REDEFINING REVOLUTIONS

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1. A Nice Knockdown Argument

Moti Mizrahi does an admirable job in pruning the thicket that has grown up around Thomas Kuhn's incommensurability thesis. He begins with a distinction between two versions of the thesis:

> *Taxonomic Incommensurability (TI)* Periods of scientific change (in particular, revolutionary change) that exhibit TI are scientific developments in which existing concepts are replaced with new concepts that are incompatible with the older concepts. The new concepts are incompatible with the old concepts in the following sense: two competing scientific theories are conceptually incompatible (or incommensurable) just in case they do not share the same "lexical taxonomy". A lexical taxonomy contains the structures and vocabulary that are used to state a theory.

> *Methodological Incommensurability (MI)* There are no objective criteria of theory evaluation. The familiar criteria of evaluation, such as simplicity and fruitfulness, are not a fixed set of rules. Rather, they vary with the currently dominant paradigm. (Mizrahi 2015a, 362; references omitted).

Mizrahi's focus is exclusively on (TI), a focus that I will share. He proceeds to argue that, understood as (TI), the incommensurability thesis is poorly motivated. His critique differs from that of many other authors in focussing on the weakness of the arguments in support of (TI), rather than the strength of the arguments against it. He claims that the former arguments must be either deductive or inductive. In both cases, he presents a counterargument. Against deductive support, he argues as follows:

(1) Reference change (discontinuity) is conclusive evidence for (TI) only if reference change (discontinuity) entails incompatibility of conceptual content.

(2) Reference change (discontinuity) does not entail incompatibility of conceptual content.
    Therefore:

(3) It is not the case that reference change (discontinuity) is conclusive evidence for (TI). (Mizrahi 2015a, 367).

Against inductive support, he argues as follows:

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(1) There is a strong inductive argument for (TI) only if there are no rebutting defeaters against (TI).
(2) There are rebutting defeaters against (TI).
    Therefore:
(3) It is not the case that there is a strong inductive argument for (TI). (Mizrahi 2015a, 371).

The merits of this critique have been addressed elsewhere (Kindi 2015; Marcum 2015; Patton 2015; Mizrahi 2015b,c). In this chapter, I will take a somewhat different tack, by examining the implications for the philosophy of mathematical practice, specifically the debate whether there can be mathematical revolutions. Hence I will not engage closely with all the details of Mizrahi's argument. However, I do wish to draw attention to his use of 'rebutting defeater'. Before introducing an example from the history of medicine of a revolution in which conceptual continuity is displayed, he stresses that

> the following episode is not supposed to be a counterexample against (TI). It is not meant to be a refutation of (TI). Rather, it shows that an inductive argument based on a few selected historical episodes of scientific change does not provide strong inductive support for (TI). Or, to put it another way, this episode—and others like it—counts as what Pollock calls a *rebutting defeater*, i.e. a *prima facie* reason to believe the negation of the original conclusion; in this case, the negation of (TI) (Mizrahi 2015a, 368; reference omitted).

That is, cases of revolutionary change without conceptual discontinuity are rebutting defeaters for (TI) since they are reasons to think that we can have one without the other. They are not undercutting defeaters, since the familiar cases of revolutionary change *with* conceptual discontinuity are still reasons to believe (TI), but if we have as many reasons to disbelieve it as to believe it, we should probably suspend our judgment.

2. There's Glory for You!

Alice's encounter with Humpty Dumpty is well-known to philosophers:

> "I don't know what you mean by 'glory,'" Alice said.
>   Humpty Dumpty smiled contemptuously. "Of course you don't—till I tell you. I meant 'there's a nice knock-down argument for you!'"
>   "But 'glory' doesn't mean 'a nice knock-down argument,'" Alice objected.
>   "When *I* use a word," Humpty Dumpty said, in rather a scornful tone, "it means just what I choose it to mean—neither more nor less."
>   "The question is," said Alice, "whether you *can* make words mean so many different things."
>   "The question is," said Humpty Dumpty, "which is to be master—that's all." (Carroll 1897, 106 f.).

Several echoes of this passage may be heard in this chapter, most clearly in a Humpty-ish passage of my own:

> A *glorious* revolution occurs when the key components of a theory are preserved, despite changes in their character and relative significance. (We will refer to such preservation, constitutive of a glorious revolution, as *glory*.) An *inglorious* revolution occurs when some key component(s) are lost, and perhaps other novel material is introduced by way of replacement. ... A *paraglorious* revolution occurs when all the key components are preserved, as in a glorious revolution, but new key components are also added. ... A *null* revolution[1] ... when none of its key components change at all (Aberdein and Read 2009, 618 f.).

