

Against Defaultism and Towards Localism in the Contingency/Inevitability Conversation: Or, Why We Should Shut up About Putting-Up

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Abstract

Philosophers and historians of science have for some time now debated whether the results of current science are ‘contingent’ or ‘inevitable’. Scholars have noted that inevitabilism often enjoys the status of a presumptive default position. Consequently, contingentists are, from the outset, lumbered with the burden of proof. This is evident in the case of the inevitabilist demand that the contingentist “put up or shut up” (PUSU). This paper adds to the existing case which says that inevitabilism’s default-status is unjustified. However, whilst some have suggested that contingentism should replace inevitabilism as the default position, I argue that the contingency/inevitability (C/I) conversation should proceed *sans* default. This move is motivated largely by my claim that the C/I issue is best conceived as a ‘local’, rather than a global or universal one. The main problem with taking inevitabilism or contingentism as the default is the *globalist* nature of such a tack. Whilst localism is arguably an emergent reality of the growing C/I literature, its implications have not been fully realised. I suggest that fully and explicitly embracing localism, including the closely related move of doing away with defaults, represents the most promising way forward for the C/I conversation. In addition, I will show how these moves entail that we stop worrying about the inevitabilist PUSU demand, or more bluntly, that we shut up about putting-up.

Keywords

contingentism; inevitabilism; “put up or shut up”; defaultism; globalism; localism

1. Introduction

Are the results of science contingent or inevitable? Has the historical development of science gone just as it should have – indeed the only way it could have? Or might things have gone differently?¹ So formulated, the contingency/inevitability (C/I) issue resembles an either/or question; as though one must necessarily defend either contingentism or inevitabilism about *the whole damn thing*.² This is not the case. Some of the most significant contributions to the literature since Ian Hacking’s original framing (1999; 2000) have addressed the need to specify, along various axes, the *target* and *nature* of a C/I claim.

¹ A small but steadily growing body of literature engages these provocative questions; see especially, Soler, Trizio and Pickering’s (2015) edited volume devoted to the C/I issue.

² Thanks to Ian James Kidd for suggesting this snappy phrase.

Joseph Martin's (2013) article, which purports to address a "lingering ambiguity in the way contingency is defined" (919), is one such contribution. In it, Martin proposes a *taxonomy* of contingency claims, organised according to "the things within science considered to be contingent and the factors they are presumed to be contingent upon" (920).³ The taxonomy (927-28) differentiates between:

- (A) *Trivial contingency*, concerning the "details of scientists' everyday lives"
- (B) *Sociocultural contingency*, concerning the "social structures that constitute scientific activity"
- (C) *Methodological contingency*, concerning the experimental techniques, practices, instrumentation, etc., which constitute the ways in which we *do* science
- (D) *Interpretive contingency*, or, the way scientists "expound data in order to fill theoretical gaps"
- (E) *Theoretical contingency*, which says that "scientific theories themselves and the claims they make about the world might have been different"⁴

Another layer of complexity is added to the possibility-space of C/I claims by considering the factors upon which each of A through E are purported to be contingent, including "everyday events, sociocultural contexts, methods, interpretations, theories" (929). To deny the contingency of any of A-E is to adopt the corresponding *inevitalist* stance.

E-type claims – the most controversial kind of contingentism – are our present concern. We can agree they are the most controversial variety of claim, as those inclined towards inevitabilism might happily admit various combinations of A-D, while objecting to E; our experimental methods, or the applications of our science might well be contingent, but any successful science would refer to genes, electrons, the constancy of the speed of light, etc. Unsurprisingly, E-type claims enjoy special attention from philosophers interested in the C/I issue. Several landmark papers in the C/I literature specifically address E-type claims, often grappling with the formulation which began the present essay: "Are the results of science contingent or inevitable?"⁵ We can roughly approximate Martin's "theoretical contingency" category with claims about "scientific results", which Léna Soler, an important commentator on the C/I issue, takes to include: whole theories; theoretical laws; theoretical entities; the values of physical magnitudes, and; experimental facts (2015b, 47).

When discussing whether these aspects of science could have been different given an alternative historical trajectory, the weight of presumption tends to fall on the side of

³ Martin's goal is to show, contra Hacking's original framing, that C/I is a *what* question, rather than a *how much* question. He begins with John Beatty's (2006) useful distinction between contingency as 'causal dependence', and contingency as unpredictability, or contingency *per se*. Contingency claims have tended to refer to causal-dependence upon historical antecedents, and so invoke a sensitivity of the historical development of science to initial conditions. Meanwhile, the *per se* variety invokes stochasticity/unpredictability in the historical process. Claims of the latter kind are less common, though certain of Andrew Pickering's arguments (1984; 1995) provide notable exceptions. Hence, our focus will be on the causal-dependence variety.

⁴ I have added the A-E letters for ease of reference; they do not appear in Martin (2013).

⁵ This framing provides the title of articles by Soler (2008a) and Kinzel (2015), both published in this journal.

inevitabilism. Contingentism is saddled with the burden of proof, whilst inevitabilism enjoys default-status. Several scholars have identified and deplored this state of affairs (Soler, 2015b; Kidd, 2016), and in section 2 I build upon their foundation, offering a range of arguments toward the conclusion that inevitabilism's default-status is unjustified. In section 3, I tackle the question of whether a default position can *ever* be justified. This seems like a strange question, as 'default position' can imply a certain lack of explicit justification or argumentation. Does not a default position, then, cease to be a 'default' once we provide such a positive case? In tackling this issue I separate out two closely related issues at the heart of the C/I issue as it has played out in the literature: *defaultism* and *globalism*. For our purposes, the former consists of asserting a contingentist or inevitabilist position *from the outset*, or without argument, whilst the latter – treated more fully in section 4 – consists in asserting a monolithic position (C or I) regarding the 'results of science' generally. Proceeding in either mode, I will argue, is unacceptable. As such, I reject Ian James Kidd's (2016, 15) suggestion that, having disposed with the inevitabilist default, we adopt contingentism as a general position about the results of science. One can debate whether or not Kidd is guilty of defaultism – the issue is not straightforward. In any case, we can reject his move on the basis of its explicit *globalism*. These two 'isms' are closely related, and often manifest together. As we shall see, globalist positions, in a sense, all allow defaultism in by the back door.

Section 4 makes the case for the unacceptability of globalism with respect to the C/I issue, by drawing on recent contributions to the literature on scientific realism/anti-realism. I here characterise and challenge methodological globalism, and make the case for *methodological localism* in the C/I conversation. 'Science' – including the 'results' thereof – is a far too complex and heterogeneous domain for us to expect that either contingentism or inevitabilism will apply globally, and we should not proceed as if this is our expectation. The C/I conversation must take place at a much more localised level, concerning particular fields of research, theories, or particular ontological posits. Whilst the growing number of zoomed-in case studies in the C/I literature suggest that we are collectively embracing a localist methodology, I maintain that localism's implications for the nature and framing of the C/I problematic have yet to be fully realised, and as such, indicators of globalist thinking are detectable in the literature.

The latter will be particularly evident in section 5, where I analyse the so-called "put up or shut up" (PUSU) demand, bequeathed to us by Ian Hacking. Faced with contingentist critique, Hacking explains, the inevitabilist is inclined to ask their interlocutor: if the results of science are indeed contingent and thus could have been otherwise, present a convincing example of an alternative science, meaningfully different from our actual science; put up or shut up! (1999, 79, 89; 2000, 70). Using Soler's (2015b, 55-56) comprehensive reconstruction of the PUSU demand, I will highlight its reliance on the default-status of inevitabilism – a status put under severe challenge in section 2. Revisiting Hacking's formulation of the PUSU demand will demonstrate its embeddedness within *methodological globalism*, which comes under attack in section 4. Doing away with defaults renders the inevitabilist PUSU demand, in most of its instantiations, a non-starter, whilst fully and consciously embracing localism means that 'the PUSU demand' is replaced with indefinitely many PUSU demands – as many as there are potential targets for C/I claims. At this point, the distinctive argumentative and rhetorical

force of the PUSU demand is almost entirely lost. Methodological localism renders any more globalist version of PUSU a non-issue.

