant facts about the past are considered inherently important enough for further recording and investigation while others are not.[30] One of the examples Carr gave was that, of the millions of people who have crossed the Rubicon it is only Julius Caesars' journey that interests us. However, as

numerous scholars have pointed out, with the rise of social history, social historians would be very interested in the millions who crossed the Rubicon, and would have liked more to be recorded about them.[31] And this ties in with Kuhn's ideas about the function of the paradigm. With a change of paradigm what counts as a 'significant fact' also changes.[32] Take for example, the historiography of the origins of the Second World War. While the Nuremberg/ 'Guilty Men' thesis[33] held sway over the field – that is, that Hitler had intended war all along and was aided by 'cowardly' appeasers – 'significant facts' in the field were confined to things like Hitler's ideology and intentions and also those of the appeasers. But following the publication of A.J.P. Taylor's *Origins of the Second World War* and other revisionist works, the kind of 'facts' that historians were looking for changed.[34] Historians began to look at British and French foreign policy throughout the 1920's, as well as re-examining ideas about appeasement. The change of emphasis meant that more research needed to be done in different areas.

The second activity Kuhn posits as one of the key activities of normal science is that of testing predictions that the paradigm makes. Naturally there is no way for us to do this in history with events that have already taken place in 'the-past-in-itself'.[35] However, it could be argued that a similar activity does take place in assessing a historical theory for internal consistency. Granted, historical theories are not 'tested' as in science; however, they are subjected to critical scrutiny by fellow historians. Following the publication of the Taylor thesis, numerous articles were published pointing out the various contradictions that Taylor's interpretation entailed.[36] In order to point out such

contradictions, it may well be necessary to uncover new sources, or apply new techniques to existing ones; just as in science it is sometimes necessary to construct new instrumentation to test a theory.[37]

As the historian of science George Sarton notes:

> the truths of today will perhaps be considered tomorrow, if not as complete mistakes, at least as very incomplete truths; and who knows whether the errors of yesterday will not be the approximate truths of tomorrow?[38]

So we may never achieve what Acton called 'ultimate history'. The historiography of the Cold War provides ample evidence of this. While historians were unable to access any Sino-Soviet documents from the period, any interpretation or theory of the conflict would have been extremely provisional at best. Now however, with documents from that era coming to light, it is possible for historians to examine such theories with much more rigor than they previously had been able to.[39] Following E.H. Carr's thoughts on the provisionality of historical writing, it is self-evident to say that we are closer to the 'truth' about the Cold War than we were in 1990; and we will be closer in 2030 than we are now.[40]

The third key activity of normal science is 'empirical work undertaken to articulate the paradigm theory, resolving some of its residual ambiguities and permitting the solution of problems to which it had previously drawn

attention.'[41] This is normal science as 'puzzle solving', the 'mopping up' operations that are left over once the new theoretical framework has been adopted. This is the 'minutiae' that I mentioned earlier. Kuhn's remark about how fascinating the so called 'hack work' of mopping up operations can actually be is equally applicable in many cases to the historian. The extent of Quaker influence on the writing of Charles Brockden-Brown was a puzzle that needed to be solved; there was a gap in the literature and the author filled it accordingly. To an extent, it could be argued that all historical research is puzzle solving. The old objectivist fallacy that the historian approached the archives with a blank mind has been long dispelled.[42] More often than not, historical research begins with a question in the author's mind. Indeed, the main concern of this article could be formulated in such terms – 'I wonder what would happen if Kuhn's ideas on science were applied to history?' Like Kuhnian normal science, however, the analogy of puzzle solving falls down when it comes to the solution. By definition, a puzzle must have an assured solution, whereas the same cannot be said for the contemporary practice of science. There is no such thing as a theory which is able to account for every anomaly.[43] Just as all historical writing is, as discussed above, at best provisional, the idea of an Actonian Ultimate history – an untouchable corpus of finished works covering every conceivable historical subject – has ceased to be the ideal of historical profession. Just like the scientist, the ultimate criterion for the acceptance of an historical work is the judgement of the historical community. David Abraham's *The Collapse of the Weimar Republic* or David Irving's *Hitler's War* and similar works have been deemed to be

unacceptable by the current standards of historical methodology and they have been largely abandoned by the historical profession.[44]

On another related tack, in *Structure* Kuhn remarked that 'Normal science does not aim at novelties of fact or theory, and when successful, finds none.'[45] Rather, bringing a normal research problem to a conclusion requires 'the solution of all sorts of complex, instrumental, conceptual and mathematical puzzles.'[46]This can be likenened to historians addressing gaps in their field bought about by a shift in conceptual emphasis. For instance, in his article 'George Grote and the Study of Greek History', Arnaldo Momigliano takes his fellow historians to task on some of the gaps in Greek historiography:

> We have no up-to-date history of archaic Ionia, of the Athenian empire, of Magna Grecia in the fifth and fourth centuries BC, of the Seleucid empire, of the province of Achea under the Roman Empire. The studies of Greek private law are deplorably behind and those of public law can be improved. There is no history of Greek agriculture, or of Greek coinage; and that of Greek trade is out of date. Finally, a history of Greek political theories after Aristotle and of historiography after Thucydides is still to be written.[47]

Similarly, in his bibliographical review in *The Revolution of 1688 in England*, J.R. Jones provides a similar list of subjects that needed attention at that time:

> Of all the subjects that need research perhaps the most important is to establish the connections between business and politics, and in particular to examine mercantilist policies, principles and interests. The tensions within the East India Company, and the careers of Sir Josiah Child & Sir John Friend, would repay attention. Secondly, the court of Charles I and James I would seem to provide ample grounds for analysis. Some of its leading figures – Rochester, Godolphin, Melfort and Halifax – need new biographies. The politics of the army are an important subject upon which we know little. There is room for a systematic study of pamphlet literature in the years before 1688. At the local level we need to know more about the function of the Commission of Peace after James II's wholesale purges, and the effect of royal policies on the corporations is a subject of first rate importance.[48]

Jones's book was published in 1972, but few if any of these issues have been addressed since the author issued his plea. Several histories of the East India Company have since been published. However, none seem to have addressed the issues that Jones raised in any meaningful way. The First Lord Godolphin was the subject of a biography in the first half of the 1990's and Lord Rochester received biographies in 2004 and 2005. Sir Josiah Child, Sir John Friend, Melfort and the first Earl of Halifax still await full length treatments.[49] On the face of it, it would seem that these mopping up operations are not a pressing concern within the field. Nevertheless, the idea

of 'puzzle solving' within historical writing remains strong, particularly at a postgraduate level. In order to be taken on, a thesis candidate must chose a subject which a potential supervisor feels is worthy of study. 'Mopping up' type topics are often entirely suitable. Account must also be taken of the more individualistic nature of historical study compared with the more community orientated approach of science – one only has to read Watson's *The Double Helix* to appreciate this.[50] Having addressed the idea of normal science and puzzle solving, let us now turn to what Kuhn cites as the mainspring of paradigmatic change, anomalies and crisis.

Anomalies and Crisis

There is no historical equivalent to 'the great scientific experiment' during which the cumulative effect of multiple anomalies leads to a major theoretical shift. Rather than resulting from a period of accumulating anomalies, if anything what we will now term 'extraordinary' works of history often seem to explode from nowhere.[51] Take for example, A.J.P. Taylor's *Origins of the Second World War*. It cannot be said that there was a build-up of anomalies or a period of crisis leading up to its publication. On the contrary, there was a happy consensus over the origins of the war until Taylor's book upset the applecart. Maurice Cowling's *Impact of Labour* performed a similar service in its field, as did Fritz Fischer's *Griff Nach Der Weltmacht*.[52] Perhaps 'catalytic' might be a better word for such revolutionary works.[53] The word takes its etymology from the Greek *katalyein,* to dissolve. The words of Taylor, Cowling and Fischer dissolved the hard and fast conceptions of their particular fields, dissipating much of the stagnancy that had been a feature of these areas of

historiography. But more than this, these works also acted as metaphorical hand grenades, blasting away an accumulation of detritus and rubble that had accumulated over the years.

Conversely, there have been episodes where a work of extraordinary history has arisen from a crisis induced by the accumulation of anomalies as described by Kuhn. A celebrated example is the series of events that led up to the publication of Laurence Stone's *The Causes of the English Revolution.*[54] In 1941 R.H. Tawney published an article on the rise of the gentry prior to the English Revolution which seven years later Stone expanded upon.[55] Three years after this Hugh Trevor-Roper subjected Stone's article to devastating treatment in the same journal, and subsequently unleashed a similar assault on Tawney's original paper.[56] After nineteen years of further debate and exchanges in the pages of various journals, Stone published his masterwork, a natural consequence and result of the exchanges that took place over the preceding years.[57] Naturally this did not bring an end to the controversy – Richard Evans later remarked that the historiography of the English Revolution seemed to constitute a 'perennial all out fight'[58] – the debate did settle down, and was a good deal less fractious than it had been.[59] Another example of an accepted historiographical thesis being bought down by the sheer weight of inconsistencies might be found in Francois Furet's *Interpreting the French Revolution*. In this book Furet mounted a full frontal attack on Albert Soubol's Marxist interpretation of the Revolution. Furet was not the first to take issue with Soubol: historians had been demonstrating inconsistencies in his argument for quite some time. Nevertheless it was Furet's systematic

rebuttal of the central tenets that ultimately bought the whole edifice tumbling down.