Here is a slightly more formal characterization of this fourfold distinction. Let us specify that the key components of a theory T_n comprise a structure of some sort, K_n. Then succession between theories T_n and T_{n+1} would be a null revolution if their respective key components are unchanged, that is $K_n = K_{n+1}$, and a glorious revolution if the key components are in some sense isomorphic, $K_n \cong K_{n+1}$, that is if there is a well-motivated bijection between them which respects their roles in each theory. By contrast, in a paraglorious revolution there would be an analogous isomorphic embedding of the key terms of the old theory within the new, $K_n \hookrightarrow K_{n+1}$, such that the new theory contains terms with no clear counterpart in the old. And, in what we may call a strict inglorious revolution, there would be a similar isomorphic embedding of the key terms of the new theory within the old, $K_n \hookleftarrow K_{n+1}$, since there are components of the old theory which have been irretrievably lost in the new. In other words, inglorious revolutions would exhibit 'Kuhn loss', a loss of (actual or potential) explanatory power (for further discussion, see Votsis 2011, 111 ff.). (The more general sense of inglorious revolution may be thought of as a strict inglorious revolution combined with a paraglorious revolution. That is, there would be some structure K_n', not necessarily corresponding to any actually espoused theory, such that $K_n \hookleftarrow K_n' \hookrightarrow K_{n+1}$.)

In my earlier presentation of this distinction, I addressed a number of questions, the most important of which are what components are 'key' and how are they 'preserved'? The simplest answer to the first question would be to make *all* components of a theory key, or at least all components without which the theory could not be articulated. A more subtle account would permit distinctions between the theory proper and auxiliary theories, tentative extensions, and other inessential components, but we need not explore that account here.[2] Indeed, mathematical theories are less trouble than empirical theories in this respect: they characteristically have fewer components and their dependencies are much more clearly stated. The second question is much more of a challenge. Indeed, it is central to understanding what makes a revolution revolutionary: how much change is required for a revolution? Conversely, how much change can a theory undergo without revolution? In other words, just what do we mean by 'glory'? An obvious starting point would be taxonomic commensurability, that is the absence of TI. Notice, incidentally, that lexical taxonomy is shared across neither inglorious nor paraglorious revolutions.

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[1]Less happily, I also referred to theories in null revolution as being in stasis, but 'stasis' is a false friend: although it has come to mean an absence of change, for Aristotle it meant something much like revolution, and is often translated as such (Howell 1985, 18).

[2]The sort of distinction I have in mind is that Stathis Psillos draws between idle and essentially contributing constituents (Psillos 1996, S311) or Philip Kitcher between presuppositional and working posits (Kitcher 1993, 149).

I have defined inglorious and paraglorious revolutions such that the distinction is essentially a matter of historic sequence: whether such a transition counts as inglorious or paraglorious will depend on which theory came first. The definition of (TI), however, despite references to 'new' and 'old', does not seem to directly appeal to chronology.

Another question posed by this framework concerns the transitivity (or not) of these different characterizations of change. A succession of null revolutions must be a null revolution, because we have stipulated strict identity between key components. But a succession of glorious revolutions need not be a glorious revolution: a series of comparatively small changes might add up to a big change, as in a sorites sequence of small changes of colour from red to blue. Likewise, the preservation aspect of paraglorious revolution may fail over a long enough sequence of such revolutions, making the sequence as a whole inglorious. Inglorious revolutions themselves are more straightforward: in principle, two consecutive inglorious revolutions might cancel each other out, so inglorious revolution must be intransitive.

A final concern leads us back to Humpty Dumpty: mere survival (or not) of vocabulary is not what is at issue.[3] Conceptual change might be disguised by a shift in the meaning of a shared vocabulary; conversely, a drastic change of vocabulary may give a misleading impression of change when nothing substantive has occurred. This issue is familiar from political examples: Augustus strategically reused much of the terminology of the old Roman Republic; Stalin was careful not to call himself a Tsar. In our context, this suggests that there are not four, but sixteen relationships between the key components of succeeding theories (using subscripts to indicate outward appearances):

$$K_n =_= K_{n+1} \quad K_n \cong_= K_{n+1} \quad K_n \hookrightarrow_= K_{n+1} \quad K_n \hookleftarrow_= K_{n+1}$$
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The salutary point here is that we need to be careful in how we specify our terms, lest we misclassify (apparent) revolutions. In particular, the sharing of 'lexical taxonomy' had better be more than just lexical, or the innocuous null revolution at the bottom left could count as a case of TI while the stealthy inglorious revolution at the top right does not.

A bold sceptical thesis would be that the rightmost two columns are in practice uninstantiated; that is, all revolutions are glorious, and all appearances to the contrary deceptive. As we will see in the next section, just such a view has been proposed by some historians of mathematics.

3. Mathematical Revolutions?

The discussion of mathematical revolutions essentially begins with Michael Crowe, who boldly asserts as a 'law' that 'Revolutions never occur in mathematics' (Crowe 1975, 19). Nonetheless, his own subsequent writings are increasingly nuanced: he has moved from denying that there any revolutions in mathematics to suggesting that even inglorious revolutions may be possible (1988, 264 f.; 1992, 313). Joseph Dauben has published several articles arguing for the existence of mathematical revolutions (Dauben 1984, 1992, 1996). However, as we shall see, his position is

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[3]Mizrahi also addresses this issue (Mizrahi 2015a, 374).

much closer to Crowe's than might be expected. Indeed, as one commentator, writing more than twenty years ago, has remarked, the literature on mathematical revolutions represents an 'authentic theoretical shambles' (Otero 1996, 193). There have been two collections of papers on the topic (Gillies 1992; Ausejo and Hormigón 1996). But the editor of the first notes that each of his authors 'has a different theoretical perspective' (Gillies 1992, 8). And a contributor to the second pointedly observes that none of these authors make much use of a Kuhnian framework in their other writing on mathematical practice (Corry 1996, 170). In this section I shall attempt to resolve some of this confusion.[4]