2. Inevitabilism: An undeserving default

On the question of whether the results of science are contingent or inevitable, Soler has observed that, “for many people, inevitabilism is, *as a matter of fact*, the default position, or, in other words, a position that is intuitively (and often tacitly) assumed.” Furthermore, due to inevitabilism’s supposed *prima facie* plausibility, “no need is felt of any quest for explicit justification insofar as no plausible alternative comes to threaten it. Accordingly, it is assumed that the burden of proof lies with contingentists” (2015b, 56). This, despite the relative dearth of self-avowed inevitabilists in the wild; physicists Steven Weinberg and Sheldon Glashow represent the most frequently cited of a small band of outspoken advocates (e.g. Soler, 2008a, 225-227). Few philosophers of science defend frank and explicit inevitabilist positions in such terms. Nevertheless, Soler explains that “any philosopher of science interested in the contingentist/inevitabilist issue quickly experiences that the inevitabilist instinct is deeply entrenched and widely shared, including within philosophical, sociological, and historical analysts [*sic*] of science” (2015b, 54).

Certainly, a number of historians of science have pronounced on the influence of inevitabilist thinking within their field.⁶ Greg Radick has for some time been pursuing counterfactual-historical work on biometry and Mendelism during the formative years of genetical science at the turn of last century (2005; 2016; Jamieson & Radick, 2013; 2017). He laments that, when it comes to imagining the possibilities for what a science of biological inheritance *might* have looked like, the “weight of presumption has traditionally come down so much on the side of gene inevitabilism, ... that gene contingentism has hardly had a look” (2005, 26). Hasok Chang presents a similar situation: the so-called ‘chemical revolution’, during which Lavoisier’s oxygen theory is supposed to have rapidly and comprehensively displaced earlier phlogistonist chemistries. While most scholars depict this episode as an inevitable victory for Lavoisierian chemistry over an inferior rival, Chang maintains that phlogiston theory had much going for it, and as such things could – perhaps *should* – have gone differently (2009; 2010; 2012). Both Radick and Chang identify, and seek to contest, a body of historiography which simultaneously assumes, and licenses belief in, the inevitability of the triumph of one scientific alternative over its competitor(s).

Moving from its historiography to science itself, Chang elsewhere diagnoses a “dominant intuition among scientists” that contingency in science is something *harmful*. He connects this to a “strongly monist scientific education that many of us have received”, meaning that “contingency is either feared as a threat to scientific knowledge or relished in a rebellious spirit” (2015, 362).⁷ The scientific and historiographical manifestations of the “inevitabilist instinct” are surely mutually reinforcing. A science shaped by a pervasive inevitabilist instinct will deeply influence the way in which that science’s history is written,

⁶ John Henry (2008, 557), for example, explores how the historiographical practices of both positivists and social constructionists, in different ways, “favour the view that modern science will turn out the same, come what may.”

⁷ Chang’s remarks echo Soler’s diagnosis of a “monist regime” operative in science, past and present, and about which we shall hear more in section 5.

and vice versa.⁸ This, as several commentators have bemoaned, is where we stand. We might hope that it is a state of affairs well justified; that there is good reason for the superior level of *prima facie* plausibility granted to inevitabilism concerning the results of science. We hope so, because the consequences run far and deep, in science, its historiography, and in the C/I conversation where contingentism is widely saddled with the burden of proof. The observations of Soler which began this section do not, however, inspire much optimism.

Part of the worry about inevitabilism's default-status is that explicit justification is lacking; it rests instead on an ill-defined feeling or "sense" (Kidd, 2016, 16). In the interest of a charitable discussion, we will temporarily afford inevitabilists the benefit of the doubt by examining the justifications they *might* put forward to ground the epistemically-privileged status of their position, even if in fact they rarely do. There appear to be at least three different potential justifications which might conceivably bestow upon inevitabilism an appreciable *prima facie* plausibility. Without claiming exhaustiveness, I shall briefly outline these options, and suggest why each fails to convincingly justify inevitabilism's default-status in the C/I conversation.

2.1. The pull-of-reality justification

We begin with the most obvious justification; what I am calling the 'pull-of-reality' (POR).⁹ It goes something like this: *The world just is a certain way, and it is the task of science to describe it. The world really is the way our best theories describe it, and the theoretical objects we posit really are out there. Science aims at uncovering truth, and if we do it well we will inevitably bump into nature's true structure(s), sooner or later.* For an example of a POR-inevitalist in the wild, we need look no further than the progenitor of the C/I conversation, Ian Hacking, who states frankly:

I do not think the answers to live questions are contingent upon social considerations ... I am saying, against my constructionist friends, that answers to live questions about the natural world have nothing to do with us (2000, 70).

Nothing to do with 'us,' and everything, it seems, to do with 'the natural world'.

As with all strong realist positions, POR-inevitalism is "undoubtedly risky to claim, and very difficult to argue" (Soler, 2008a, 226). If one holds that all or most of our *current* science is inevitable (and will endure) given that it latches onto reality in some way, opponents might swiftly – and in the spirit of Larry Laudan's (1981) "pessimistic meta-induction" – point to a scientific past littered with discarded theories, positing entities and processes we now consider not to exist in nature, but which were (sometimes greatly) successful in their own times, on their own terms. Why assume, then, that the current results of science will stick around indefinitely? The scientific theories which presently dominate may well appear to give

⁸ Especially if, as was the case in our field until relatively recently, those *doing* science and those *writing its history* are the same people.

⁹ Many commentators have noted the affinity between realism on the one hand, and inevitabilism on the other. The relationship, though, appears to be "a matter not of logic but of psychology"; Radick (2005, 47).

the best account of their respective domains of nature, compared with any past attempt or present minority alternative.¹⁰ But as Kyle Stanford suggests in outlining his problem of unconceived alternatives, “present theorists are no better able to exhaust the space of serious, well-confirmed possible theoretical explanations of the phenomena than past theorists have turned out to be.” Thus, ‘better’ alternatives may be out there in principle, as they demonstrably were for past thinkers – we just have not conceived of them yet (2006, 4). The above meta-inductions from science’s long-run development are not necessarily *fatal* to the POR-inevitabilist, but they do put pressure on the legitimacy of resting inevitabilism’s default-status upon a strong realism which is, at present, far from established.¹¹

One might instead hold, like physicist Steven Weinberg (1993), that whilst much current scientific knowledge might not latch onto the realities of nature, *eventually*, we will converge on a *final theory* which does. However, for this to be so, either scientific decisions must be made on the basis of the highly problematic notion of closeness/approximation to truth, or else, in Andrew Pickering’s words, the POR-inevitabilist must hold that:

the situatedness and path dependence [of scientific practice] ... somehow (and sometimes) wash out, so that scientific knowledge eventually converges on a mirroring relation to nature, independently of where scientific practice starts and whatever direction it sets off in. I can only note that nothing in my analysis points to such an erasure of situatedness and path dependence (1995, 185, note 6).

A huge challenge for the final theory POR-inevitabilist is to show how the unfathomably varied systems of science, grounded in disparate material and conceptual practices and concrete technical knowledges, put towards a plethora of practical and intellectual goals, and employing an array of virtues for measuring success in these tasks, could possibly (never mind *inevitably*) converge on a true (whatever this might mean) “mirroring relation to nature”. I do not assert the impossibility of this task; merely that much work is to be done for POR-inevitabilism to *legitimately* command substantial initial credibility.¹²

2.2. *The uniqueness-rationality justification*

¹⁰ Perhaps, though, the perceived superiority of actual science is itself contingent upon its historical dominance. The very data available to us for adjudicating alternatives are laden with the *actual* theory that has happened to be dominant. Consequently, any theory which *happened* to become dominant will come to *appear* inevitable given its superior (but unsurprising) ability to account for the data (which were collected/constructed under its own dominion). See Radick (2005, 40-44); also, Hacking (1992); Kidd (2015, 16).

¹¹ One might also note the recent challenges made to the very notion of adopting a ‘global’ realism of the kind POR-inevitabilism seemingly rests upon; see section 4.

