**TOWARD A HISTORY AND PHILOSOPHY OF SCIENTIFIC AGENCY**

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***Two Biases***

Two latter-day biases motivate the project outlined in the following pages, both of which grow out of major Kantian themes. The first, central to the *Critique of Pure Reason*, is a firm commitment to the framework-dependency of all scientific knowledge; the second, an equally firm commitment to the essentially self-critical stance central to the sort of reflective, free-willed self-governance on which normativity is made to turn in the *Critique of Practical Reason* and *The Metaphysical Elements of Ethics*. For Kant the two ideas remained understandably unrelated, because for him the framework made explicit and analyzed in the First Critique cannot be considered a *normative* system. The two forms of intuition, the categories, and the body of synthetic a priori truths to which they give rise, govern human cognition as a matter of *fact*, not as a matter of what Christine Korsgaard calls “reflective endorsement;”[[1]](#endnote-1) conceived by Kant as fixed and universal facts of human nature, disclosable by transcendental deduction; facts on which we can reflect and gain progressive understanding, but of which we have no say. We cannot choose, or even seriously contemplate seeing things differently.

But in the neo-Kantian picture of science late of Schlick, Reichenbach, Carnap, and Kuhn, and developed most fully by Michael Friedman,[[2]](#endnote-2) this is no longer the case. By relativizing the constitutive a priori, mounting "Kant on Wheels", as the late Peter Lipton nicely put it,[[3]](#endnote-3) scientific frameworks are no longer seen as belonging to human *nature*, but as acquired, self-fashioned components of human *culture*, internalized through scientific training as *second* nature, as McDowell would have it. They thus become *normative* systems par excellence, that in addition to determining the right and the worthy in science for their practitioners, are deemed themselves to be right and worthy; systems practitioners are trained and expected to abide by, yet are at liberty, in principle, to obey or violate - and, hence, we would like to think, capable of calling into question.

One would have therefore expected philosophers of science, especially those of neo-Kantian leaning, not to follow Kant, and to run the two principles centrally together. But this has not been the case. If in Kant they are relegated to separate Critiques, latter-day philosophers of science have relegated them to diametrically opposed schools of thought! Neo-Kantians, like Kuhn and Friedman, who fully concede Framework Dependency, resist the idea that to be considered rational, a framework's normative hold must derive from it passing normative critical muster, and, more importantly, that its replacement, if rational, be motivated by its failure to do so. Popper and his school, on the other hand, while fully conceding the fundamental centrality of self-criticism to the rationality of all acts of scientific endorsement and rejection, largely ignore, and in most cases explicitly reject what Popper dismissively refers to as “the myth of the framework”.[[4]](#endnote-4)

***The Problem***

The reluctance of those committed to Framework Dependency to apply the principles of critical rationalism to frameworks, and that of critical rationalists to concede the constitutive role of frameworks, is not an accident of history, of course, but a symptom of heartfelt difficulty. This is because commitment to both principles, renders the problem of the rationality of framework replacement that of articulating the possibility of agents adopting a normatively critical stance toward the very normative frameworks constitutive of their thinking – a problem widely considered insurmountable. For how can a constitutive framework be impeached by the very principles of rational reasoning and reckoning of which it is constitutive? Without saying so explicitly, philosophers of science have generally treated the two principles as incompatible; as if forced to choose between them. Frameworks are immune to normative criticism from within, it is implied, not because they are fixed, but because they are constitutive of such criticism: they are criticizable, argue Popperians, because they lack constitutive force; they are constitutive, argue neo-Kantians, and, therefore, cannot be criticized. Indeed. one would be hard-pressed to name a single philosopher of science whose work displays serious commitment to the grounding ideas of both Popper *and* Kuhn.[[5]](#endnote-5)

Even Friedman, with whose notion of the relativized a priori, and deep concern for the rationality of framework replacement in science this paper is in substantial agreement, carefully avoids any talk of rational *motivation* or *agency* in this context. He analyzes what was newly introduced and what of the old was retained in the course of the framework transitions he studies, but the intense erotetic processes of deliberation by which the agents involved deemed the elements they retained to be suitable, those they discarded wrong, and those they modified wanting, is passed over in silence. Friedman doesn't raise the problem of squaring the two principles explicitly, but he does write *as if* it is insoluble; as if his firm commitment to Framework Dependency renders the keen pondering and earnest deliberation of the agents responsible for the moves he charts, irrelevant, or external to the rationality of the transitions for which they were responsible.

***Aim and Strategy***

This paper is an attempt to argue against the grain for retaining serious commitment to both neo-Kantian principles described at the outset – arguing for a general account of normative self-criticism that nonetheless remains fully committed to the framework dependency of science in the strong sense of the term late of Reichenbach, Kuhn, and Friedman, as well as to the radical diversity of successive scientific frameworks (that Friedman tends to play down). The second part of the paper explores some of the historiographical implications of such account by critically developing two of Friedman’s most novel departures from Kuhn.

The first of Friedman's departures from Kuhn, which is further developed in his contributuion to the present volume, is the claim that the physics community is not, and cannot be the only source of ideas capable of proving relevant to physics. Key elements eventually incorporated in new scientific frameworks, he argues, were first long discussed, in discursive settings on which the prevailing scientific framework had no constitutive hold, long before they were put into play by physicists. This is an important observation, and not only for its historical accuracy. Serious consideration of alternatives to a constitutive framework, requires an incentive and ability to think out of the box one does not expect to find among practitioners for whom that framework is constitutive of what "counts as really (physically) possible" Friedman (2010, 714). The very idea of a truly constitutive, yet, at the same time, rationally replaceable framework, requires, according to Friedman, external arenas of unconventional thinking. In all his previous work, Friedman locates the external source of such thinking exclusively in the intersecting fields of mathematics and (scientific) philosophy. His contribution to the present volume broadens the field to include technology religion and politics. I shall return to this important point further down. But first to pinpoint our disagreement.