Many accounts of revolutions in mathematics distinguish two sorts of revolution, usually in terms of the presence or absence of some sort of conceptual continuity. Hence Crowe distinguishes a 'transformational event', in which 'an accepted theory is overthrown by another theory, which may be old or new', from a 'formational event', in which 'an area of science is not transformed, but is *formed*. The discovery that produces this effect is usually new, and by definition overthrows and replaces nothing' (Crowe 1992, 310, citing his own earlier work). Dauben likens mathematical revolutions to the Glorious Revolution of 1688, in the persistence of the 'old order', albeit 'under different terms, in radically altered or expanded contexts' (Dauben 1984, 52). This political analogy is echoed by Donald Gillies, who frames the distinction as between 'Franco-British' and 'Russian' revolutions: in the former a 'previously existing entity persists' through 'a considerable loss of importance'; in the latter the 'previously existing entity' is 'overthrown and irrevocably discarded' (Gillies 1992, 5). Gillies's choice of terminology provokes the distracting historiographical question of why the French revolution should be more like the British than the Russian. After all, France and Russia both ended up as republics, whereas Britain did not. The answer seems to be that Gillies stops the clock at some point in the reign of Louis-Philippe. My own use of 'glorious' revolutions, inspired by Dauben's usage but not intended to be given any specific historical reading, at least has the merit of sidestepping such musings. More importantly, we may notice that these distinctions do not necessarily coincide—Crowe's formational events seem closer to paraglorious than glorious revolutions, for example—and that, although all of them are binary, none of them seems to be exhaustive. So a more fine-grained distinction may be a source of clarity.

However the distinction is drawn, most of its framers agree that only glorious revolutions are possible in mathematics: 'One important consequence, in fact, of the insistence on self-consistency within mathematics is that its advance is necessarily cumulative. New theories cannot displace the old' (Dauben 1984, 62); 'In science both Russian and Franco-British revolutions occur. In mathematics, revolutions do occur but they are always of Franco-British type' (Gillies 1992, 6); 'revolutions do occur in mathematics, but are confined entirely to the metamathematical component of the community's shared background' (Dunmore 1992, 223). In this regard they concur with the opinion of many mathematicians that their discipline is cumulative. Crowe finds such sentiments expressed by Fourier in 1822, Hankel in 1869, and Truesdell in 1968 (Crowe 1975, 19). Indeed, celebrated mathematicians are still saying as much: 'central contributions have been lasting, one does not supersede another, it enlarges it' (Langlands 2013, 25). As Crowe poetically summarizes the

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[4]For a more extensive account of the debate over revolutions in mathematics, see (François and Van Bendegem 2010, 107 ff.).

conventional view, 'Scattered over the landscape of the past of mathematics are numerous citadels, once proudly erected, but which, although never attacked, are now left unoccupied by active mathematicians' (Crowe 1988, 263). Nonetheless, there are exceptions to this trend. Michael Harris notes Kronecker in 1891 observing that 'in this respect mathematics is no different from the natural sciences: new phenomena overturn the old hypotheses and put others in their place' and Siegel in 1964 characterizing work revisionary of his own as 'a pig broken into a beautiful garden and rooting up all flowers and trees' (Harris 2015, 4).

Bruce Pourciau complains that the 'Crowe–Dauben debate' is actually a 'Crowe–Dauben consensus', viz.: Kuhnian revolutions are inherently impossible in mathematics' (Pourciau 2000, 301). For Pourciau a revolution is Kuhnian or '*noncumulative* whenever some true statements of the old conception have no translations (faithful to the original meaning) which are true statements in the new conception' (Pourciau 2000, 301). Pourciau argues that Brouwerian intuitionism is a (failed) Kuhnian revolution. Certainly, if adopted, this would have required wholesale revision of results treated as certain by prior mathematicians, thereby meeting the strictest definition of Kuhnian revolution. Its usefulness as an example might be somewhat compromised by the fact that it never actually happened, but it was seriously proposed and still commands some support. However, Pourciau may be overestimating the difficulty in supplying examples of Kuhnian revolutions in mathematics in two ways: one of scale, one of chronology. Firstly, although the standard example of a Kuhnian revolution in natural science is the Copernican revolution, an epochal upending of an all-encompassing worldview, it is a mistake to suppose that all Kuhnian revolutions need be so drastic. Stephen Toulmin once complained that Kuhn had surreptitiously revised his position to admit 'small-scale "microrevolutions"' (Toulmin 1970, 47). Kuhn strenuously rejected this imputation: 'My concern ... has been throughout what Toulmin now takes it to have become: a little studied type of conceptual change which occurs frequently in science and is fundamental to its advance' (Kuhn 1970, 249 f.). He subsequently characterized a paradigm as 'what the members of a scientific community and they alone share' where such communities may comprise 'perhaps 100 members, sometimes significantly fewer' (Kuhn 1974, 460; 462). Happily enough, this coincides with an influential estimate of the size of mathematical research communities: 'a few dozen (at most a few hundred)' (Davis and Hersh 1980, 35). Secondly, as observed above, paraglorious and inglorious revolutions are essentially symmetrical; they differ only in the chronological sequence of the contrasting theories. Paraglorious revolutions are cumulative, but they exhibit a conceptual discontinuity formally identical to that exhibited by inglorious revolutions. Chronological sequence alone does not seem to be a principled basis on which to discount paraglorious revolutions as Kuhnian.[5]