¹² Add to this: the extrapolation into the future involved in the Weinbergian ‘final theory’ claim is without justification, and may fairly be characterised as based on ‘faith’ alone.

A thoroughgoing commitment to *realism* need not, though, be the grounding for inevitabilism.¹³ Talk of ‘rationality’, for instance, can serve in its place. Restricting their attention to local scientific decisions and consensuses – e.g., regarding whether a significant result has been achieved – Soler (2008a, 223) and Kinzel (2014; 2015), have both pointed to what I call ‘uniqueness-rationality’ (UR) as a potential grounding of inevitabilism. Kinzel writes:

Regarding a specific scientific result we can then ask whether it *had to* emerge or be accepted *given the reliable operation of scientific rationality and methodology*, or whether the processes of scientific consensus formation were such as to allow for *more than one rationally acceptable outcome*. The inevitabilist embraces the first option and the contingentist the second (2014, 18-19, my emphasis).¹⁴

The above says nothing of scientific realism. Inevitabilists about scientific closure ground their position in the assumption that “rational considerations and the available evidence determine a unique choice, such that local factors never become relevant for the decision at issue.” Closure-contingentists do *not* argue that scientific decisions are irrational, but rather that “multiple outcomes will typically be rationally acceptable”, and hence local factors must play a role in selecting a unique outcome (104).¹⁵ Harry Collins’ (1985) arguments concerning the purported detection of gravity waves in the 1970s represent a contingentism of this variety. Allan Franklin’s response (1994) exemplifies the corresponding UR-inevitabilist stance.

For proponents of UR-inevitabilism such as Franklin to establish its credibility they must, according to Kinzel, show that the kind of ‘epistemic flexibility’ granted by confirmational, or ‘Duhemian’, holism is *not* genuinely available to scientists during episodes of scientific closure. Indeed, successfully rejecting this holism “constitutes a minimal requirement for inevitabilism about scientific closure to be considered a plausible option” (2014, 104). Kinzel shows, however, that the UR-inevitabilist’s most promising lines of challenge all beg the question against confirmational holism, as each requires or assumes the existence of some “stable background, a given that is not subject to possible transformation or modification in the face of recalcitrant evidence” (105). Yet it is this very notion which confirmational holism denies. Again, my suggestion is not that UR-inevitabilists will fail indefinitely to establish plausible grounds for their position; I merely note the present lack of such grounds.

2.3. *The in-the-air justification*

¹³ Radick (2005, 23-25), points out one may occupy any of the four positions entailed by adopting inevitabilism *or* contingentism on the one hand, and realism *or* anti-realism on the other. Indeed, Soler (2008a, 222), notes that many *contingentist* positions are “realistically-framed” in that they aim to display contingency within a perceived relation between ‘how the world is’, and ‘what science says about the world’, whilst implicitly granting the cogency of such a relation.

¹⁴ Kinzel’s analysis is set out comprehensively in her unpublished doctoral thesis (2014). Thanks to Katherina for sharing this with me.

¹⁵ Though, as Kidd has emphasised to me, it is well-established that our bias-ridden epistemic practices fall short of idealised standards of ‘rational’ cognitive performance. That’s not a slight at ‘scientific rationality’; it is merely a recognition of our limited – though impressive – cognitive capabilities as situated human agents.

A third potential grounding for inevitabilism invokes the necessity of certain scientific developments, given wider historical/social/cultural trends; one might claim that a particular development was ‘in the air’ (ITA).¹⁶ To some readers, this will strike as the most obviously problematic of the three groundings for inevitabilism, given that ITA-inevitabilism seems to espouse an unapologetic overdeterminism about history. According to Steve Fuller, historical-overdeterminists believe that “Y had to happen, but it need not have happened via X”. Underdeterminists, meanwhile, maintain that “X need not have happened, but once it did Y had to happen” (2008, 581).

POR- and UR-inevitabilism are both also overdeterministic. However – and despite their shortcomings – their overdeterminism at least follows from considerations clearly befitting an *inevitabilist* position; be it the ‘pull-of-reality’, or the purported uniqueness of rational scientific outcomes. In ITA-inevitabilism, however, a decisive role is bestowed upon social and cultural considerations; an irony since contingentism and constructivism have been derided by some traditional thinkers for just this reason. Furthermore, sociocultural considerations are widely considered *contingent*, as they ‘could have been different’. ITA-inevitabilists can either go along with this, claiming outcomes are *inevitable*, *given* such-and-such (contingent) sociocultural factors, or they can hold that these factors are not contingent, but are in fact inevitable. The former move results in a rather vapid position, labelling as *inevitable* that which most would label *contingent*.¹⁷ Opting for the latter takes us from a claim about inevitability in science’s development to a much broader one concerning human history generally – a move we must not make lightly.

3. New default or no default?

The basis of inevitabilism’s default-status in the C/I conversation often appears to be little more than instinct or intuition. We have now seen that the prospects of an explicit, reasoned case for the *prima facie* plausibility of inevitabilism are rather bleak at present. So far, then, it appears I am open to the implication that, *should* sufficiently good reasons for adopting inevitabilism (or contingentism!) as default be forthcoming, *then* inevitabilism (or contingentism) could *deservedly* occupy the default position in the conversation over the inevitability/contingency of the results of science, and hence the burden of proof would fairly and reasonably fall upon those challenging the default. I will now consider whether this is a desirable state of affairs for the C/I conversation going forward.

¹⁶ Darwin’s theory of evolution by natural selection is a classic subject of ITA-inevitabilism. Aside from the question of whether natural selection is ‘real’ – ITA-inevitabilism is neutral on the realism issue – some historians have argued that its emphasis upon the competitive struggle between individuals reflects the values of the rising industrial classes of Victorian England, to which Darwin belonged. As such, natural selection was a product of this time and place, and if not Darwin, then someone within a similar milieu would inevitably have forwarded similar ideas, which were ‘in-the-air’. For details and criticisms, see Radick (2003) and Bowler (2008; 2013).

¹⁷ I have no problem as such with the notion of qualifying inevitabilist claims with various conditionals. However, to move from the fact that one can reliably arrive at inevitabilism given enough conditionals, to the global *prima facie* plausibility of inevitabilism, seems disingenuous. Similar issues arise in section 4.

Let us agree, based on the foregoing, that no good case for inevitabilism-as-default has been presented in the literature, and as such, that inevitabilism is undeserving of the default-status it widely enjoys. An obvious next move would be to argue that contingentism take inevitabilism's place. Indeed, we can read Kidd, in his (2016) essay 'Inevitability, contingency, and epistemic humility', as advocating just such a move. After disposing with inevitabilism, he proclaims that the "real" question facing us is not whether the results of science are contingent or inevitable, but rather, "how contingentist ought we be?" (2016, 15). His case is built upon the notions of epistemic 'humility' and 'hubris' as developed by David Cooper (2002). Kidd argues compellingly that inevitabilist stances are epistemically hubristic and should thus be rejected. Any kind of interesting inevitabilism, he explains, must presuppose that its claims regarding the necessity of scientific results "can be established in a sufficiently warranted manner." "Otherwise", he goes on, "inevitabilism can only gesture to, but never actually assert, the inevitability of whichever scientific results interest them ... and this is a poor sort of inevitabilism." This presupposition, Kidd points out, rests upon hubristic estimations of our epistemic capabilities. Exhibiting the inevitability of scientific results requires also establishing the inevitability of the relevant "questions, assumptions, concepts, methods, practices, disciplinary cultures, institutional structures and so on", which facilitated the actualisation of the results (2016, 13). A vast body of historical and sociological scholarship, however, overwhelmingly points to the role of chance and historical contingency in the emergence of just these conditions; it is hubris on the inevitabilist's part, Kidd contends, to imagine that they can demonstrate otherwise. However, to bite the bullet and maintain that a scientific result was inevitable *given* these various contingent conditionals is, for Kidd, to simply collapse into contingentism.