Friedman implies that an unconventional idea's mere *availability* to scientists attentive to these fields, renders it a viable *scientific* option for them, which in turn explains the rationality of them taking it seriously. While the "constitutive principles operating at a given time and in a given historical situation define … for all practitioners of a given paradigm … what now counts as really (physically) *possible*", the "succession of new mathematical, empirical and philosophical situations … makes it *intelligible and reasonable* for those operating within an earlier paradigm … to entertain – seriously to entertain – a transition to a later one … to envisage – seriously to envisage – a *genuine expansion of the space of available intellectual possibilities*."[[6]](#endnote-6)

But the fact that philosophers or mathematicians happen to toy with an idea that flies in the face of scientific sensibilities - non-Euclidean geometry, for instance - is, *of itself*, no reason for any scientist to seriously entertain it, let alone go out of his way to incorporate it in a near-meaningless modification of the framework constitutive of his work and thinking. For Poincaré, for example, to be deemed rational for considering the scientific viability of a non-Euclidean form of intuition adopted by convention, I insist, requires that he do so *for a reason*, namely, in response to a felt failing or inadequacy of the framework still in play. If we consider him taking such a self-critical stance toward the framework constitutive of an agent’s work impossible, then it matters little where and how the renegade idea originated, or why the agents who elected to run with it initially found it attractive.[[7]](#endnote-7) If they can't be described as responding to problems, as striving knowingly to mend, restore or improve the framework they propose to modify or replace, then we have no choice but to view them, as Richard Rorty does, as aimlessly, or ironically toying with the strange and the exotic with a view to stumbling inadvertently on an alternative that may prove worthwhile in retrospect.

I doubt this is the type of rationality we'd be comfortable attributing to a Galileo or a Poincaré! Rorty, of course, does not find such a position in the least embarrassing.

[The] Wittgensteinian analogy between vocabularies and tools has one obvious drawback. The craftsman typically knows what job he needs to do before picking or inventing tools with which to do it. By contrast, someone like Galileo, Yeats or Hegel . . . is typically unable to make clear exactly what it is that he wants to do before developing the language in which he succeeds in doing it. His new vocabulary makes possible for the first time a formulation of its own purpose. [[8]](#endnote-8)

As DiSalle argues insistently, it seems absurd not to be able ever to describe those responsible for framework replacement in science as exercising their agency in self-conscious and creative response to a perceived crisis.[[9]](#endnote-9) And yet taking seriously the constitutive force of linguistic frameworks seems necessarily to deem such talk incoherent. *Because* they are definitive of their practitioners standards of propriety, a linguistic framework, it is argued, cannot be deemed inappropriate or unfitting *from within* by those whose framework it is. Friedman’s account of the rationality of framework transition is motivated primarily by the problem of incommensurability - namely, by the problem of arguing comparatively for a radically different, well-formed alternative to the framework from which one is arguing. But this is a different problem from that of accounting for the *reasons* an agent might have for wanting to develop such alternative in the first place. Friedman leaves this latter question of the rationality of framework *construction*, elegantly unattended.

***Restating the Problem***

But can it be met? I believe it can, but to see how, the problem needs to be further narrowed and more pointedly stated. As noted above, as much as we would like rational agents to be required (both in and outside science) not only to *exercise* their norms *in* self criticism, but to make them the *object* of such criticism, no serious philosophical account of normativity and personal identity I know of makes room for such feats of self-negation. We engage in self-critical reflection constantly, but our ability to do so must supposedly stop short of the very normative commitments we utilize in self-evaluation. Harry Frankfurt describes the nature of human reflexivity and normative self-assessment as a form of self-alienation. We hold the noisy flow of our feelings, desires, and beliefs in review by “introducing a sort of division within our minds” that “establishes an inward directed monitoring oversight” that enables us to *identify* with the desires, motivations and beliefs that we *want* to have, and dissociate ourselves from those, which although ours, we treat as unwanted alien intruders (such as the urge for a cigarette, or the strong desire to throttle a critic), denying them “any entitlement to supply us with motives or with reasons.”[[10]](#endnote-10)

Frankfurt's much discussed hierarchical model of personhood and agency, to which my work in recent years owes a major debt,[[11]](#endnote-11) premises a basic distinction between first- and higher-order volitions. Our core self - call it our I-part – by which we conduct the self- corrective processes of self-review and self-identification, is the seat of our deepest commitments. According to Frankfurt, what lends our I-part its special judicial authority, is that it is constituted not merely by what we earnestly want, but by what we earnestly *want to want*, and what we want to want pertains to what we most dearly care about. Our I-part is hence the seat of our norms and standards, and of the "volitional necessities"[[12]](#endnote-12) that explain the normative hold they exert on us. It is the part of us with which we wholeheartedly identify, in which we locate who we are and who we aspire to be.

One’s I-part is capable of *self*-reflection, of course, but, and this is the crucial point, its capacity for normative self-*criticism* is limited to *prioritizing*. It can rule a passing desire worthy or unworthy of identification, but it cannot, of itself, normatively impeach any part of itself - which is just another way of saying that we cannot change at will what we wholeheartedly want. Standing back from our I-part, we are left with no normative evaluative means at our disposal (other than to troubleshoot for clarity or consistency). So the problem of rational framework modification, scientific or other, cannot be addressed at the level of personal, intra-subjective, normative self-criticism *simpliciter*. Left to her own devices, a person is incapable *of her own accord* of finding her norms, even her conceptual norms, normatively wanting.