Three broad strategies for the identification of Kuhnian (or nonglorious) revolutions in mathematics arise from this discussion. Firstly, we may look directly for inglorious revolutions: conceptual shifts within mathematics in which key components have been lost. Secondly, we may look for paraglorious revolutions: conceptual shifts within mathematics in which key components have been gained. Thirdly,

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[5]I make no claim as to Kuhn's own view on this issue, although I note that he does refer to historians experiencing revolutions by 'moving through time in a direction opposite to scientists' (Kuhn 2000 [1983], 57), which at least suggests an openness to temporal symmetry.

we may look for sorites-like sequences of glorious (or paraglorious) revolutions which exhibit non-transitivity of glory, that is which are collectively inglorious. The search is complicated by several factors. In particular, it is not easy to determine whether a given episode is revolutionary; nor is it easy to determine what type of revolution a given revolutionary episode exemplifies. Hence some of the same examples might be claimed as successes for more than one of these search strategies. An analysis of even a single case study thorough enough to settle all of these issues would be beyond the scope of this chapter. However, in the following sections I will discuss several putative mathematical revolutions in what I hope to be at least enough detail to indicate the prospects for these strategies.

4. $\mathbb{Q} \to \mathbb{R} \to \mathbb{C}$

The most obvious example of incommensurability in mathematics must be incommensurability itself! The concept is, of course, originally a mathematical one, credited to the very earliest Greek geometers, the Pythagoreans. Thomas Heath, in his edition of Euclid, quotes a scholium on the first proposition of Book X, attributed to Proclus: 'They called all magnitudes measurable by the same measure commensurable, but those which are not subject to the same measure incommensurable' (Heath 2006 [1908], 684). Specifically, the Pythagoreans discovered that $\sqrt{2}$ was incommensurable with the natural numbers, that is, it cannot be expressed as a ratio of natural numbers or, as we would say, as a rational number. The discovery was credited to one Hippasus of Metapontum, who is reputed to have drowned in a shipwreck. As the historian of mathematics Kurt von Fritz observes,

> The discovery of incommensurability must have made an enormous impression in Pythagorean circles because it destroyed with one stroke the belief that everything could be expressed in integers, on which the whole Pythagorean philosophy up to then had been based. This impression is clearly reflected in those legends which say that Hippasus was punished by the gods for having made public his terrible discovery (Fritz 1945, 260).

Even in ancient times, the allegorical aspects of this story were already apparent, 'hinting that everything irrational and formless is properly concealed, and, if any soul should rashly invade this region of life and lay it open, it would be carried away into the sea of becoming and be overwhelmed by its unresting currents', as Proclus puts it (Heath 2006 [1908], 684).

For our purposes, the crucial point in this narrative is that the change initiated by Hippasus was revisionary of earlier mathematics: it 'required an entirely new concept of ratio and proportion and a new criterion to determine whether two pairs of magnitudes which are incommensurable with one another have the same [ratio]' (Fritz 1945, 262). The completion of this task by later mathematicians, notably Theaetetus and Eudoxus, is one of the great achievements of Greek mathematics, and plausibly a major driver of its early development of the concept of rigorous proof. For Dauben it is one of the best examples of a mathematical revolution. He stresses that the 'transformation of the concept of number ... entailed more than just extending the old concept of number by adding on the irrationals—the entire concept of number was inherently changed, transmuted as it were, from a world-view in which integers alone were numbers, to a view of number that was eventually

related to the completeness of the entire system of real numbers' (Dauben 1984, 57).

In my terminology, this is clearly not a glorious revolution, since a literally incommensurable concept has been added. So it is at least a paraglorious revolution. Might we go further and identify it as also inglorious? On the one hand something has certainly been lost: the 'world-view in which integers alone were numbers', for a start. On the other hand, world-views are not part of the subject matter of mathematics. Hence Caroline Dunmore identifies this shift as 'the first great meta-level revolution in the development of mathematics' (Dunmore 1992, 215). On Dunmore's account, object level revolutions in mathematics are always glorious, but they are always accompanied by inglorious revolutions in the meta-level, that is in the philosophical or methodological presuppositions (Dunmore 1992, 225). The rational numbers are still an object of mathematical enquiry and the Pythagorean results about their comparison still hold. Nonetheless, it is highly misleading to conceive of the real numbers as a conservative extension of the rationals. The real numbers are constructed on a quite different basis, but in such a way that a subset isomorphic to the rationals may be identified.