The upshot is that inevitabilism – so widely afforded default-status – is epistemically hubristic and should thus be rejected.¹⁸ Our conclusions so far are similar, though arrived at by different routes. Kidd reasons that inevitabilists implicitly presuppose that their claims about the results of science are able to be "established in a sufficiently warranted manner." This is not the case, according to Kidd, and so inevitabilisms are hubristic, and ought to be dismissed. I think it is reasonable for Kidd to *hope* that inevitabilists would try – or at least recognise the need – to 'establish' their claims. However, we are perhaps destined for disappointment, given the pervasive and entrenched inevitabilism-as-default supposition in the C/I discourse. For what it's worth, I think it is more likely that inevitabilists tend *not* to see the need to establish their claims, which appear to them to be *self-evident* and *prima facie* highly plausible (a presumption which section 2 invalidated).¹⁹

Having dethroned inevitabilism for reasons of epistemic hubris, Kidd proposes that in its place we crown *contingentism*, a position which he argues exemplifies great epistemic *humility*. For starters, the contingentist stance invites us to "critically enquire into the grounds

¹⁸ Kidd also argues that the inevitabilist's PUSU demand is hubristic. I will delay discussion of this until section 5.

¹⁹ Recall Soler's remarks (2015b, 56), which we heard in section 2: "for many people, inevitabilism is, *as a matter of fact*, the default position, or, in other words, a position that is intuitively (and often tacitly) assumed." Furthermore, due to inevitabilism's supposed *prima facie* plausibility, "*no need is felt of any quest for explicit justification* insofar as no plausible alternative comes to threaten it" (my emphasis).

of our confidence when making certain claims ... about the inevitability of certain scientific results, and the concepts, methods, practices, and so on upon which they are contingent” (Kidd, 2016, 16). Secondly, we are better positioned to “identify and take seriously alternative epistemic possibilities”, including in the historical development of scientific enquiry; thus, we will better appreciate the “complexity and contingency” of our knowledge-making practices (16-17). For Kidd, all of this necessitates we do the epistemically humble thing and adopt contingentism, allowing us to move away from the ‘contingentism *versus* inevitabilism’ question, and consider instead the variety, strength, and degree of our contingentism.

I will suggest here an alternative move. Instead of crowning contingentism, we should do away with defaultism. That is, we should not adopt any one position – inevitabilism *nor* contingentism – from the outset. In this, am I simply urging good old-fashioned agnosticism?²⁰ In one sense, yes, but it is a very particular sense. Before fleshing this out, I should first note the potential worry over whether it is fair and reasonable to read Kidd as advocating contingentism-as-*default*, in that doing so implicitly equates his position with that of inevitabilists who uncritically assert the default-status of their own position on the basis of an instinct or feeling.²¹

The *Oxford English Dictionary* defines ‘default’ (*noun*) as: “A usual, customary, or standard option, course of action, etc., *esp.* one adopted or reverted to in the absence of conscious choice or viable alternatives.”²² Kidd’s is certainly a conscious choice, and he clearly does not see inevitabilism as a viable alternative; a claim towards which he provides sustained and compelling arguments. In this sense, it perhaps is unfair to equate his position with the inevitabilism-as-default orthodoxy criticised in section 2, especially as Kidd explicitly forwards reasoned arguments for the superiority of contingentism, rather than falling back on an instinct, sense or feeling. Both positions, though, are of a kind in a more general sense. What I wish to focus on here, more than whether the label of ‘default’ is fitting, is that both the inevitabilism-as-default orthodoxy, and Kidd’s contingentism-as-default alternative – however thoughtfully argued they might be – are (relatively) *global* positions. They each suggest, from the outset, that we adopt the same position, be it inevitabilism or contingentism, about the results of science generally.²³ The parenthetical ‘relatively’ serves to acknowledge this, along with the sliding scale that exists between the ‘global’ and the ‘local’. From the perspective of *methodological localism*, the ‘default’ label is fitting for Kidd’s position, because by taking a stance on the results of science generally, it implicitly takes a stance on individual instances – particular scientific results – prior to careful historical-philosophical exploration of such instances. It is thus – like the inevitabilism-as-default orthodoxy – a methodologically *globalist* position, and an instance of *defaultism*.

²⁰ In doing so I would be in good company. Elliott Sober (2005) decries unjustified defaults and champions agnosticism in the debate over the employment of Morgan’s Canon in research on animal cognition, whilst William Bausman and Marta Halina (2018) make similar arguments concerning what they call “pseudo-null hypotheses” in community ecology and comparative psychology.

²¹ I thank Soler for drawing my attention to this important worry.

²² Entry on “default, n.” in Oxford English Dictionary Online (2018).

²³ Going back to Martin’s scheme, it seems likely that Kidd would defend contingentism concerning A-D. If, then, he also proposes a global contingentism about the results of science (E-type), then his is indeed a contingentism about ‘the whole damn thing’.

The next section will outline the alternative, methodologically *localist* approach to the C/I problem. Whilst C/I scholars have embraced localism to a certain extent, they have not, I suggest, gone far enough. In a sense, the localist approach entails initial agnosticism at the level of ‘the results of science’, taken together. This is the ‘very particular sense’ in which I said I am urging agnosticism. What I am suggesting is not simply that we do away with default positions, but that we do away with (relatively) *global* defaults pertaining to the ‘results of science’ generally. Instead, we must go *local*.

This change in tack suggests we should not worry ourselves over the merits of various arguments for this or that global default; they are simply not part of the C/I landscape once we embrace methodological localism. Before closing the door on this issue, then, I will note two things. Firstly, as reasons for a (relatively) global default go, Kidd’s seem to be rather good ones. Humility is certainly an admirable virtue, and hubris a vice. But there are many ways to be humble. Methodological localism, as we shall see, is one of them. Secondly, why, if I am not interested in arguing over the justification of default positions, did I spend section 2 challenging the potential justifications of inevitabilism-as-default? Why did I not save myself, and the reader, some time, and begin with my case for going local? My reasons for proceeding as I have are twofold, and intimately connected.

First off, uncritically taking inevitabilism as the default position in the C/I conversation is the orthodoxy. My sustained criticism of the orthodoxy will, I hope, be of use in motivating a collective move away from it. Relatedly, I do not want the tendency to unthinkingly install inevitabilism as default to ‘follow’ us when, in the next section, we challenge another orthodoxy – methodological globalism – and ‘go local’. That is, without first systematically challenging the legitimacy of inevitabilism’s claim to default-status, there might have been a danger that inevitabilism would serve as a local default in each particular instance, as we set out to examine the results of science case-by-case. Without further ado, let us move on to my case for going local in the C/I conversation.

4. Going local

Taxonomies of contingency claims, such as Martin’s (2013) which we met in the introduction, make it abundantly clear that the C/I issue is not as simple as debating whether ‘science’ is contingent *or* inevitable; C/I claims can be made about lots of different *aspects* of science.²⁴ Indeed, C/I claims *have been* made about lots of different aspects of science.²⁵ An emphasis on

²⁴ Martin’s taxonomising efforts have been fruitfully elaborated upon by Kinzel (2015, 62-65). Her four-dimensional scheme retains the need to specify a C/I claim’s *target*, whilst also addressing issues such as: the nature of the alternative to actual science that is under consideration (is it only a *logically* possible alternative, a *genuine historical alternative*, etc.); how we assess alternatives (are they equally/unequally successful? For what purposes? etc.), and; how alternatives are related to one another (are they logically, or ontologically incompatible? Or incommensurable? etc.).

²⁵ See Soler (2015a, 7-8), for a distillation of the “highly diversified cases” discussed in *Science as it could have been*. Case-studies include, e.g., Michel Bitbol and Claire Petitmengin (2015) on the “(quasi-) disappearance of introspection in psychology”, and Yves Gingras (2015) on the discovery of electron diffraction.

the local, then, seems to be an emergent reality in the C/I literature. Nevertheless, I will suggest that we are in need of some clarity, methodologically-speaking, on where this leaves the C/I conversation.