Some historians would argue that far from constituting a crisis, the state of affairs that characterised the historiography of both the English Civil War and the French Revolution were fertile periods that resulted in extremely good history. In fact, there is probably some veracity to the suggestion that most practising historians would favour the Popperian view that historical theses and accounts should be debated and tested constantly, and be somewhat opposed to the idea that large-scale interpretations should be left alone so that historians can concentrate upon 'puzzle solving'. Nevertheless, the practice of history does not conform entirely to the Popperian model, just as it does not conform entirely to the Kuhnian cycles of normal/revolutionary science. Any potential Kuhnian schema of historiography will need to include two catalysts for historical change: the 'weight of numbers' effect, and the 'explosive' work of extraordinary history, each having the potential to bring down an accepted historical thesis.[60] In the next section, I will work towards defining what constitutes an 'extraordinary' work of history.

Extraordinary History

First, a distinction must be made between an extraordinary work of history and a 'classic' or canonical work of history. A good recent example of the latter would be the final volume of Richard Evans's trilogy on the Third Reich.[61] This erudite work is the result of an extraordinary amount of research. Clearly, it will be the standard work on the subject for many years to

come.[62] And yet it would be tendentious to describe this as an 'extraordinary' work of history.[63] As mentioned earlier, the idea of a catalyst is key to the concept of extraordinary history. Thus, a work of extraordinary history must have at its core a thesis that is so controversial that it polarises the profession, before gradually gaining enough followers to ensure that what was once shocking then becomes an accepted part of the tradition. As with choosing a paradigm, one must either be for it or against it.

One might argue that if this is indeed the case, it is surely a historical non-sequitur that works such as those of Robert Bauval, Marcus Garvey and David Irving would qualify as 'extraordinary history' whereas the efforts of Evans and Kershaw, for example, would not. To redress this anomaly, a caveat that such works must be deemed by the profession to be 'legitimate' contenders for the title 'extraordinary,' must be added.[64] This ties in with Larry Laudan's definition of scientific revolutions:

> a scientific revolution occurs when a research tradition, hitherto unknown to, or ignored by, other scientists in a given field, reaches a point of development where scientists in the field feel obliged to consider it seriously as a contender for themselves or their colleagues.[65]

Most historians disagreed with both the Taylor thesis and the Fischer thesis upon their publication - indeed, the controversy generated over the so-called *Sonderweg* interpretation of history was such that by the late 1980s it had

exploded into the public consciousness as the *Historikerstreit* - but most historians also recognised that there was a case to answer.[66] In *Structure*, Kuhn remarked that 'more and more attention is devoted to [anomalies] by more and more of the field's most eminent men.'[67] Moreover, 'the field will no longer look quite the same as it had earlier.'[68] Much the same can be said for fields of historiography after the impact of a work of extraordinary history. Such works essentially re-orientate the entire discipline around them; so that after publication, no-one working in their field can do anything without making at least passing reference to them. It might be said that after a work of extraordinary history, for historians working in that field there can be 'no going back'. With regards to *Griff Nach Der Weltmacht*, Esmonde Robertson stated that Fischer :

> 'has bought to bear on his subject a conceptual and methodological approach entirely new to German historiography… The majority of German historians now admit there can be no going back to the criteria of Fritz Fischer's predecessors.'[69]

This echoes my earlier point that the discipline re-orients itself around these works. We might call this the 'no going back' thesis, analogous to the Kuhnian idea of 'scientists' working in a different world.'[70] In order to establish whether a work qualifies as extraordinary or not, we might ask a counterfactual question: 'If the work in question had not have been published, would the historiography in question have taken a different course?' Perhaps the best

words to describe a work of extraordinary history come from Namier: after such a work has been published, 'others should not be able to practise within its sphere in the terms of the preceding era.'[71]

A Sociology of Historical Writing?

On more than one occasion in *Structure* and subsequent pieces, Kuhn notes that in some senses his view of science is 'irreducibly sociological.'[72] However, he did not really develop the idea, which has led some scholars to argue that 'the sociological approach to methodological questions does not receive much support from Kuhn's views and certainly nothing like as much support as sociologists are wont to claim.'[73] A number of sociologists though – in particular the so-called 'Edinburgh school' – began to examine the idea of 'external' influences on the development of science.[74] Such an idea is aptly applied to historiography.