Strictly speaking, the taxonomic incommensurability between mathematics defined over $\mathbb{Q}$ and mathematics defined over $\mathbb{R}$ runs both ways. Clearly, real mathematics cannot be done with rationals alone, but techniques that work over $\mathbb{Q}$ fail over $\mathbb{R}$. So a revolution from the mathematics of $\mathbb{Q}$ to the mathematics of $\mathbb{R}$ would be paraglorious and inglorious. However, the actual revolution could also be described as a shift from the mathematics of $\mathbb{Q}$ to (eventually) the mathematics of $\mathbb{Q}$ and $\mathbb{R}$, understood as separate projects. That shift would be strictly paraglorious—assuming that the mathematics of $\mathbb{Q}$ has been preserved, and not just reconstructed. The same issue arises with supersets of the reals, whether well-established, such as the complex numbers, or more contentious, such as the hyperreals, which include infinitesimals (Bair et al. 2013). The underlying issue of cross-sortal identity is a known problem for a wide range range of philosophies of mathematics (Cook and Ebert 2005, 124). Textbook presentations of the foundations of mathematics are obliged to address cross-sortal identity, which they do in a variety of ways, often at odds with mathematical practice (for a careful discussion, see Ganesalingam 2013, 180 ff.). It is also important to note that retrofitting a new foundation onto existing mathematics is not confined to number systems. Indeed it has been a major feature of mathematical research since the nineteenth century— and it is precisely what Siegel was complaining about as 'rooting up all flowers and trees' (Lang 1994). It is a deep question whether such moves can be understood as merely adding 'a new storey to the old structure' (Crowe 1975, 19, quoting Hankel). To pursue the architectural metaphor, they might be better characterized as 'façading', whereby the front elevation of an otherwise demolished building is incorporated into its successor. I shan't settle that question here, but we may observe that these shifts are at least paraglorious and perhaps inglorious.

5. Irony for Mathematicians

One way of approaching the issue of inglorious revolution in mathematics is through a related question: when do mathematicians say things that are not so? One prospect might be the assumptions of reductio proofs. In a recent essay, the mathematician Timothy Gowers briefly considers, but ultimately rejects, the

intriguing idea 'that proofs by contradiction are the mathematician's version of irony' (Gowers 2012, 224). He objects that 'when we give a proof by contradiction, we make it very clear that we are discussing a counterfactual, so our words *are* intended to be taken at face value' (op. cit.). Perhaps more tellingly, we might frame this objection as saying that a proposition assumed for the purposes of proof by contradiction is presented as the antecedent of a conditional: 'If P were the case, then ... a contradiction would follow. So, not P.' Nonetheless, at least for the duration of the ellipsis, the mathematician proceeds as though P were being seriously entertained. An inattentive reader who began reading a proof part way through would not necessarily be able to tell which proposition the mathematician intended to show to be false—or even that any of them were presented with this intent.

Another possibility might be unproven conjectures upon which mathematicians sometimes rely, when exploring their consequences. The mathematician Barry Mazur talks of 'architectural conjectures' that 'play the role of "joists" and "supporting beams" for some larger mathematical structure yet to be made' (Mazur 1997, 199). The formulation of such conjectures

> is often a way of "formally" packaging, or at least acknowledging, an otherwise shapeless body of mathematical experience that points to their truth. ... These conjectures sometimes round out a field by being clear, general (but not yet proved) statements enabling one to understand where a certain amount of on-going, perhaps fragmentary, specialized work is headed; they provide a focus (op. cit.).

Architectural conjectures seldom arise alone; they often comprise elaborate networks of interlinked conjectures that present the outline of what is hoped to be many years of fruitful work. One of the best known such networks of conjectures in contemporary mathematics is the Langlands programme, 'an extensive web of conjectures by which number theory, algebra, and analysis are interrelated in a precise manner, eliminating the official divisions between the subdisciplines' (Zalamea 2012, 180). This has been enormously influential, guiding the work of scores of mathematicians who have confirmed some—but by no means all—of its key conjectures.

As with reductio hypotheses, conjectures are strictly to be understood as the antecedents of conditionals. Mathematicians should not be seen as *asserting* them until they have actually been proven. Nonetheless, as with reductio hypotheses, they are presented in apparent earnest, and their implications investigated with all due rigour: they 'are *expected* to turn out to be true, as, of course, are all conjectures' (Mazur 1997, 199). So, naively, it may seem as though neither sort of hypothesis is much use for present purposes, since mathematicians seem to have an uncanny knack of only assuming for proof by contradiction things that are false and only assuming as conjectures things that are true (even if not yet proven). This would be a profound misperception: attempted reductios sometimes founder on the truth of the hypothesis (famously so in the accidental discovery of non-Euclidean geometry) and sometimes substantial effort is devoted to exploring the consequences of false conjectures. In the next section we will encounter an example of the latter.

6. The World Without End Hypothesis

In 2016 Michael Hill, Michael Hopkins, and Douglas Ravenel published an article in one of the most prestigious journals in mathematics, with the following abstract: 'We show that the Kervaire invariant one elements $\theta_j \in \pi_{2^{j+1}-2}S^0$ exist only for $j \leq 6$. By Browder's Theorem, this means that smooth framed manifolds of Kervaire invariant one exist only in dimensions 2, 6, 14, 30, 62, and possibly 126. Except for dimension 126 this resolves a longstanding problem in algebraic topology' (Hill et al. 2016, 1). This was the final, fully vetted version of a result that they had announced seven years earlier. As they summarize their result in a preliminary expository article, they showed that 'certain long sought hypothetical maps [the θ_j for $j \geq 7$] between high dimensional spheres do not exist' (Hill et al. 2010, 32). This outcome was a surprising one, not just because of the technical depth of the work required but because many experts in the area had long expected the opposite result: 'The problem solved by our theorem is nearly 50 years old. There were several unsuccessful attempts to solve it in the 1970s. They were all aimed at proving the opposite of what we have proved' (Hill et al. 2010, 32). The hypothesis that the sought after maps all exist came to be known as the World Without End Hypothesis; the contradictory hypothesis that the θ_j only exist for small j was known as the Doomsday Hypothesis. Hill, Hopkins, and Ravenel proved the Doomsday Hypothesis, and thereby disproved the World Without End Hypothesis.