And the same goes for *inter*-subjective dialogue within groups whose members share a constitutive framework. In this kind of discursive setting, as noted above, only two areas of critical space remain available: that between what a norm *is* (such as the meaning of a concept in a given language), and what it is *taken to be* by particular speakers, on which Brandom builds his account of deontic scorekeeping,[[13]](#endnote-13) and that between the conceptual content an agent consciously applies in considered empirical *judgment* and the conceptual content he actualizes spontaneously in empirical *experience*, on which, in recent work, McDowell bases his notion of empirical accountability.[[14]](#endnote-14) I think Friedman would agree that neither of these essentially expressivist or interpretivists options is capable of grounding the rationality of the kind of framework transition in which we are both interested.

In other words, the rational motivation for the need to replace a framework cannot be articulated in that framework’s own terms. There is no way a committed Aristotelian, for example, could argue for the need to replace his Aristotelian framework by force, exclusively, of *Aristotelian argument*. The rational critical dialogue of framework replacement must, therefore, somehow transcend the volitional necessities of the individuals or community whose framework it is. Hence, *pace* DiSalle, the crucial importance of Friedman’s point about the potential relevance to physics of ideas cultivated and debated in adjacent disciplines. On the other hand, though capable of being *questioned* only from without, a constitutive framework can only be effectively *changed* from within, by the practitioners whose work and thought it governs. Only physicists can change physics! Hence, the general problem of the rationality of normative framework replacement can be narrowed further to that of our capacity to *endorse* and *internalize* normative criticism leveled at us by others.

We all know that external criticism can be very effective, but to what extent can its transformative outcome be considered *rational*? For how can a someone else prompt us to do what we are incapable of doing ourselves? What is it about normative criticism leveled at us by others that can set in motion a considered, thoughtful, truly transformative process of normative *self*-reflection? Wittgenstein, who as a rule steers wide of questions concerning the dynamics of language game *variation*, raises the question of the reasonableness of criticism leveled across language-game barriers only once - in *On Certainty*. Wittgenstein speaks for many, in admitting only two possible outcomes when arguing across such fundamental divides. He terms such criticism "combat" (*bekämpfen*), and sees no third option between total failure, when “each man declares the other a fool and heretic”, and unreasoned “persuasion,” the term he uses for “what happens when missionaries convert natives”.[[15]](#endnote-15)

The reason seem simple enough. Rationally accepting or dismissing a critical argument, one would think, should be a straightforward matter of evaluating premises, inferences, and data. But *normative* criticism is different, it purports to question things a person *cannot help* care about – her very norms and standards. Normative criticism inevitably runs up against its addressee's very “volitional necessities.” Hence, for the same reason that normative *self*-criticism is unthinkable, when leveled by others, such criticism can never straightforwardly *convince*. Even when the argument's premises can’t be faulted and its reasoning deemed impeccable, if its conclusions are considered normatively unacceptable we will remain unaffected. Impeccably sound arguments to an objectionable conclusion are either deemed paradoxes and set aside for later consideration, or dismissed defensively without further ado. This, I believe, is the logic behind Wittgenstein's decisive conclusion as well as Kuhn's account of the way anomalies that fly in the face of the prevailing paradigm are ignored in the course of normal science.

The question of rational framework replacement, if it can still be considered a question at all, hence reduces further to whether (and if so, how) external normative criticism can effectively stimulate transformative normative self-criticism *despite it being inevitably rejected*? The very idea of a consciously dismissed argument having such an effect, will strike many as downright absurd. The fact that we have reached such an *impasse*, they would claim, proves by *reductio* that Rorty and Wittgenstein are right in asserting that our normative frameworks mark the outer boundaries of rational consideration, rendering unfounded the very idea of their rational review and replacement.

***The Dynamics of Reasoned Critique***

It is true that arguments critical of a person’s heartfelt norms can never *convince*, but their disputative ineffectiveness has little to do with the quality of their premises or their arguments. Normative criticism is rejected not because its logic is deemed faulty, but because its conclusions are deemed preposterous (or unthinkable, or repulsive). This is the strength of normative commitment, but it is also its weakness. To see why, a brief word about criticism in general is in order.[[16]](#endnote-16)

Criticizing presupposes doubt, but should be firmly differentiated from mere doubting. To doubt is to *suspect* something might be wrong, to criticize is to attempt to *prove* it wrong; to doubt requires no more than applying a question mark, to criticize requires framing an argument - but not any argument. First, critical arguments resemble existence proofs, they argue affirmatively rather than hypothetically - not that ‘*if* these premises are true, *then* the system must be defective,’ but that ‘because they *are* true, the system *is* defective!’ Second, prudent criticism, however, aspires to achieve more than to merely establish the truth of its conclusion. Prudent criticism is an addressed speech-act designed to move its addressees to action. Criticism, in short, is *an argument from firm commitment to the conclusion that something is sufficiently amiss to require its addressees' attention*.

In other words, prudent criticism is aimed at being heard and *endorsed* by its addressees as *self*-criticism. Critics must, therefore, be convincing and base their case on premises (they believe) those they criticize hold true, and in ways *they* deem valid. And the same applies to normative criticism. The problem is that in the case of *normative* criticism such a case can never be made *simpliciter*. For there is never available a set of premises a person is liable to consider true, that can be shown to entail a denunciation of her very norms, exactly because, as we have seen, it is impossible to argue validly against a normative framework *from within*, or *on the basis of* that framework.