Martin (2013, 919) professed to address a “lingering ambiguity” in defining the concepts at the heart of the C/I issue. In a sense, I want to do similar. Recall Martin’s characterisation of *theoretical contingency* (E-type) claims: “scientific theories themselves and the claims they make about the world might have been different” (928). Notice that it is not made entirely clear whether E-type claims pertain to “scientific theories” generally (i.e., a C/I claim about all/most scientific theories), or whether Martin is categorising together all claims which are about *particular* instances of scientific theories being contingent/inevitable. In other words, it is unclear whether this category of claims covers (relatively) globalist theses about the contingency/inevitability of *all or most* scientific theories, or rather localist theses pertaining to particular scientific theories, without the presumption that the conclusion in each case can be extended beyond the confines of that case. Elsewhere, Soler has observed that “one can be contingentist about a certain kind of element [of science] but inevitabilist about another kind” (2015a, 7). Such remarks are commonplace and suggest a certain openness to localism. The talk of *kinds* of elements - examples of which might include physical constants, natural laws, fundamental equations, theoretical entities, to name but a few – does, however, somewhat temper the localist sentiment. Soler seems to be talking in terms of contingentist/inevitabilist theses about ‘kinds’ such as these, rather than theses about particular tokens of these kinds. It would be entirely consistent, however, for us to be contingentist about one token of the theoretical entity ‘kind’ – say, genes in biology – but inevitabilist about another token of that kind – say, electrons in physics.²⁶ Perhaps I am merely nit-picking, and possibly the localist interpretation of Martin’s E-type category is implicit. Perhaps Soler would be happy to modify her remark so that it speaks about *particular elements* rather than *kinds of elements*. We can and should be explicit in these matters. To see why, and to clarify what I mean by localism/globalism, we will take another detour outside of the C/I realm.

4.1. Going local in the scientific realism debate

Traditionally, scientific realist and anti-realist positions have been characterised as *global*, rather than *local* ones.²⁷ Scientific realism, we often hear, involves belief in the (approximate) truth of our best scientific theories, whilst anti-realism holds that such belief is unwarranted. Note the globalism at play on each side of the dichotomy: a single epistemic attitude is adopted towards theories throughout all those domains we call ‘science’.²⁸ This globalism is characteristic of many of the realism-relevant stances developed in the literature. Entity realism

²⁶ Relatedly, Catherine Allamel-Raffin and Jean-Luc Gangloff wonder whether certain scientific sub-fields are more amenable to contingentist interpretation than others (2015, 105). If so, the risk involved in extrapolating C/I conclusions beyond disciplinary boundaries is heightened.

²⁷ Globalism is sometimes referred to as *universalism*. ‘Wholesale vs. retail’ represents an alternative term for the global/local dichotomy. Confusingly, Mohamed Elsamahi (2005) uses ‘localized’ realism to denote what others call ‘selective’ realism. I stick to the meaning of ‘local’ realism in Asay (2016) and Henderson (2018).

²⁸ Sometimes with the qualification that the science must be ‘mature’. Even if we are minimally charitable, and grant that *only* physics is ‘mature’, this still captures a huge variety of fields/sub-disciplines.

recommends belief in the theoretical entities which appear *in our best science*; structural realism recommends belief in the ‘structures’ *in our best science*; constructive empiricism recommends agnosticism concerning the reality of unobservables (even) *in our best science*, and so on. As Jamin Asay (2016) puts it, these are all *first-order* globalist stances on the realism issue. The position adopted covers many or most instances of a ‘kind of element’ of science. Furthermore, attempts to support these stances are most often *methodologically* globalist. That is, the arguments provided are presumed to apply for all (mature, successful) science, even if made on the basis of one or a small handful of particular cases from specific scientific domains. As Asay notes, the central arguments for and against realism – i.e., Putnam’s (1975) no miracles argument, and Laudan’s (1981) pessimistic meta-induction – are methodologically globalist. The former addresses the “success of science in general”, whilst the latter moves from *particular* historical cases towards a *general* philosophical anti-realist thesis. Proceeding in either of these modes assumes either the general applicability of one’s arguments, or else some fundamental similarity of unexamined to examined cases.

Riding a small wave of momentum in the literature, Asay (2016) defends an alternative *localist* approach to the realism issue (see Magnus & Callendar, 2004; Saatsi, 2010; 2017; Henderson, 2018). This change of tack does not entail disposing with the various positions developed in the realism debate’s long history – entity realism, structural realism, constructive empiricism, etc. They still have a place in the localist landscape, just without the globalist expectation that any one doctrine is applicable to all science. One must examine each scientific domain on its own merits and decide which ‘realism-relevant stance’ is most appropriate to adopt *in that domain*. How zoomed-in these domains should be is an open, empirical question. It may turn out that the same stance can be adopted regarding an entire discipline, or only at the level of an individual theory, or at some even lower level. To approach the issue this way is to adopt *methodological* localism. If it turns out that, say, structural realism provides the most appropriate account of fundamental physics, whilst entity realism captures best some other domain, in neurobiology perhaps (the logical space of possible combinations is vast), then one will have arrived at a *first-order* localism. In Asay’s words:

The best way to come to the truth about scientific realism is to explore each branch of science individually on its own metaphysical and epistemological terms. The result of such enquiries could be a first-order globalism ... Even if this turns out to be the case, we will have arrived at that fact more honestly and appropriately if we adopt methodological localism (2016).

It is in this sense that, as I suggested in the previous section, to adopt methodological localism is one way to be humble. In advocating this methodological approach to the C/I issue, we can draw on the reasons Asay provides for doing so in the realism debate. The most relevant for our purposes, and the most potent by Asay’s reckoning, is the “rampant diversity” of science. The various systems of practice we call sciences are extraordinarily diverse in their aims, methodologies, material and experimental cultures, and notably the *kinds of things* they study.²⁹ Our ‘mature scientific theories’ are products of this incredible diversity of practices,

²⁹ For an extreme contrast, consider the differences between ‘historical’ and ‘experimental’ sciences, along the dimensions listed.

and they attempt to account for a baffling array of different kinds of phenomena. The search for a single, unified scientific method has long been abandoned, as has that for anything beyond a sociological criterion of what constitutes ‘science’. We don’t, then, find ourselves in a very strong position to assert *from the outset* – that is, in a *defaultist* manner – that we should adopt the same epistemological stance *globally*, i.e., towards all ‘mature’/‘successful’ scientific theories.

4.2. Going (even more) local in the C/I conversation

‘Rampant diversity’ in the sciences is not news to those invested in the C/I issue. It is acknowledged implicitly by those, like Soler, who observe that “[a]n indefinitely wide variety of targets at different scales and in different scientific fields can be the object of contingentist/inevabilist claims” (2015a, 7; see also 2008a, 222), and explicitly by Allamel-Raffin and Gangloff (2015), for whom it is possible that contingentism accounts for certain scientific sub-fields better than others (see my note 26). Indeed, the diversity argument arguably applies even more strongly in the C/I case than in the realism case; claims in the latter realm tend to focus only on theoretical entities, whereas C/I claims, as we saw in Martin’s taxonomy, can be directed at almost *any* aspect of science, thus greatly increasing the diversity of targets. Even restricting ourselves to claims about the ‘results of science,’ – which include among other things whole theories, theoretical laws, theoretical entities, the values of physical magnitudes, and experimental results (Soler, 2015b, 47) – the C/I-scholar is still arguably presented with greater diversity of potential targets than the realist/anti-realist.