As noted above, it is generally accepted that historians bring various political and social philosophies to their writings, try as they might to suppress them; 'before you study the history study the historian.'[75] Nevertheless, there has been no examination of the external constraints on historiography. Yet there are many instances where the historiography of an area has been seriously impacted upon by the wider socio-political context. The *Historikerstreit* is a case in point. Not only is it an example of a crisis in historical writing leading to what we might term a period of extraordinary history, the *Historikerstreit* also provides an excellent case study for contemplating the influence external factors can have on historical explanation. If we choose to utilise Foucauldian

theory about the conditions of truth, it can be argued that in the twenty years succeeding the Second World War the 'Hitler was a bad man' thesis was the only interpretation permissible given the political conditions of the time. Practically everyone benefited from the thesis: Churchill and Namier could display their credentials of having warned of the dangers of Hitler; appeasers could claim that 'appeasement had been a wise, and would have been a successful policy if it had not been for the unpredictable fact that Germany had been in the grip of a madman.'[76] Even the Germans could profit from sticking to this line – 'with the Nazis either dead or hiding, they could claim to be blameless and take on a new, respectable role in the democratic alliance.'[77] Furthermore, laying the guilt at Hitler's door 'every other German could claim innocence; and the Germans, previously the most strenuous opponents of war guilt, now became its firmest advocates.'[78]

A sociological analysis of various historiographical episodes could shed much light on the motivations impacting the relevant historians - if also a considerable amount of heat – and demonstrate the need for a reconstruction of these episodes. Several authors, while not consciously adopting a sociology of knowledge framework – such as that utilised by the 'Edinburgh school' for example - have already provided historical surveys which take into account changing political circumstances.[79] One obvious example is Pieter Geyl's *Napoleon: For and Against*. More recently, Annika Mombauer's survey of the historiography of the origins of the First World War traces the changing interpretations of the war origins against the background of the German political scene.

Conclusion

In addition to his thoughts on normal and extraordinary science, Kuhn's work provides many other insights worthy of contemplation by historians interested in the methodology of their subject.[80] As stated earlier, there is a consensus that 'not only the period of modernist but also that of postmodernist reflection is over, and that the world that has changed fundamentally since 1990 calls for a "new theory of history"'[81] Thus, the onus is on historians and theoreticians to examine potential new avenues and theories of history – particularly if we wish to avoid having them foisted upon us from without again.[82] As John Zammito notes, in writing *Structure*, 'Kuhn charged that we [philosophers] were somehow misguided in our sense of science, and he proposed to correct this image through the invocation of history.'[83] In formulating their theories, Kuhn argued philosophers of science should look to the actual record of science as it progressed, as opposed to formulating *a priori* theories of how the scientific process *should* work. Indeed, when contemplating recent historiography it could be argued that the logical positivist approach accounts for the failure of the postmodernist project to make much impact on the practice of history. Despite many prescriptions as to how the new postmodern history should actually be written, few have taken on the task of writing such works – and when said works have been written, they are severely watered down compared to the grand design initially put forth.[84] As Christopher Lasch puts it, postmodernist historians demonstrated 'their willingness to observe the prevailing conventions and to write books that

were just as narrow, tedious, and predictable as the books written by their ideological opponents.'[85]

It must be said that philosophers of science were/are resistant to Kuhn's work. A key question to be answered then, is why should historians adopt ideas that those for whom they were developed did not? Limitations of space preclude a full response, but the impact and reception of Kuhn's ideas was more nuanced than traditional accounts have allowed. Granted, they did engender a fierce amount of criticism, but they also set the agenda for studies in the philosophy of science for twenty years or so. Furthermore, while *philosophers* of science were none too happy with Kuhn's work, practicing scientists themselves were very receptive to his ideas.[86] One of the fundamental problems that the discipline of history has suffered from throughout its existence has been the gulf between theoreticians and practitioners. Kuhn's work tried to bridge such a gap, and one can't help but feel that a Kuhn-type figure is needed in our own discipline.

How then, are we to use Kuhn's ideas to construct a potential post-post modern theory of history? In deciding upon the direction of its future, the discipline of history might be wise to look to its past. In Hobbes' famous dictum, 'No man can have in his mind a conception of the future, for the future is not yet. But of our conceptions of the past, we make a future.' This is not to imply a return to the old empiricist model of historiography that would receive the posthumous approval of historians such as Arthur Marwick and Geoffrey Elton. Nevertheless, Elton's premise that emphasis should be placed on the

actual *practice* of history seems well founded. As opposed to speculative theorising, we need concrete studies of what historians actually do. For instance, in England the Research Assessment Exercise has had a huge impact on the production of scholarly works, yet studies of its effect have not yet penetrated any of the primers on historical methodology. We need studies of how historical works are productions, and indeed how they are received within the historical community in order to better understand the processes of historiographical change.[87] If we are better able to understand what motivates historiographical change, this in turn might enable us to examine the way in which we write history. In studying Kuhn over the past few years, I have become more and more convinced we need something like a sociology of knowledge production in history to achieve this aim. Indeed, there are signs that such a movement is underway.[88] One can only hope that this article provides a small contribution to the process.