Prudent normative critics are aware of this. They know (or at least instinctively sense) that arguing from their addressee's perspective is impossible, but they also know that for their criticism to somehow register and be taken rationally to heart, it must be leveled as far as possible on the basis of premises he can recognize as his own. What normative critics, therefore, normally do – and this is the crucial point I believe Wittgenstein misses in *On Certainty* – is to frame their arguments somewhat *untruthfully*, arguing from an *imaginary* perspective, close to that of their addressee, but sufficiently different from it to be able to make their point.[[17]](#endnote-17) Arguing from the left, critics will surreptitiously premise certain liberal and socialist norms to make their case, while those who argue from the right, will tend to smuggle in just enough conservative value to make their arguments stick.

Honest normative criticism challenges certain of its addressee’s norms by premising a portrayal of his I-part, that though largely true, differs significantly from his own self-image. Since leveled against heartfelt norms, the argument itself, as I have argued, will be dismissed, though not because its premises are deemed false, but because its conclusions are deemed preposterous. And precisely because the argument's premises are *not* refuted, the picture they imply of its addressee's normative identity *may linger on and register* uncoupled from the argument itself.

The subtle but crucial difference, then, between ordinary and normative criticism, is that in the latter case, the normative profile we are presented with, though incongruous with our own, *is not one we have actively refuted*, so that for a brief moment at least, we may find ourselves entertaining side by side both incongruous depictions of our I-part. And since the points of disagreement between the two pictures will pertain to the very norms being questioned, their incongruity, just as in the case of a disturbing playback device, may well have the effect of undermining our initial commitment, and rendering us *ambivalent* toward them. And norms to which we become ambivalent, lose their wholehearted volitional grounding, and with it their I-part status. Thus demoted, they will be subjected, as a matter of course, to the normative critical scrutiny of the self’s remaining I-part. In this way, I submit, deep reaching and trusted normative criticism is capable of creating the inner leeway necessary for truly transformative normative *self*-criticism. But the level of inner-discordance and self-alienation it demands, requires a potentially "ambivalating" echo-chamber of trusted normative criticism that far transcends the possible boundaries of even the keenest self-reflection.

If there is any truth in the account here briefly outlined, then it is possible to take Framework Dependency with utmost seriousness, and still retain a notion of normative self-criticism sufficiently robust to sustain the idea of rational framework review and replacement as a third, deliberative option between Wittgenstein’s two. The argument is quite general, and should, in principle, equally apply to science. If the transition to science can be made to stick, two important causes will have been served. First, it would no longer be necessary to make a special case for scientific rationality (or for that of mathematical physics), as Friedman does. Second, and more importantly, the wishful thinking of Popper and McDowell[[18]](#endnote-18) would be made a philosophically viable reality, even for those who, like myself, are firmly committed to the neo-Kantian grounding ideas of the dynamics of reason. In the remainder of the paper I wish to argue for such an application to science and briefly indicate how it could be exemplified.

***Toward an Historiography of Scientific Agency***

Truly transformative normative self-criticism is possible, but depends necessarily on a challenging environment of trusted, potentially ‘ambivalating’ normative critics. From the neo-Kantian perspective I share with Friedman, such criticism is clearly not forthcoming from within the 'normal,' paradigm-governed discursive settings of one's own field (a point to which DiSalle's otherwise insightful critique of Friedman remains oblivious). The initial source of destabilization must, therefore, be external. On the other hand, as noted, a scientific field can only be transformed from within. Hence, any historiography of scientific paradigm-shifts faces two main tasks: first, to discern the science's external sources of trusted normative criticism, and then to explain how, and by whom their subsequent challenge was converted into an internal transformative force.

First to the question of a science’s trusted critics? Who, outside the boundaries of the community, is capable of producing, at least in individual practitioners, the sort of normative ambivalence toward key components of the prevailing scientific framework, capable of eventually setting in motion a paradigm shift? Let us return to Freidman's first interesting departure from Kuhn. Because their work is *not* determined by the scientific paradigm in force, philosophers, mathematicians, engineers and even theologians, he argues, are capable on occasion of thinking the scientifically unthinkable without running up against heartfelt commitments. They serve science, according to Friedman, as sources of novel and unconventional ideas and possibilities.

Friedman, however, gives no account of the rational incentive a scientist might have for considering such ideas seriously. DiSalle suggests, in Popperian fashion, that the incentive to do so be located in the critical stance certain practitioners adopt toward the framework constitutive of their work and thinking; scientists who, as a result of "conceptual analysis", come to deem their constitutive framework normatively wanting. But as we have seen, DiSalle gives no account of how adopting such a stance is at all possible without forsaking the framework’s very constitutive force and function. I concede Friedman's point about the crucial importance to scientific rethinking of the discussion of scientific ideas in neighboring fields. I also concede DiSalle's central point about identifying rational incentive with self-criticism. However, while they[[19]](#endnote-19) insist on locating the initial source of criticism within the community, and the initial resources for meeting that criticism outside it, I propose the reverse. Namely, to locate the potential sources of scientific questioning in such fields as philosophy and mathematics, viewing them less as providers of *alternatives* to scientific thinking, so much as providing scientists, by virtue of their potential ambivalating effect, with *incentives* to *seek for* such alternatives themselves. With respect both to the scope of professional engagement, and its potential critical content, I find that Peter Galison's account of scientific "trading zones"[[20]](#endnote-20) offers a more promising perspective than Friedman's "meta-frameworks."[[21]](#endnote-21)

***Practitioners Abroad***

Galison's interesting account of the give-and-take between a science's sub-disciplines and neighboring cultures is not motivated by the problem of the rationality of paradigm shifts, as is Friedman's – at least not explicitly. His motives are more squarely historiographical; a desire to get the story of science right by doing justice to the sense in which physics as a whole exhibits, in his opinion, a far greater stability and continuity across major changes in theory than Kuhn's picture would have us think. He does not (or, at least, should not) oppose the idea of major Kuhnian paradigm shifts occurring *within* any one subculture of physics. The only Kuhnian idea he does oppose is the assumption that physics as a whole "marches in lockstep with theory" Galison (1997, 798). In other words still, Galison remains unmoved by the problem of incommensurability *within* each of physics' 'subcultures', and, subsequently, quite unlike Friedman's so-called meta-paradigms, his "trading zones" appear at least not to be meant to solve it.[[22]](#endnote-22)

More recently, Galison has come to acknowledge the fact (formerly pointed out by others[[23]](#endnote-23)) that scientific trading normally extends far beyond the give and take conducted between practitioners of a single science's subcultures on which he focuses in *Image and Logic*.