While not remotely on the scale of the Langlands Programme, the World Without End Hypothesis was not just a single assertion, but the basis for a whole system of 'architectural conjectures': the new proof demolished 'what Ravenel calls an entire "cosmology" of conjectures' (Klarreich 2011, 374). The triumph of the Doomsday Hypothesis undercut a growing sense of understanding provided by the World Without End Hypothesis. As the reviewer of Hill, Hopkins, and Ravenel's paper in *Mathematical Reviews* comments, the World Without End Hypothesis 'was so compelling that many believed the θ_j must exist; now that we know they don't, the behavior of the EHP sequence is much more mysterious. In particular, Mahowald's η_j-elements ... now appear entirely anomalous' (Goerss 2016). The author of a book surveying some of the techniques developed in pursuit of the World Without End Hypothesis published shortly before Hill, Hopkins, and Ravenel's announcement concluded his preface as follows: 'In the light of the above conjecture [the Doomsday Hypothesis] and the failure over fifty years to construct framed manifolds of Arf-Kervaire invariant one this might turn out to be a book about things which do not exist' (Snaith 2009, ix).

The collapse of the World Without End Hypothesis seems to be an inglorious revolution. It is clearly a case of referential discontinuity, since a whole class of objects which were key to the old theory have been shown not to exist. It might be objected that there is no taxonomic incommensurability, since the conjectures are still perfectly intelligible, despite now being known to be false. Indeed we have seen that Mizrahi argues that referential discontinuity does not entail conceptual incompatibility, and is thereby insufficient for taxonomic incommensurability (see §1 above). So what else is required? A natural candidate would be Kuhn loss: reduction in actual or potential explanatory power. Kuhn loss does not seem to feature in the examples Mizrahi adduces of referential discontinuity without conceptual incompatibility (Mizrahi 2015a, 365 ff.), but it is exhibited here: the understanding that had seemed to follow from the World Without End Hypothesis has been lost.

Specifically, comments such as Paul Goerss's reveal the 'mysterious' and 'anomalous' nature of some of the surviving results, now that the architectural conjectures which mathematicians had relied on to understand them have been falsified. This is what we should expect from a reduction in explanatory power.

If the collapse of the World Without End Hypothesis is a revolution, it is certainly a small-scale one. The constituency of homotopy theorists for whom this was an active research area would be at the low end of Kuhn's 'perhaps 100 members, sometimes significantly fewer' comprising a scientific community (Kuhn 1974, 462). However, this makes this revolution much more likely to be typical of mathematical revolutions. Large-scale revolutions are necessarily extremely rare; so much so that their instances may well be *sui generis*. Conversely, small-scale revolutions are much better placed to support generalizations: there are plenty of other failed architectural conjectures that could be explored in similar detail (see, for example, some of the 'cautionary tales' in Jaffe and Quinn 1993, 7 f.).

7. Inter-Universal Teichmüller Theory

One of the more widely discussed open problems in modern number theory is the *abc* conjecture. Like many such problems it can be stated quite simply, but defies simple solution. Here is what it says:

The *abc* conjecture. For every $\varepsilon > 0$, there are only finitely many triples (a, b, c) of coprime positive integers where $a + b = c$, such that $c > d^{1+\varepsilon}$, where d denotes the radical of abc (the product of its distinct prime factors).

For example, try $a = 15$ and $b = 28$. These are coprime, but $c = 43$ and $d = 2 \times 3 \times 5 \times 7 \times 43 = 9030 \gg 43$. So $(15, 28, 43)$ is not one of the specified triples (for any ε). On the other hand, let $a = 1$ and $b = 63$. Then we have $c = 64$ and $d = 2 \times 3 \times 7 = 42 < 64$. So $(1, 63, 64)$ is such a triple (at least for values of $\varepsilon < .11269$).

In a series of preprints appearing on his website in 2012, the respected mathematician Shinichi Mochizuki claimed to have a proof of the *abc* conjecture. However, Mochizuki's claimed proof introduced so many new techniques and concepts that other leading mathematicians in the field described it as like 'reading a paper from the future, or from outer space' and as 'very, very weird' (cited in Chen 2013). The scale of the proof (more than 500 pages) and its sheer incomprehensibility, even by the standards of cutting-edge research mathematics, have so far stalled all attempts at the normal processes of confirmation and acceptance that transform a proof claim into an established proof. Although a handful of other mathematicians now profess to understand Mochizuki's work, they have had little success sharing that understanding more widely. One anonymous mathematician, quoted in *Nature*, summed up the problem:

> "Everybody who I'm aware of who's come close to this stuff is quite reasonable, but afterwards they become incapable of communicating it"... The situation, he says, reminds him of the *Monty Python* skit about a writer who jots down the world's funniest joke. Anyone who reads it dies from laughing and can never relate it to anyone else (Castelvecchi 2015, 181).