But we can add further to the case for methodological localism. Recall Asay’s admission that in the realism case, it is possible that methodological localism will end in a first-order globalism. Is an eventual first-order globalism similarly possible in the C/I case? I am made sceptical by the conceptual nature of contingency and inevitability – by contrast with that of realism and anti-realism. Take temporality: realist/anti-realist claims typically refer to a particular, static time-slice (often, *now*). Confronted with a claim that this or that element of science is/is not real, we would not usually ask when? Given what historical conditions? Realists ordinarily want to assert the *permanent* and *enduring* reality of their chosen target. Contingency claims, however, are *inherently* historical and, thus, spatiotemporally situated. We say that X is contingent *upon* such and such historical conditions. What about claims of inevitability? Many inevitabilists – especially POR-inevabilists – want to assert the inevitability of their chosen target *tout court*. Toward rendering inevitabilist claims more empirically tractable, Ronald Giere has suggested an historicised understanding of inevitability.³⁰ On this view, a scientific result or consensus “becomes inevitable when there are no remaining plausible contingencies to divert it” (2015, 188). Thus, a scientific result which was contingent at a given moment in history, might over time *become* inevitable, or more provocatively, “a result that was once inevitable might later be overturned” (2015, 188; see also Allamel-Raffin & Gangloff, 2015, 106-7; Gingras, 2015). On Giere’s picture, not only the *whats* but also the *whens* (and there are indefinitely many) greatly diversify the possibility-

³⁰ More approaches to making inevitabilism tractable will be discussed in section 6.

space of inevitability, as well as contingency claims.³¹ If individual cases are temporally situated in the strong sense that adjusting the time-conditional could alter our conclusion as to whether the same scientific result was contingent/inevitable, we can hardly expect a first-order globalism about scientific results to emerge!

If Giere's historicised understanding of inevitability should prove a little exotic for some tastes, we can recall as well that much besides temporality requires qualification. Kidd explains that inevitabilist claims implicitly hold constant relevant "questions, assumptions, concepts, methods, practices, disciplinary cultures, institutional structures—and so on—all of which are, of course, subject to their own contingencies" (2015, 13). The implications drawn from the need for these conditionals are often negative. For Hacking, they render inevitabilist claims "close to an empty platitude" (2000, 66), whilst for Kidd they collapse the distinction between contingentism and inevitabilism (2016, 13). For our present purposes, we can simply note that if these conditionals do anything, they add immeasurably to the diversity of possible C/I claims, which in turn bolsters the case for localism. They certainly bar us from extrapolating from local contingentisms/inevitabilisms to a global position, and thus make it rather difficult to imagine moving by methodological localism towards a first-order-globalism. If we have determined that X was contingent at T₁ but by T₂ it had become inevitable, how can we move from these particular claims to a globalist one about the contingency/inevitability about results of science in general?

Just as the appropriateness of, say, entity realism concerning electrons should not move us to embrace entity realism globally, the conclusion that a particular scientific result was contingent/inevitable says little about the rest of science, including other 'results' of the same 'kind' (other theoretical entities, or experimental results, or whatever). Finally, that the possibility of arriving at a first-order globalism regarding the C/I issue appears vanishingly small provides an additional damning argument against (global) defaultism of the kind criticised in sections 2 and 3. Defaultism and globalism are closely linked and mutually reinforcing, and the C/I conversation will benefit from explicitly disposing with both. The next section deals with what is perhaps their most salient manifestation: the inevitabilist PUSU demand.

5. Shutting up about putting up

The PUSU demand is as old as the C/I conversation itself. Both began life in Ian Hacking's (1999) book, *The Social Construction of What?* In an attempt to distil points of genuine philosophical importance from the so-called 'science wars' of the 1990s, Hacking introduced two warring factions: the inevitabilists and the contingentists. Faced with contingentist critique, we hear, the inevitabilist is inclined to ask their interlocutor: if the results of science are indeed contingent and could thus have been otherwise, to present a convincing example of an alternative science, meaningfully different from our actual science. In Hacking's memorable phrase, the inevitabilist challenges the contingentist to "put up or shut up" (PUSU) (1999, 79,

³¹ This is not the case for standard realists in the realism/anti-realism debate who want to maintain the mind-independent existence of unobservables (though, we might speak of our *knowledge* of an entity's reality being temporally emergent).

89; 2000, 70). Only recently have authors – Soler (2015b) and Kidd (2016) – provided extended analysis of the PUSU demand. Both conclude that we should not worry about it. I concur. Whilst their reasons differ, both hinge on PUSU being unanswerable in certain ways. I hold instead that it is *unaskable*. The PUSU demand, as I will demonstrate, is symptomatic of defaultism and globalism. I have argued in the preceding sections that these ‘-isms’ be explicitly rejected. Doing so, I will argue, renders the inevitabilist PUSU demand as it presently stands, a non-issue. I will summarise Kidd’s, then Soler’s, reasons for shutting up about putting up (SUPU), before elaborating my own.

5.1. SUPU: Kidd and Soler’s reasons

As we saw in section 3, Kidd charges inevitabilism with epistemic hubris. His rejection of PUSU follows suit. Implicit in the demand, Kidd explains, is the “put-up presumption” (PUP), which holds that “it is actually possible, both in practice and in principle, for the contingentist to attempt to ‘put up’” (2016, 14). The PUP, however, is “incompatible with the practical and social realities of scientific enquiry.” Kidd continues:

It is probably truistic to state that the performance of scientific enquiry, of whatever form, requires at least three things: enormous expenditure of resources, prolonged periods of time, and a community of enquirers ... Given these facts, it is effectively impossible, in practice if not in principle, for a contingentist (or even a community of them) to produce an alternative scientific theory or tradition independently of such resources, time, and community (2016, 14).³²

The PUP, then, asserts an inflated sense of our epistemic capabilities. Thus, in turn, the PUSU demand is based upon a hubristic presupposition. The “practical and social realities of scientific inquiry” render PUSU unanswerable, and *unfairly* so.

Soler’s (2015b) rejection of PUSU comes along with her meticulous formal analysis of the demand and the associated argumentative network (55-56), which I now reproduce:

The put-up-or-shut-up inevitabilist argument against contingentism

Premises:

(P1) The only convincing way to make contingentism plausible would be to exhibit an *actual* (i.e., not fictitious, but really existing) alternative science verifying the three conditions of genuine science, similar questions, and equal value.

(P2) Until now, contingentists have been unable to provide any such alternative.

Conclusion:

³² Kidd here echoes remarks by Emiliano Trizio (2008, 258). Hasok Chang (2015, 360-62), is more optimistic on the possibility of cultivating alternative sciences; see also chapter 6 of his (2004).

(C1) Until further notice, contingentism has no plausibility.

[...]

Auxiliary argument for inevitabilism:

Premises:

(P3) Inevitabilism is the “default position.”

(P4) Contingentism, if it was plausible, could threaten inevitabilism as the default position, but (C1).

Conclusion:

(C2) Inevitabilism is secured until further notice.

Having set out Soler’s scheme, we can now inquire as to her reasons for SUPU.³³ They are twofold. One follows from the so-called ‘monist regime’ which Soler sees as operating throughout the various levels of scientific inquiry. Our science is monist:

in the sense that the development of a multiplicity of alternatives is *not valued* and *not socially encouraged and supported*... [O]ur epistemic activities, are governed by a *monist ideal* and a *uniqueness commitment* that seem deeply entrenched (2015b, 85).

The regime is manifest in our traditional obsession with *unique* theory choice, in the face of theoretical pluralism. As a result, multiple scientific accounts do not long hang around. The past action of this monist regime renders the history of science a thoroughly unpromising place for locating fruitful alternatives to our actual science (rarely were any allowed to develop), whilst its ongoing action all but excludes the possibility of potential put-ups being developed *de novo*.

Even if, despite the monist regime, contingentists *are* able to identify a promising put-up, Soler explains that the inevitabilist holds all of the cards when it comes to evaluating its success. C/I scholars have long struggled with the unenviable task of defining and delineating the three conditions stipulated in P1: genuine science, similar questions, and equal value (e.g., Soler, 2008a; 2008b; Trizio, 2008; Kinzel, 2015). Often these exemplary analyses yield more questions than answers. Given these uncertainties over what constitutes a satisfactory ‘put-up’, it is all too easy for the inevitabilist to claim that the contingentist’s effort fails to meet one or more of the stated criteria (for details, see Soler, 2015b, 83-84).³⁴ In the exceedingly rare case that the inevitabilist is forced to admit that a put-up alternative satisfies the stated criteria, they might finally fall back on the claim that this alternative is “only-transiently-as-good” as the

³³ I am grateful for personal communications with Soler which have been invaluable in clarifying my understanding of her position.