We need this kind of approach not only to grasp the relation of algebraic geometers to string theorists - but also the way soil scientists speak with farmers, the way fishermen speak with fisheries regulators, the way research scientists speak with students.[[24]](#endnote-24)

And the list can and should be extended further to include all forms of professional scientific interfacing, such as 'trading' for financial or public support, interacting with other scientific disciplines, offering scientific opinion in courts of law and scientific advice elsewhere, deliberating the didactics of best teaching one's subject,[[25]](#endnote-25) as well as engaging one's students, the media, the wider public and the historians and philosophers of one's fields.[[26]](#endnote-26) Trading zones abound.

Galison’s metaphor forcefully brings to the fore the professional, interest-driven purposefulness of such interactions. Viewing practitioners from different fields as *trading* rather than as mere conversation partners, captures the essential self-critical element that I have shown elsewhere is sorely missing in Sellars and Brandom's account of the more general game of giving and asking for reasons.[[27]](#endnote-27) Professional traders frequent trading zones equipped with shopping lists that mark what they perceive they lack, and constantly on the lookout for opportunities they failed to envisage. The trading they actually accomplish will often extend beyond their initial lists, as unanticipated offerings come to their attention. (Professional fairs are primarily geared to exposing potential buyers to new and unforeseen possibilities.) But even then, the dealing will seldom be idle. Unanticipated offerings will be "purchased" only when conceived as *needed*. To return to a point I raised earlier, the mere *availability* of an offering will not suffice to render it a 'live option' for 'traders' unless they perceive it, even in retrospect, as something they *lack* – as a new solution to an existing problem or as an improvement to an existing solution. The very idea of agency in trading settings, usefully presupposes an inherent measure of self-critical incentive.

But this type of self-critical awareness is precisely *not* the kind in which we are interested. The need practitioners feel to trade and exchange with neighboring disciplines pertains to the type of queries Carnap dubbed “internal”, namely, questions (or as Kuhn would have it, 'puzzles') that arise *within* a functioning field of inquiry, as opposed to 'external' questions pertaining to difficulties and shortcomings perceived to plague the framework constitutive of the field. In this sense, rather than create ambivalence, trading tends to reinforce commitment to the prevailing framework.

To what extent can practitioners find themselves *challenged* in trading-zone settings to the point of destabilizing their allegiance to the framework constitutive of their thinking? Galison's analysis offers little in this respect. As he sees it, the meeting ground facilitated by the trading zone's mediating pidgins and creoles is cordoned off from the deep ontological commitments that divide the traders, creating a safe space of shared phenomenology, as he puts it. But I believe he takes his metaphor a step too far, and in doing so misses something crucial.

If Galisonian trading zones are to hold the key to normative transformation in science, we need to look less to what traders bring with them *to* the meeting ground – in terms of both their offerings and shopping lists - and more to what they are liable to come *away* with. Galison concentrates exclusively on the former. His analysis, towards the end of *Image and Logic*, of the "pidginized … foreigner version" of relativistic quantum field theory, fashioned for non-theorists in Bjorken and Drell’s 1964 textbook, is an excellent example. He lays great stress on the 'watering down' involved in making one’s science accessible to 'foreigners', how major practitioners like Bjorken and Drell bring to the trading zone significantly diluted versions of their worldviews. But in one important respect, consistently overlooked by Galison, foreigner 'pidgin' versions most often contain much *more* than their in-house, full-blooded originals. For when abroad, practitioners are frequently required to articulate in simple terms (to pidginize, if you wish) elements of their worldview that are so taken for granted in their home communities, that they remain there largely *unarticulated*. These are the things, Andy Warwick and Harry Collins have taught us, seasoned researchers have internalized to the point of *not* having to justify or explain in their proofs; the things novices are taught ‘go without saying’ in the field.[[28]](#endnote-28) But when presenting to others, such basics often need to be made explicit – and the allusion to Brandom is not unintentional.

Not all trading zones are the same in this respect. Parties to interfaith dialogue, or peace negotiations, for instance, tend diplomatically to tread carefully so as not to offend their trading partners by unintentionally pushing too hard on matters of ideology and faith. But with regard to science such reservations are extremely rare. On the contrary, when representatives of different scientific cultures 'trade' with non-scientists or with each other, more times than none they are met with real curiosity, even fascination, and subjected to the friendly prompting and interrogation of parties who are honestly interested in what they do and how they think, and who jump at the opportunity of receiving an authoritative first hand report and responding to it.