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[1] See for example, the TLS list of the most 100 influential books since the war. Thomas Kuhn, *Structure of Scientific Revolutions, 3rd Edition,* University of Chicago Press, London, 1993

[2] See David Wootton, *Bad Medicine*, Oxford University Press, Oxford 2005; Oliver Daddow, *Britain and Europe since 1945*, Manchester University Press, Manchester, 2004; David Hollinger's "T.S. Kuhn's Theory of Science and its Implications for History", *The American Historical Review*, Vol. 78, No. 2 (Apr., 1973), pp. 370-93. Kuhn addressed the issue himself in a famous essay, arguing for a rapprochement between historians and historians of science. However, none seems to be forthcoming. See Kuhn, "The Relations Between History and History of Science", reprinted in *The Essential Tension*, University of Chicago Press, Chicago,1977, pp. 127-64.

[3] George Iggers, "A Search for A Post-Post Modern Theory of History", *History and Theory*, Vol. 48, No.1, February 2009, p. 122.

[4] Although this isn't exactly re-inventing the wheel – three prominent examples are: Benedetto Croce, *History as the Story of Liberty,* Allen & Unwin, London, 1941; R.G. Collingwood, *The Idea of History*, Oxford University Press, Oxford, 1944; Charles A. Beard, "Written History As An Act of Faith", *American Historical Review*, Vol. 39, 1934, pp. 219-31; also "That Noble Dream", *American Historical Review*, Vol. 41, 1935, pp. 74-87.

[5] See Alexander Macfie, "On the Defence of (my) History", *Rethinking History*, Vol.14, May 2010, pp. 209-27.

[6] See for example, Jörn Rüsen, "Introduction", in *Meaning and Representation in History*, Berghahn, Oxford, 2007 pp. 1-2. A word or two needs to be said here about the use of the

word 'history' and 'historians' in this piece. It might be said that I am somewhat homogenizing history and historians in this article – that is, treating historians as a generic group who generally utilise the same approaches for studying and writing about the past. I would argue to an extent that this is true, drawing on Kuhn's idea of eternal values in science. However, to explain and justify this position would need an essay in itself.

[7] I am currently working on a full length treatise of Kuhn's thought as it relates to history and historiography.

[8] Martin L. Davies, *Historics*, Routledge, London, 2006, p. 196.

[9] Antoon de Baets, *Responsible History*, Berghahn Books, Oxford, 2009, p. 14.

[10] The disciplinary matrix consists of 'the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community.' Thomas Kuhn, *Structure of Scientific Revolutions, 3rd Edition*, p. 175. At a late stage of proof reading this article, I came across Michael Oakeshott's remarks about the unity of the sciences outweighing their diversity, and I believe a similar case can be made for history. See Oakeshott, *Experience and it's Modes,* Cambridge University Press, Cambridge, 1933, p. 244.

[11] As Hayden White puts it, 'the historical profession likes to think of itself as a guild; it might be more aptly likened to a tribe. Certainly the rites of passage through which one must pass share more with tribal initiation than they do with admission procedures in the craft guild.' White, "The Politics of Contemporary Philosophy of History", reprinted in Robert Doran, ed., *The Fiction of Narrative*, Johns Hopkins University Press, Baltimore, 2010, p. 137.

[12] This sketchy exegesis will be elaborated on in my forthcoming thesis.

[13] I use the term 'boilerplate' here in the sense of its original use : to describe syndicated material supplied especially to weekly newspapers in matrix or plate form – i.e. a formulaic template.

[14] See Ian Hacking: 'Science journalists may now fill their articles with the word 'paradigm', but it is not a word that plays any role in reflection about serious research. It is quite the opposite in the social and psychological sciences,' in I. Hacking, "Five Parables" in *Historical Ontology*, Harvard University Press, Cambridge Massachusetts, 2002, p. 40. Again, this will be addressed more fully in my aforementioned thesis. Briefly, Kuhn's conception of the paradigm was extremely vague and philosophically messy. Although he attempted – and to an extent succeeded – in clarifying the concept with the ideas of the 'disciplinary matrix' and 'exemplar', the idea of the paradigm disappeared from his thought. In his last essays written before his death, he simply used the term 'theory' in its place. Indeed, if one opens any random page of *Structure* and replaces the word 'paradigm' with 'theory', inevitably it works just as well.