³⁴ For her part, Soler believes that Bohmian quantum mechanics represents a compelling put-up against the actually dominant Copenhagen interpretation (2015b, 75-77); see Cushing (1994).

actual science. “[W]ith *further time, further efforts* and *further evidence*, one will prove superior to the other, as always” (2015b, 77; see also 81-84). So goes Soler’s second reason for SUPU. As with Kidd, PUSU is unanswerable, but here because the inevitabilist will never concede that the demand has been satisfactorily met. Hence the contingentist should simply not worry about PUSU.

As they stand, Kidd and Soler’s reasons for SUPU seem to be good ones, and my aim here is not to pick holes in them.³⁵ However, they might not be necessary. My contention is that if the C/I sphere does away with (relatively) global defaults of the kind criticised in sections 2 and 3, then PUSU is a non-starter. Further, embracing localism breaks up *the* PUSU demand into indefinitely many local PUSU demands. In doing so, PUSU’s distinctive argumentative and rhetorical force is all but lost, and the PUSU demand as it has stood so far in the C/I conversation is rendered a non-issue.

5.2. SUPU: The case from (anti-)defaultism

Examining again Soler’s analytical PUSU framework, we can see that the commitment to inevitabilism-as-default – invalidated earlier in section 2, as well as being challenged in Soler’s own work – is made explicit in P3 of Soler’s above reconstruction of the PUSU argument, as one of the premises for the auxiliary argument for inevitabilism. On first appearances then, stripping inevitabilism of default-status bars the *auxiliary argument for inevitabilism* (and thus invalidates C2), but leaves the *put-up-or-shut-up inevitabilist argument against contingentism* (and the conclusion C1) untouched. This is not, though, the end of the story. The very ability of the inevitabilist to legitimately voice the PUSU argument against contingentism (P1-2; C1) is *dependent upon* the premise that inevitabilism *does command an appreciable level of initial plausibility*. Otherwise, inevitabilists would be under the same pressure to prove the credibility of their position as is the contingentist, and would hence be in no position to entirely saddle the latter with the burden of proof. We can highlight the action of inevitabilism-as-default thinking by filling in some unspoken aspects of Soler’s reconstruction. P1, for instance, might become P1* (my addition is underlined):

(P1*): The only convincing way to make contingentism plausible would be to exhibit an *actual* (i.e., non fictitious, but really existing) alternative science verifying the three conditions of genuine science, similar questions, and equal value. Inevitabilism, on the other hand, is *prima facie* highly plausible, and hence does not require such evincing!

Also clearly problematic – as Soler notes (2015b, 94) – is the stipulation, in P1, that putting-up is the *only* means by which contingentism can accrue plausibility. Note the asymmetry; inevitabilism is taken to possess some intrinsic plausibility which contingentism

³⁵ Forced to conjure criticisms, one might suggest that Kidd’s account does not adequately cover put-ups lifted from actual history of science (such as Bohmian QM); here the hubris of PUP might not apply so strongly. Further, one could put pressure on his pessimism concerning the cultivation of scientific alternatives, e.g. by drawing on Chang’s writings on ‘complementary science’: (2004, Chap. 6); (2015, 380-81). Countering Soler, one might claim that the writings of so-called ‘descriptive pluralists’ bring into question the extent to which a “monist regime” is active in our science; see, e.g., essays in Kellert et al (2006). Soler anticipates this move (2015b, 96-98).

lacks, and must earn through the unenviable task of putting-up. The asymmetry reflects a misplaced sense of the superior initial plausibility of inevitabilism, and thus the PUSU argument cannot be legitimately levelled against the contingentist.³⁶ For the inevitabilist to demand that their opponent put-up-or-shut-up, the former's stance must legitimately occupy the default position, owing to its significant initial plausibility, or justified grounding through some other means. Such legitimisation is currently lacking.

Doing away with defaults would mean inevitabilists are unable to legitimately level the PUSU demand. Hence, we do not get so far as needing Kidd and Soler's arguments for PUSU being unfairly unanswerable. Is this the end of the story? Possibly not. Though first introduced by Hacking as a weapon in the *inevitabilist's* arsenal, PUSU's association with the inevitabilist camp is a matter of convention, not necessity. We can imagine a contingentist 'reverse-PUSU' levelled at the inevitabilist: "You think the results of science are inevitable? Show me one instance in which things could not *possibly* have been otherwise..." That no such notion has been developed in the C/I literature likely represents yet another manifestation of the inevitabilism-as-default orthodoxy. In any case, we would have to reject reverse-PUSU on the same grounds as PUSU, unless, for instance, we follow Kidd and cede contingentism the significant initial plausibility required to enjoy default-status. In any case, I have suggested that embracing localism necessitates we steer clear of adopting (relatively) global positions such as these from the outset.

What about someone *agnostic* about the C/I issue? Could they not co-opt the PUSU demand, supplemented by a modified *auxiliary argument for agnosticism*? Without the unjustified presupposition of inevitabilism-as-default, it is just possible that PUSU becomes 'askable', and the case for SUPU would have to employ Kidd's charge that the PUP is hubristic, or Soler's arguments from the monist regime, and from the unenviable difficulty of convincing one's interlocutor that a put-up satisfies the criteria of genuine science, similar questions, and equal value. We may note, however, that this last argument would be blunted were an agnostic behind the PUSU demand. This, because an agnostic would likely be far less invested in denying that a put-up satisfies the criteria in P1 (indeed, they might even entertain different criteria altogether; see note 36), and in mobilising the "only-transiently-as-good" reply. Furthermore, a good agnostic would level their put-up demand not just at contingentists, but also and equally at professed inevitabilists. These speculations aside, doing away with (relatively) global defaults of the kind criticised in section 2 renders the PUSU demand, *as forwarded by the inevitabilist*, a non-starter.

5.3. SUPU: The case from localism

Making the SUPU case from localism will require us to go back and take a closer look at the passages in which Hacking first introduced the PUSU demand. Although he mostly anchored

³⁶ Though I won't here, one could attempt to build a case which says that not only the premises, but also the *criteria* listed in P1 are symptomatic of a C/I argumentative landscape infused with the inevitabilist instinct. Perhaps if we were instinctively contingentist, or agnostic, about the results of science, we might have landed on different, maybe more inclusive, criteria for evaluating alternative sciences.

it in specific, local examples, Hacking's conception of PUSU was a methodologically globalist one, as we will see. Fully and consciously embracing methodological localism breaks *the* PUSU demand into indefinitely many PUSU demands; as many as there are potential targets for C/I claims. Once we recognise this, the distinctive argumentative and rhetorical force of PUSU is lost. In order to see the globalist methodology at play, it serves to quote Hacking at some length:

The constructionist maintains a *contingency thesis*. In the case of physics, (a) physics (theoretical, experimental, material) could have developed in, for example, a nonquarky way, and, by the detailed standards that would have evolved with this alternative physics, could have been as successful as recent physics has been by *its* detailed standards. Moreover, (b) there is no sense in which this imagined alternative physics would be equivalent to present physics. The physicist denies that. Physicists are inclined to say, put up or shut up. Show us an alternative development (1999, 78-79).

Buoyed, presumably, by Andrew Pickering's landmark *Constructing Quarks* (1984), Hacking's contingentist claims that there could have been a successful nonquarky physics. This is a relatively local contingency claim; one which Hacking's inevitabilist-physicist denies. I say *relatively* local because "physics (theoretical, experimental, material)", though more restricted than 'the results of science' in general, is still a rather large domain. In any case, that Hacking's imagined interlocutors are operating within a *globalist* methodology is made abundantly clear, as he continues:

The sticking point need not be at quarks. But some things definitely are noncontingent, say the physicists, and their appearance in physics was inevitable if science was to progress at all. When the physicist's sticking point is placed under severe challenge, there are several fallback examples: Maxwell's equations, the Second Law of Thermodynamics, the velocity of light. *The contingency claim* is that neither the law nor the equations nor the velocity (*nor anything equivalent*) are inevitable parts of any science as successful as present science (1999, 79, my emphasis).