Galison's oversight may be charged to the fact that in the 'real' trading zones that are the subject of anthropological and sociological studies of 'real' pidgins and creoles, the parties are usually far more cagey and reserved in this respect. Trading across ethnic or religious boundaries is often considered culturally threatening and potentially corrupting by at least one of the sides. Because trading is indispensable, the trading zone must be made safe by ensuring that negotiations are limited to what each side seeks to purchase from the other, while cordoning off the broader issues that sorely set them apart. Trading is enabled and safeguarded by interlanguages that subtly serve the double function of both contact medium and *buffer zone*. Scientific pidgins resemble ‘real’ pidgins in the former, but hardly ever in the latter sense. Scientific traders engage each other with interest, fascination, and, at times, even incredulity, not with suspicion or mistrust. Unlike 'real' pidgins, scientific pidgin’s are neither designed nor employed with a view to *shielding* the communicating parties from one another's world views. Insofar as the parties entertain "radical differences in ontology", as in Galison's example, their meeting place will be most often characterized less by a polite and considerate *bracketing* of foundational commitment, than by lively good-natured, unthreatening curiosity as to how the other side sees the world. It is hard to conceive of anything like a scientific analog to the kind of fear and suspicion typical of 'real' trading zones.

This is not to say that making explicit one's innermost professional intuitions and commitments is always an easy or happy exercise. Many people have great difficulty articulating, let alone justifying what they take for granted. Others find it less difficult than disconcerting. Either way, few if any in the scientific world would consider being honestly prompted by fellow academics, technicians, university management, students, grant officials, or the media to openly explain the presuppositions of their world view as threatening or potentially corrupting as is often the case in ‘real’ trading zones.

There is an important sense, then, that when ‘trading’ *away* from home at the trading zones they frequent, far more than amidst their professional milieus back home, that, I suggest (*pace* DiSalle, and going a large step beyond Galison) scientists are liable to become exposed to the kind of ambivalating normative criticism they and their peers are incapable of self-leveling. Often the very act of making explicit a taken-for-granted norm or constitutive presupposition will be enough to create the self-estrangement required to render them ambivalent and consequently subject to self-criticism. Hearing oneself think aloud in response to the innocent probings of the uninitiated can suddenly and unexpectedly strike one as hollow and unconvincing.[[29]](#endnote-29) But more often than not practitioners prompted to explain themselves do so comfortably and confidently without experiencing any sense of self-estrangement. In these cases, it is the incredulity of their ‘foreign’ interlocutors, or their very different way of thinking, that may prove sufficiently “ambivalating” to set the transformative process in motion.[[30]](#endnote-30)

***The Transformative Force of Creative Indecision***

The creative individuals initially responsible for rationally transforming a field, are thus to be sought among those who where lucky to be exposed to the “ambivalating” challenge of real or imaginary[[31]](#endnote-31) trusted external critics. But that is not enough. An individual successfully rendered ambivalent to what Haugeland calls his “existential commitments,”[[32]](#endnote-32) may end up thinking differently from his colleagues, or even considering a career change, but that of itself will not suffice to transform their *field*. To have an impact on the normative commitments of their home communities, it is not enough for individual practitioners to merely *be* or *become* normatively ambivalent, their ambivalence must be capable of spreading and infecting a critical mass of their fellow researchers.

Whole communities do not travel abroad, only individuals. Whole communities can, therefore, only be ambivalated at home, from within, by already ambivalated members of sufficient standing and voice to be heard and adhered to. (This is where I steer closest to Walzer’s notion of *connected* criticism.) If there is any truth to my conclusions so far, then the role of such individuals in the history of major scientific upheavals is decisive. But their story has largely yet to be told. Current historiography of science, diverse, wide-ranging, and richly reflective as it has become in recent decades, passes such figures over in near silence. Historians of science show little awareness of their existence, and hardly any interest at all in exposing and analyzing the sources of their ambivalence and its effect on their peers. Telling their story raises two related questions: that of their visibility, and that of the nature of their impact.

First to visibility. Ambivalent, undecided scientists of the past are hard to see. Kuhn's account of paradigm shifts tends to juxtapose vivid 'before' and 'after' pictures in dramatic, seemingly unbridgeable contrast, as in a weight-watchers' ad. It is easy to see how this kind of narrative structure remains virtually blind to the intermediary figures whose commitment to the old is waning, with no solid alternative yet in sight.[[33]](#endnote-33) Those whose historiography of science leans more toward Popper than to Kuhn are no better off. For here, again, those responsible for the intermediary moments of deliberative *indecision* are lost from sight in the dazzle of the knockdown *experimentum crucis*, the decisive refutation and the bold and novel conjecture. The idea, central to the account proposed here, that the driving force of normative reassessment be located primarily in moments of discordant ambivalence, and painful indecision, sits uneasily with virtually all current historiography of science. Ambiguity and indecision are as a rule not considered the stuff of science, at least not the stuff capable of contributing to understanding the drama of bold scientific advancement.[[34]](#endnote-34) Friedman, of course, is the notable exception.

But first, it is important to understand that the invisibility of ambivalent, undecided scientists owes to more than historiographical blinkering. It is not as if such individuals could be clearly seen had *historians* not been looking elsewhere. For they are obscured from the sight even of their own communities for the simple reason that doubts and indecision are hardly ever considered worthy of *publication* even by those they plague. Modern science is a predominantly written culture whose world view is cultivated, enriched, expanded, modified and proliferated primarily by means of research papers. Scientific research papers are normally not where one doubts and dithers. As a rule, doubters and ditherers do not view their ambivalence and indecision worthy of scientific publication, let alone their potential editors, referees and readers. Indecision and doubtfulness are not regarded contributions to knowledge as are the kind of reasoned suggestions for new developments and reasoned challenges to old developments that are deemed publishable. It is true that current historiography of science remains largely *uninterested* in the ambivalent and undecided members of the communities it studies, but it is equally true that the ambivalent and the undecided remain largely hidden from scientific sight *within* those communities, where their work is relegated almost entirely to the private realm of the diary, the journal, the log-book, the personal memoir or the personal correspondence, none of which are capable of carrying their master's voice effectively into the public professional domain of scientific discourse proper.