Mochizuki calls his work inter-universal Teichmüller theory, or IUTeich. He has reflected on the verification process of IUTeich in a pair of papers that comprise

a valuable resource for philosophers of mathematical practice (Mochizuki 2013, 2014).[6] Mochizuki warns that 'any attempt to study IUTeich under the expectation that the *essential thrust* of IUTeich will proceed via a similar pattern of argument to existing mathematical theories is likely to end in failure' (Mochizuki 2013, 5; all emphases Mochizuki's). Even Teichmüller theory itself is only an indirect inspiration. Nonetheless, IUTeich does echo Teichmüller theory in at least one respect—the papers in which Oswald Teichmüller laid out his theory were not immediately accepted by the mathematical community either: 'It was after several years of hard work by several mathematicians that all the arguments in these papers were considered as being sound' (Ji and Papadopoulos 2013, 128). So a lengthy gap before final community acceptance is not unusual in itself. (Note also the seven years between initial announcement and final publication of Hill, Hopkins, and Ravenel's work.) What is unusual in Mochizuki's case is that the mathematical community appears to be completely stumped.

The trouble arises from both the scale and the nature of the task required of mathematicians who wish to come to terms with Mochizuki's work. He suggests, perhaps optimistically, that 'it is quite *possible* to achieve a *reasonably rigorous understanding* of the theory within a period of *a little less than half a year*' (Mochizuki 2013, 4). But this is still a substantial investment of time. Mochizuki also notes that his work is essentially independent of the Langlands Programme (discussed in §5) (Mochizuki 2014, 10). Since this has guided so much recent work in number theory, many of the individuals most interested in the *abc* conjecture have a background that does not particularly suit them for tackling IUTeich, and should not necessarily expect to acquire techniques that would further their own projects from the six months or more of concentrated intellectual effort required. Indeed, Mochizuki stresses the incompatibility of the ideas behind IUTeich and the ideas most number theorists are familiar with: 'the most essential stumbling block lies not so much in the need for the *acquisition of new knowledge*, but rather in the need for researchers ... to *deactivate the thought patterns* that they have installed in their brains and taken for granted for so many years and then to start afresh' (Mochizuki 2014, 11 f.). He complains that

> when a researcher with a solid track record in mathematical research decides to read a mathematical paper, ... such a researcher will attempt to digest the content of the paper in as efficient a way as is possible, by *scanning* the paper for important terms and theorems so that the researcher may apply his/her vast store of expertise and deep understanding of the subject to determine just *which* of those topics of the subject that, from point of view of the researcher, have already been "digested" and "well understood" *play a key role in the paper.* ... Of course, in the case of IUTeich, a researcher who already possesses a deep understanding, as well as a solid track record in mathematical research, concerning such topics as absolute anabelian geometry, the rigidity properties of the étale theta function, and Hodge-Arakelov theory, may indeed find such "occasional nibbling" to be more than sufficient to attain a quite genuine understanding of IUTeich. In fact, however, for

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[6]For a very different application of these papers to the philosophy of mathematical practice, see (Tanswell 2016, 187 ff.).

> better or worse, *no such researcher exists* (other than myself) at
> the present time (Mochizuki 2014, 8 f.).

This is an eloquent description of conceptual incompatibility. Mochizuki is not just saying that no one is capable of understanding IUTeich; on the contrary, he is confident that the material is well within the grasp of competent research mathematicians. But he stresses that, if they are to understand IUTeich, they must sat aside their existing conceptual frameworks and build a new one from scratch. The contrast which Mochizuki draws echoes that Kuhn draws between translation as opposed to interpretation and language acquisition; incommensurability being a bar to the former, but not the latter (Kuhn 2000 [1983], 53).

If the revolution which Mochizuki has so far failed to ignite succeeds, it would appear to be strictly paraglorious in nature. He does not wish to overturn anything; rather he wishes to comprehensively supplement the existing apparatus of number theory. If IUTeich is correct, it will represent a substantial leap forward in mathematics. Mochizuki's problem is that he's trying to do it all in one go. Conversely, if an irreparable flaw is found in Mochizuki's reasoning, and IUTeich collapses, then its fall would be an inglorious revolution. In either case, IUTeich would exhibit similar conceptual incompatibility with mainstream number theory.
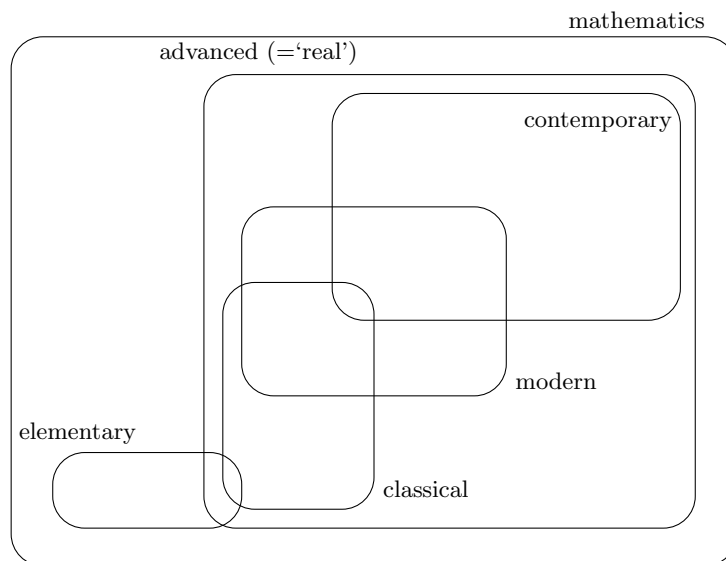
8. Classical → Modern → Contemporary