[15] In the words of Lewis Feuer, 'As the scientists work...has become more of more of a job in a bureaucratised and institutionalised setting, he has surrendered the optimistic self dedication which characterized his predecessors. The contemporary scientist usually advocates no 'new' philosophy; instead he shares the prevalent ideology and values.' This might equally be applied to historians. Lewis Feuer, "The Psychological Revolution: The Emotional Source of the Scientific Movement" in George Basalla, ed.,, *The Rise In Modern Science: Internal or External Factors*, DC Heath, Massachusetts, 1968, p. 69.

[16] Although interestingly enough in one of his works, Thompson put into print the thoughts that have crossed the minds of many a historian : 'I sit here in my study... the desk and floor piled high with five years of notes, Xeroxes, rejected drafts, the clock once again moving into the small hours , and see myself, in a lucid instant, as an anachronism. Why have I spent these years trying to find out what could, in its essential structures, have been known without any investigation at all?' Edward Thompson, *Whigs and Hunters*, Allen Lane, London, 1975, p. 260.

[17] As David Cannadine puts it, 'much of this vast public output [from historians] is read by so small an audience that it is tempting to wonder what is the point of writing it and publishing it in the first place.' David Cannadine, *Making History Now*, University of London Institute of Historical Research, London, 1999, p. 10.

[18] Thomas Kuhn, *Structure*, p. 24.

[19] Such a view pre-supposes a theory of historiographical holism – and micro-historians might argue that their work constitutes both the part and the whole.

[20] Thomas Kuhn, *Structure*, p. 24.

[21] This is not to solely equate history with archives.

[22] Quoted in Maurice Powicke, *Modern Historians and the Study of History*, Odham Press, London, 1955, p. 33.
[23] Geoffrey Elton, *Practise of History,* Fontana, London, 1967, p. 12. Elton was responding to Carr's remark that 'I have no patience with the fashion…of pretending that Mommsen's greatness rests not on his History of Rome, but on his corpus of inscriptions and his work on Roman constitutional law: this is to reduce history to the level of compilation,' in E.H. Carr, *What Is History, 2nd Edition,* Penguin London, 1990, p. 37.
[24] Maurice Powicke, *Modern Historians,* p. 93.
[25] Quoted in Lawrence Stone, *The Past and the Present Revisited*, Longman, 1989, p. 8.
[26] Lawrence Stone, *The Past and the Present*, p. 8.
[27] Thomas Kuhn, *Structure*, p. 25.
[28] Thomas Kuhn, *Structure*, p. 25.
[29] E.H. Carr, *What Is History?* p. 12.
[30] E.H. Carr, *What Is History?* p. 12.
[31] And of course, the advent of social history discredited the notion of the historical fact as an indivisable entity. See, amongst others, Carl Becker, "What Are Historical Facts?" *Western Political Quarterly,* Vol.8, No.2, 1955, pp. 327-40.
[32] Thomas Kuhn, *Structure*, p. 25.
[33] See for example "Cato", *Guilty Men*, Victor Gollancz Ltd., London, 1940; Lewis Namier, *Diplomatic Prelude*, Macmillan, London, 1948.
[34] A.J.P. Taylor, *The Origins of the Second World War*, Hamish Hamilton, London, 1961.
[35] As George Kitson-Clark famously put it, the historian 'cannot unscramble the eggs of history in order to make up his mind which of them spoiled the dish that was to be eaten.' in G. Kitson-Clark, *The Critical Historian*, Heinemann, London, 1967, p. 23.
[36] For a collection of said articles, see Esmonde Robertson,, ed., *The Origins of the Second World War*, Macmillan, London, 1971.
[37] See Thomas Kuhn, *Structure*, p. 156.
[38] Quoted in Arnold Thackeray and Robert K Merton, "On Discipline Building: The Paradoxes of George Sarton", in *Isis*, Vol. 63, No. 4, p. 481. In his book *Historics*, Martin L. Davies argues that the provisional nature of history does not differentiate it from science: on the contrary, 'being provisional gives it the science look....The difference is rather: as theoretical reconception, science is progress; as affirmative recognition, history is the result. This makes it [history] a dubious science,' In M. L. Davies, *Historics,* Routledge, London, 2006, pp. 128-29.
[39] For instance, see John Gaddis, *We Now Know*, Oxford University Press, Oxford, 1997; Also Eric Hobsbawm: "Can We Write The History of The Russian Revolution", reprinted in *On History*, Abacus, London, 1998, pp.319-333.
[40] E.H. Carr, *What Is History?* p. 32.
[41] Thomas Kuhn, *Structure*, p. 27.
[42] It should perhaps be noted that historians had reached this conclusion before Norwood Hanson and others did in science – see A.J.P. Taylor, "The Rise and Fall of Diplomatic History", reprinted in *Europe : Grandeur and Decline*, Penguin, Harmondsworth, 1967, p. 167.
[43] See Thomas Kuhn, *Structure,* pp. 52-65.
[44] See David Irving, *Hitler's* War, Viking, New York, 1997; David Abrahams, *The Collapse of the Weimar Republic*, Princeton University Press, New Jersey,1986. Irving's case may be an example of the Planck effect. It is notable that the few historians who were willing to say anything mildly positive about him after the Lipstadt libel trial – John Erikson, D.C. Watt, John Keegan – were 71, 74 and 66 years old respectively. At the time of writing only Keegan is still alive.
[45] Thomas Kuhn, *Structure,* p. 52.
[46] Thomas Kuhn, *Structure,* p. 52.
[47] Arnaldo Momigliano, "George Grote and the Study of Greek History", reprinted in A. Momigliano, ed., *Studies in Historiography*, Weidenfield, London, 1966, p. 71.
[48] J.R. Jones,*The Revolution In England of 1688*, Weidenfield & Nicholson, London, 1972, pp. 387-88.
[49] See J.W. Johnson, *A Profane Wit: The Life of Lord Rochester,* University of Rochester Press, New York, 2004; Jeremy Lamb, *So Idle a Rogue*, History Press, London, 2005; Roy Sundstrom, *Sidney Godolphin : Servant of the State*, University of Delaware Press, Newark, 1993.