This is not entirely true, however. Undecided, "ambivalated" practitioners of standing occasionally do succeed (if, somewhat unwittingly) in making their indecision known to and felt by their peers in ways capable of effectively further 'ambivalating' the field. Friedman's historiography of modern mathematical physics is an interesting first case in point.

In his attempt to find rationality where Kuhn (at least the Kuhn of the “purple” passages of *Structure*[[35]](#endnote-35)) finds gestalt switch conversion (of the kind Wittgenstein terms “persuasion”), Friedman, elects to redescribe paradigm shifts as smoother less ruptured, 'evolutionary' (to use his term) processes of transition in which new frameworks are developed naturally, and gradually out of their predecessors - which they continue to preserve as special cases. In Friedman's decidedly more leveled picture of framework replacement, figures whose work occupies halfway positions between the two end points of the transition become important for the way their work furnishes the "essential intermediate stage[s]" between the old and the new frameworks that render the latter "actually continuous with" the former.[[36]](#endnote-36) Two of his main examples are Galileo's treatment of free fall and projectile motion, that occupied a halfway position in the transition from Aristotelian to Newtonian mechanics, and Poincaré's work on the foundations of geometry that occupied an analogous position in the transition from classical physics to relativity theory, "without which," he declares, "it is hard to imagine how the use of non-Euclidean geometry in physics could have ever been envisioned as a real possibility".[[37]](#endnote-37) Friedman describes neither work as ambivalent or undecided, or as in any other way unsure or troubled. On the contrary, he presents them as confident attempts to retain certain elements of the old while developing certain novel departures that would eventually form the basis for the new. Such mediating positions, he argues, provide living disproof of the radical unbridgeability of Kuhn’s depiction of the transitions they so nicely straddled.

Friedman is right not to describe the mature works of Galileo and Poincaré as anything but confident presentations of the positions to which, by the time they were published, both were fully committed. Tycho Brahe’s hybrid grafting of a heliocentric planetary system on a basically geocentric cosmology would be another example of a midway position presented in genuine confidence and with full conviction. But accepting this does not commit one to Friedman’s account of the role of intermediaries in the course of framework transition.

The intermediary frameworks devised by Tycho, Galileo and Poincaré, I insist, were no more “natural developments” of those they purported to replace, than were the final positions of Kepler, Newton and Einstein natural developments of theirs. The fact that they were able to form and maintain such halfway positions only proves that even they, on whose work the old framework evidently still exerted a profound hold, had succeeded in becoming sufficiently ambivalent toward it to propose and endorse several radical departures. The relative 'nearness’ of the positions they formed in relation to those they abandoned as to those later adopted by their respective communities, does not absolve Friedman of the need to account for their *reasons* for forming them.

Intermediary figures of standing, whose mature works attest to a strained split, or dithering between a lingering commitment to the frameworks constitutive of their earlier work, and a deeply felt need to modify those same frameworks, bear vivid witness, I submit, not to how easy, but to how *difficult* it must have been for them to forge and maintain such positions.

To Friedman's examples, I might add several of the early Victorians on whom I have formerly worked, and to whom I plan to return in future work with a better appreciation of their significance - such curiously hybrid proposals as George Peacock's strained two-fold account of algebra,[[38]](#endnote-38) William Rowan Hamilton's insistent talk of there being two, rather than one "science of dynamics",[[39]](#endnote-39) or William Whewell's better thought-out, yet similarly strained two-sided ‘antithetical’ theory of scientific truth.[[40]](#endnote-40) Tycho, Galileo, Peacock, and Poincaré all presented their hybrid proposals with confidence and conviction, but there was nothing continuous or natural about their radical and painful breaking with the party-line geocentrism, Aristotelianism, algebraic realism, and Kantianism from which they had distanced themselves respectively. I prefer to see such fundamental departures as the work of deeply “ambivalated” practitioners, whose anxious compromises mark the extent to which, when forced, they were capable of going.

What makes such figures indispensable to the deeply transformative moments in which they partook, and, hence, to the latter-day historian, is the way, their initial ambivalence was captured in their Solomonic attempts to split their subject matter in the hybrid manner described. Carefully analyzed, pried apart at their (usually) rough seams and properly reenacted, such doubles and splits offer historians much toward recapitulating the destabilized mindset of their authors. But only if read and analyzed prospectively, as history is lived and should be studied, as acts of anxious and urgent engagement, rather than retrospectively, as mere intermediate stations between one stable framework and the next; as fossilized normative dilemmas, as it were, preserving for posterity frozen, ossified snapshots of tortured, creative indecision. For they are too often written off as unimaginative first tries, or as the work of confused reactionary rearguarding.

As noted, normative doubts or ambivalence *per se* do not spread easily within a well-functioning scientific discipline. But confident resolutions of such doubts can. And most often the first attempts to resolve them are proposed by the "ambivalated" individuals themselves. Indeed, who else but them would be motivated to do so? In Frankfurtian terms, normative ambivalence amounts to being of two minds with respect to certain elements of one's normative framework; a form of indecisive dithering with respect to those elements. To the remainder of their framework (the part of their I-part to which they have not become ambivalent) such "ambivalated" individuals remain wholeheartedly committed. It follows that even when their ambivalence is resolved by replacing the doubtful elements by others, the rest of their framework will remain firmly intact. In other words, the kind of hybrid, halfway solutions proposed by Tycho, Galileo, Peacock, Whewell and Poincaré are precisely what one *would* expect in the first instance.