Figure 1. Correlations between the areas of mathematics: elementary, advanced, classical, modern, contemporary (after Zalamea 2012, 26; used with permission)

As a final example, I wish to shift focus from some ultimately quite small-scale revolutions (albeit ones that have attracted a fair bit of publicity) to the discipline of mathematics as a whole. The mathematician Fernando Zalamea offers the following useful periodization of research in his discipline:

Classical mathematics (midseventeenth to midnineteenth centuries): sophisticated use of the infinite (Pascal, Leibniz, Euler, Gauss);

Modern mathematics (midnineteenth to midtwentieth centuries): sophisticated use of structural and qualitative properties (Galois, Riemann, Hilbert);

Contemporary mathematics (midtwentieth century to present): sophisticated use of the properties of transference, reflection and gluing (Grothendieck, Serre, Shelah). (Zalamea 2012, 27)

The scale of mathematical research grows with each generation, such that each period attacks a broader front and produces more results than its predecessors. Zalamea graphically represents this process in the diagram reproduced as Fig. 1. The moral for the philosopher of mathematical practice is a striking one: classical and modern mathematics may be familiar enough from school or undergraduate study, but contemporary mathematics almost certainly is not. Even philosophers who take pains to reflect more than just elementary and foundational work may well be quite out of touch with the conceptual underpinnings of mathematical research conducted in their own lifetimes. Hence, as Zalamea complains, the large scale conceptual shift from modern to contemporary mathematics has gone largely unremarked by philosophers. I would contend that this shift may be understood as revolutionary. Certainly the 'properties of transference, reflection and gluing' would be impossible to articulate with only the conceptual resources available to Galois, Riemann, or Hilbert (let alone Pascal, Leibniz, Euler, or Gauss). Insofar as these are key components of contemporary mathematics, their acquisition is at least paraglorious. Furthermore, contemporary mathematics has undergone a sorites-like sequence of paraglorious revolutions of such daunting scope that the key components preserved throughout the sequence have a drastically diminished role in the new era. So much so indeed, that the whole transition might best be characterized as inglorious.

9. Conclusion

To take stock, we have seen four case studies exemplifying different classes of putative revolution in mathematics: the shift from rational to real numbers (and other cases of foundational retrofitting); shifts occasioned by the collapse of an architectural conjecture, such as the World Without End Hypothesis; shifts resulting from a rapid advance, such as IUTeich; and the collective large-scale shift that has transformed recent mathematics. The first of these is at least paraglorious and perhaps also inglorious. The second seems to be strictly inglorious whereas the third is strictly paraglorious (if successful; if unsuccessful, it would be another failed architectural conjecture). Both of these examples are comparatively small-scale and might be seen as exemplary of similar shifts in other areas of mathematics. Lastly, the shift from modern to contemporary mathematics has involved numerous conceptual innovations, each of which might be seen as paraglorious, and, when taken collectively, might represent a sorites-like inglorious revolution.

So where do Mizrahi and I agree and disagree? He disputes whether there are revolutions exhibiting (TI) in science; I have argued that such revolutions can be found in mathematics. I take it that we both agree with Crowe that it is a misconception that 'the methodology of mathematics is radically different from that of science' (Crowe 1988, 271). So we should both like for the story we tell about revolutions to hold for both science and mathematics. Of course, you don't always

get what you want—conventional wisdom might suggest that we are both wrong across the board: science and mathematics are methodologically discontinuous in part because science exhibits (inglorious) revolutions but mathematics does not. Conversely, someone might defend the contrarian stance that Mizrahi and I are both right about revolutions but wrong about the methodological continuity of science and mathematics, because (inglorious) revolutions are confined to mathematics. (This is not as absurd as it may appear—some of the strategies for minimizing the revolutionary aspects of conceptual shifts in science, such as finding common referents between theories, may not work in a field where all the referents are abstract objects.) However, I believe a more satisfactory resolution is possible.

To see how this might be accomplished, it will help to recast Mizrahi's arguments in my terminology. (TI) may be understood as saying that no revolutions are glorious. So a rebutting defeater against (TI) would be a glorious revolution. Since there are glorious revolutions, Mizrahi concludes that (TI) lacks strong inductive support. However, if we restrict (TI) to inglorious and paraglorious revolutions, then glorious revolutions no longer count as rebutting defeaters. Mizrahi does consider, and reject, a related proposal from the 'friends of (TI)': retreating to the claim that 'some episodes of scientific change exhibit TI, whereas others do not' (Mizrahi 2015a, 372). He rightly objects that such a claim would have no explanatory or predictive value. However, my proposal is more robust: rather than just exclude the anomalous cases, I have offered an independent characterization of subtypes of revolution for which (TI) still holds.[7] Hence (TI) is false as a claim about revolutions in general, as Mizrahi rightly observes. But it is true of two important subtypes: paraglorious and inglorious revolutions.[8]

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[7]The contrast is analogous to that Imre Lakatos draws between monster-barring and exception-barring (Lakatos 1976, 29).

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