[50] James Watson,*The Double Helix*, Simon and Schuster, New York, 2001. This classic account of the discovery of the structure of DNA makes it clear how much collaboration is involved in science.
[51] To an extent this refutes Kuhn's view that 'often a paradigm emerges, at least in embryo, before a crisis has developed far or explicitly been recognised.' Thomas Kuhn, *Structure*, p. 86.
[52] See Fritz Fischer, *Griff Nach Der Weltmacht*, Droste Verlag, Dusseldorf, 1961; Maurice Cowling, *The Impact of Labour,* Cambridge University Press, Cambridge, 1971.
[53] I am indebted to Nick Smart for this suggestion.
[54] Laurence Stone,*The Causes of the English Revolution*, Longmans, London, 1972.
[55] R.H. Tawney, "The Rise of the Gentry", *Economic History Review*, Vol. 11, No.1, 1941, pp. 1-38.
Lawrence Stone, "The Anatomy of the Elizabethan Aristocracy", *The Economic History Review,* Vol. 18, No. ½, 1948, pp. 1-53
[56] See Hugh Trevor-Roper, "The Elizabethan Aristocracy: An Anatomy Anatomised", *The Economic History Review*, New Series, Vol.3, No.3, 1951, pp. 179-298; Hugh Trevor-Roper, "The Gentry 1540-1640", *The Economic History Review*, Supplement 1, 1953. Tawney later remarked that 'an erring colleague is not an Amalakite to be smitten hip and thigh.'
[57] Lawrence Stone gives an excellent account of the historiography in *The Causes of the English Revolution*, 2nd Edition, Routledge, London, 1993, pp. 26-40.
[58] Richard Evans, "The New Nationalism and the Old History", *Journal of Modern History*, 59, December 1987, p. 792.
[59] Does a period of infighting in itself constitute an anomaly? I would argue that when the infighting escalates to a level above the normal turf wars, then it can be classed as an anomaly, on the grounds that it usually produces a crisis which leads to a historiographical shift.
[60] Oliver Daddow has argued that taking issue with established opinion is in fact part of the historian's nature, a kind of inherent contrariness. He quoted Richard Fuller's remark that historians are like 'birds…who cannot rise except it be by flying against the wind, as some hope to achieve their advancement by being contrary and paradoxical in judgement to all before them.' This is certainly the case for the likes of Taylor and Cowling, and probably many more historians. See Oliver Daddow*, Britain and Europe*, Manchester University Press, Manchester, 2004., pp. 140-41.
[61] Richard Evans,*The Third Reich at War*, Allen Lane, London, 2008.
[62] The same can be said of Ian Kershaw's recent biography of Hitler. See Kershaw *Hitler: Hubris 1889-1936*, London, Allen Lane, 1998, and *Hitler: Nemesis 1936-1945*, Allen Lane, London, 2000.
[63] For such works as these, perhaps Jordanova's term of 'last word books – books so comprehensive and thorough that they command special respect.' Ludmilla Jordanova, *History in Practice*, Arnold, London, 2000, p. 103.
[64] This ties in with the standard definition of creativity by researchers in that particular field, which states that as well as being 'original, novel or surprising', but the idea must also be 'adaptive, functional or effective.' See Tudor Rickards, Mark A. Runco and Susan Moger, *Routledge Companion to Creativity*, Routledge, London, 2008, p. 247.
[65] Larry Laudan, *Progress and its Problems*, Oxford University Press, Oxford, 1977, p. 138.
[66] The publication of an article by Ernst Nolte and a book by Andreas Hillgrüber triggered a wide debate over the way German historiography had dealt with the Nazi past, in particular the Holocaust. The debate was not confined to the scholarly journals, but also appeared in magazines and on television. It centred around a number of questions, but at its core was the question of whether or not Germany could discard the attitude adopted in 1945 as marking that year as 'Year Zero', and start celebrating the positive aspects of their past once again? The debate raged on from 1986-1989 – at which the Berlin Wall came down, and a new factor was added to the polemics.
[67] Thomas Kuhn, *Structure*, p. 82.
[68] Thomas Kuhn, *Structure,* p. 83.
[69] Esmonde Robertson, "Introduction", in *Origins ,* p. 6.
[70] This was perhaps Kuhn's most controversial thesis: 'the historian of science may be tempted to exclaim that when paradigms change, the world changes with them.,' in Thomas Kuhn, *Structure*, p.111.