Hacking moves from talk of *a* (local, albeit it only relatively so) contingency claim to *the* (global) contingency claim. Hacking's contingentist presumably takes claims of types A-D (to return to Martin's scheme) for granted. As such, *the contingency claim* must be read as a claim about the 'whole damn thing'. Not only might the interpretations and applications of science have been different, but, nothing 'equivalent' to Maxwell's equations, or the second law of thermodynamics – that is, no 'result of science' – represents an inevitable part of a successful science. All scientific results are contingent. Correspondingly, his inevitabilists "think that *if* successful physics took place, *then* it would inevitably have happened in something like our way" (1999, 79). The commitment to the inevitability of any specific development (quarks, Maxwell's equations, the second law of thermodynamics) is dropped from the characterisation of their view; these are but temporary "sticking-points", in Hacking's terminology, settled upon for their argumentative and rhetorical force. Hacking's inevitabilist physicists are inevitabilist about physics done in "our way", and all which that includes. Contingentists and inevitabilists, in Hacking's picture, do not argue over local examples for their own sakes, but as part of a

globalist strategy in order to uphold *the* contingency thesis, or *the* inevitability thesis, respectively.

This picture might well have served as a reasonable descriptive gloss at Hacking's time of writing, in the wake of the so-called 'science wars' of the 1990s. However, I am suggesting that it is not a useful nor constructive one going forward. I have defended, instead, a commitment to *methodological localism* (section 4), wherein claims about the results of science in general cannot be supported on the basis of one or a few localised instances of contingency or inevitability. The scope of local C/I claims is just that – *local*. Hacking's contingentists and inevitabilists, on this view, are putting the first-order globalist cart before the methodologically localist horse. Here, a reminder that – for reasons set out in section 4.2 – it appears unlikely that any kind globalist cart would emerge, even after the methodologically localist horse has run its long course.

Due to its being methodologically *global*, then, the inevitabilist PUSU demand as Hacking introduced it has no place in the C/I conversation. Going back to Soler's useful reconstruction, we can recall that, according to the inevitabilist:

(P1) The only convincing way to make contingentism plausible would be to exhibit an *actual* (i.e., not fictitious, but really existing) alternative science verifying the three conditions of genuine science, similar questions, and equal value (Soler, 2015b, 55).

The unfairly unanswerable nature of the demand notwithstanding, what it states *in principle* is that a *single instance* in which contingentism provides a compelling account can support, at least to some degree, a general contingentist stance about the results of science. This is methodological globalism to a T. The localist, without good reason, could not extrapolate from a successful 'put-up' towards the plausibility of contingentism in general. The 'rampant diversity' of the sciences, in matters metaphysical and methodological, renders such a move prohibitively risky, not to mention the limits on generality imposed by the strong situatedness, temporal and otherwise, of any given contingency claim (section 4.2). Perhaps the subject matter of a successful put-up is the *only* case of contingency in the results of science. It is unlikely, but we cannot rule it out. For the localist, then, all that is demonstrated by a successful put-up – assuming for now this feat is possible – is the plausibility of contingentism *in that case*.

Methodological localism, then, breaks up *the* PUSU demand into indefinitely many PUSU demands, each pertaining to one of the indefinitely many aspects of science about which one could make a C/I claim. As per the preceding sub-section (5.2), each of these demands could only be legitimately levelled by an agnostic. The PUSU demand, as originally conceived, has all but disappeared. It was introduced by Hacking as an off-the-shelf response which the inevitabilist could level against any contingentist challenger, in order to uphold their global inevitabilism about the development of science. The semblance of the PUSU demand which remains after doing away with defaults and embracing localism is barely recognisable in comparison to what Hacking originally proposed. It applies equally to the inevitabilist as well as the contingentist, can only be legitimately levelled by an agnostic, and the results are not extrapolatable beyond the restricted domain of the particular target. What more is this besides

a call for careful historical-philosophical investigation of the emergence of particular results of science, and what does continuing to talk of it as the ‘PUSU demand’ add? Not much, is my answer.

6. Concluding remarks: The future of the C/I conversation

Arguably, I have simply reminded us of several things we already know about the C/I issue, and followed their implications further than we previously had. When it comes to the question of whether the results of science are contingent or inevitable, adopting either extreme as default is without justification (section 2), and methodologically suspect (section 3). Further, given the rampant diversity of the sciences, and the complex, conditional, and time-dependent nature of C/I claims, we have little reason to anticipate a one-size-fits-all solution. Methodological localism seems the appropriate approach to grappling with C/I issues in a domain as complex and heterogeneous as ‘science’ and its historical development. This leaves open all manner of possible first-order positions, combining contingentisms about certain elements, and inevitabilisms about others, as well as the possibility that at time T_1 an aspect of science was contingent, but by T_2 it was inevitable, or vice versa (Giere, 2015, 188). As we saw above, the PUSU demand has little place in such a C/I landscape.

Some readers may be thinking: tackling the C/I issue in a localist spirit from an agnostic starting-point is all very well in principle, but what does it mean in practice? What are the constructive lessons? There are straightforward upshots of my discussion: we should, for instance, treat manifestations of defaultism and globalism with suspicion. But what about when it comes to the *doing* of methodologically localist C/I scholarship? How can we go about demonstrating the contingency/inevitability of some result of science? Much like in the realism case, it is a matter of drawing upon existing tools, and utilising them in a localist spirit. I gather here three promising extant suggestions:

(1) *Counterfactual history*. The most widely-considered means of displaying the contingency of a scientific result is to construct an alternative historical narrative of how things might have gone otherwise (for analyses and examples, see Radick, 2008; Bowler, 2013; Hesketh, 2016). Though counterfactual history of science is lastingly controversial, Radick urges that we “can either debate its possibility or get on with the job” (2016, 153). We are seeing scholars take the latter option (Dagg, 2017), whilst methodological discussions increasingly turn from *whether* counterfactual history of science is possible, to questions of *what* it can do for us. And the news is not all bad. Luca Tambolo, for instance, has recently concluded that counterfactual histories can support ‘weak’ contingency claims about science (for details, see Tambolo, 2018, Section 5).

(2) *Active pluralism, or, cultivating alternatives*. Hasok Chang suggests that we can productively reconceive inevitability as *unavoidability* (2015, 360). How do we know if a scientific result is inevitable/unavoidable? We try to avoid it. We actively attempt to cultivate alternative “systems of knowledge in any given area of science.” Our ability to sustain a reasonable alternative will be a function of both the

contingency/inevitability of the result, and the time, resources, and efforts which we devote to our attempts. Hence, “it is not quite right to say that any given result of science *is* either inevitable or not” (360). Straightforward conclusions may be hard to come by, then, especially if we share Kidd’s scepticism concerning our ability to cultivate alternatives (section 5.1). Chang turns this scepticism on its head:

[I]f you had a situation in which even nonexperts (such as philosophers) can come up with viable alternatives, then you would know for sure that the scientific orthodoxy is not inevitable (361).

Inevitability appears harder, in principle, to demonstrate in this way; we could always increase our efforts to cultivate an alternative. We might conclude, though, that a result is inevitable/unavoidable *given* a certain level of effort to avoid it; beyond that level, we cannot speculate.

(3) *Convergence in the history of science*. In his (2005) essay, Radick suggested historical convergence upon a scientific result as a means of establishing its inevitability. Working with the example of the ‘triple-rediscovery’ of Mendelism in 1900, Radick reasoned that:

The greater the number of past trajectories that converged on the same conclusion, and the greater the independence of those trajectories, the more plausible will be the idea that the conclusion was inevitable (26).

The criteria for deciding when any of the above have provided a sufficiently warranted demonstration of contingency/inevitability will surely inspire debate. But such kinds of issues are not unique to the C/I domain. At least in a landscape rid of defaultism, the burden of proof will not sit so heavy, and the criteria of judgment will perhaps not be so unjust, nor enforced so belligerently.³⁷ Embracing methodological localism makes it unlikely we will hit upon any simple (global) answer to the question: “Are the results of science contingent or inevitable?” Indeed, this one question is replaced by indefinitely many. To paraphrase Radick (2016, 153) – himself channelling Steven Shapin (1982, 157) – we can either debate the possibilities of answering these questions, or we can get on with the job.

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³⁷ In the PUSU context, these numbered among Soler’s reasons for SUPU; see section 5.1.

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