[71] Lewis Namier, *Avenues of History*, Hamish Hamilton, London, 1952, p. 8-9.
[72] See Thomas Kuhn, "Reflections on my Critics", in Imre Lakatos and Alan Musgrave, *Criticism and The Growth of Knowledge*, Cambridge University Press, Cambridge, 1970, p. 237.
[73] Keith Jones, "Is Kuhn a Sociologist?", *British Journal in the Philosophy of Science,* No. 36, 1986, p. 451. In Kuhn's own work, see "Reflections on my Critics" – 'some of the principles deployed in my explanation of science are irreducibly sociological.' p. 237. In the preface to *The Essential Tension*, he describes his work as being 'deeply sociological,' in, Thomas Kuhn, *The Essential Tension,* University of Chicago Press, Chicago, 1977, p. xx.
,[74] See, for example, David Bloor, *Knowledge and Social Imagery*, Routledge, London , 1976 and Andrew Pickering, *Constructing Quarks*, University of Chicago Press, 1984.
[75] E.H. Carr, *What Is History?* p. 44.
[76] A.J.P. Taylor*, Origins of the Second World War*, p.34
[77] Gordon Martell, "Introduction"*,* in Martell, *Origins of the Second World War Reconsidered*, 2nd Ed, Routledge, London, 1999, p. 3.
[78] A.J.P. Taylor*, Origins of the Second World War*, p. 35.
[79] This is to say, they adopted an explicitly sociological structure in framing their studies.
[80] In particular, Kuhn's anti-representationalist philosophy, which I will expound in my forthcoming thesis.
[81] See for example, George Iggers, "A Search For Post-Modern Theory of History", p. 122.
[82] I am referring here to the multitude of post-structuralist philosophers and literary theorists who confidently assured historians that history as we know it could not, and indeed should not, continue to be practised. As Hugh Trevor Roper put it on another occasion, 'when radicals scream that victory is indubitably theirs, sensible conservatives knock them on the nose.' Quoted in E. H. Carr, *What Is History?,* p. 153.
[83] John Zammito, *A Nice Derangement of Epistemes,* University of Chicago Press, Chicago, 2004, p. 52. This is not to say that he was the only philosopher of science to do this – Kuhn himself was heavily influenced by the work of Alexander Koyre, amongst others.
[84] Although some would argue against this. See for example Gyan Prakash – 'Subaltern Studies As Postcolonial Criticism', *The American Historical Review*, Vol. 99 No.5, 1994, pp. 1475-90. See also Carolyn Steedman's argument – 'there is a double nothingness in the writing of history and in the analysis of it: it is about something that never did happen in the way it comes to be represented (the happening exists in the telling or the text); and it is made out of materials that aren't there, in an archive or anywhere else. We should be entirely unsurprised that deconstruction made no difference to this kind of writing.' Carolyn Steedman, *Dust*, Manchester University Press, Manchester, p. 154
[85] Christopher Lasch, "Consensus: An Academic Question?", *Journal of American History* Vol.76, 1989, p. 458.
[86] See Isabelle Stengers, *The Invention of Modern Science*, University of Minnesota Press, Minnesota 2000, p. 5.
[87] Although there is one exception – see Willie Thompson, *Whatever Happened To History?* Pluto Press, London, 2000.
[88] See Patrick Joyce, "The Gift of the Past", in Jenkins, Morgan & Munslow, (eds)., *Manifestos For History*, Routledge 2007, London, pp. 88-97.