Modest as their suggestions might seem in retrospect, few are capable of such creative feats of rethinking and self-dividing, however deeply "ambivalated" they become. It takes a Tycho Brahe to replace a fully-fledged, mathematically functioning, Ptolemaic, geocentric cosmology with a heliocentric planetary system embodied within an otherwise geocentric cosmology; a Galileo to replace a two-tiered, hierarchical universe that displays for each of its two parts a different kind of natural motion, with a unitary, non-hierarchical universe that incorporates both forms of natural motion in its one part; a Peacock to propose a systematic account of modern algebra as a hybrid juxtaposition of "arithmetical algebra" viewed as a truth-governed science of number extended as far as it would go, that “gives rise by suggestion” to a purely formal "symbolical algebra", sufficiently general to accommodate the full range of algebraic expression; and a Poincaré to replace Kant's account of the epistemic exclusivity and necessity of Euclidean geometry with an equally constitutive notion of the same "form of intuition", but adopted by convention. And, again, there was nothing "natural" or "naturally continuous" about any of these startlingly original proposals! On the contrary, and this is my main point, not only do such hybrids *not* seem natural and continuous to the fellow practitioners to whom they were originally addressed, they strike them as forced and jarringly disjointed. Once the coherent unity of the old system has been broken and key elements of an alternative system have been put into play by practitioners of such standing, other members of the community are liable to be rendered ambivalent with regard to larger portions of the framework, and motivated to develop the transition much further, as was the case with regard to all four.

**\*\*\*\*\***

For the reasons elaborated in the first part of the paper, historians of science committed, late of Kuhn, to the framework dependency of scientific practice and produce, have been disinclined to analyze cases of framework transition as the products of rational human endeavor. There has been a marked tendency in post-Kuhnian science-studies to tell the story of such transitions in collectivist and/or institutional terms, or, as in Friedman's case, by subtly reassigning the term ‘rational’ from the *process* to the *product* of scientific reasoning – as if a *descriptive* account of the changing *produce* of scientific endeavor will suffice to yield a *normative* assessment of the endeavor itself. Telling their story the other way around, as the reasoned and contrived products of a process of self-critical deliberation, this paper has argued, minimally requires breaking it down into three basic and ordered elements or chapters, the first and third of which pertain to the process, and the second to the creative product of the kind of normative ambivalence described above. The first chapter will show how practitioners of standing were rendered ambivalent toward central components of the prevailing framework in the course of 'trading' away from their home communities. In the second, the novel, and widely read accounts of their field produced as a result will be shown to have been profoundly and uneasily split along the faultlines of their initial indecision, and analyzed accordingly. The third chapter will show how these works then succeeded in prompting other practitioners of standing to take a more decisive stand.

Unlike their chronological ordering, the order of discovery of the three elements will usually be different. Direct evidence of the first and third stages will normally lie hidden from sight deep between the pages of private correspondence and intimate diary entries. What will inevitably first attract attention are the hybrid published works that characterize the second stage. If they are noticed and duly appreciated (their confident rhetoric notwithstanding) as works of ‘creative indecision’ (rather than mere confusion), then their origin and formation can be traced back through the archives to the ambivalating circumstances that begot them. Likewise, once thus appreciated, their careers thereafter can be traced forward through the impact they had on the work and thinking of the key practitioners who read and responded to them.

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**ENDNOTES**

1. See Korsgaard (1996) especially chs.2 and 3. [↑](#endnote-ref-1)
2. See especially Friedman (2001), (2002) and (2010), as well as his contribution to the present volume. [↑](#endnote-ref-2)
3. Lipton (2001). [↑](#endnote-ref-3)
4. Popper (1976) and (2003). [↑](#endnote-ref-4)
5. An interesting early exception is Hilary Putnam’s notion of revisable “framework principles” developed during the 1960’s and 70’s in response to Quine’s two dogmas argument. However, Putnam (1976) and (1994) lays great stress on the necessity of such principles, i.e. their immunity to empirical refutation, rather than their constitutive role regarding the empirical. For a detailed account of the stable and the changing elements in Putnam’s work on these topics, see Mueller and Fine (2005), especially §III; for a comparison of Putnam and Friedman's positions in this respect see Tsou (2009). Another more recent exception is Robert DiSalle (2002), (2006) and (2010) briefly discussed n. below. [↑](#endnote-ref-5)
6. Friedman (2010, 714) italics added. [↑](#endnote-ref-6)
7. DiSalle, who otherwise fully concedes Friedman's position, rightly calls him to task on this point, urging him to "reconsider the role of philosophy in the evolution of science – not merely as an external source of general heuristic principles and new conceptual possibilities, but, at least in the most important revolutionary developments, as an objective tool of scientific inquiry" (2002, 192). DiSalle argues that, in addition to the role attributed to philosophy by Friedman as a source of new conceptual possibilities, the history of the transitions from Aristotle to Einstein exhibits significant moments of keen "dialectical confrontation with prevailing conceptions" (2002, 204) performed by major practitioners with a view explicitly to "revealing the hidden presuppositions of the old conception, and exhibiting the internal difficulties that must be resolved by the new" (2010, 528). He protrays phycisists as being, not merely alert and open to "the rational discussion of theoretical alternatives" (527) by philosophers, but as actively partaking in that discussion as part of their work as physicists. DiSalle, however, nowhere raises the question of the very possibility of adopting such a critical stance, which, as I have argued, poses a thorny problem indeed, especially from the neo-Kantian perspective he supposedly shares with Friedman. [↑](#endnote-ref-7)
8. Rorty (1989, 13-14). [↑](#endnote-ref-8)
9. E.g. DiSalle (2002, 194) and (2010, 526), but, as noted, without taking due note of the problems involved. [↑](#endnote-ref-9)
10. Frankfurt (2006, 4). [↑](#endnote-ref-10)
11. See especially Fisch and Benbaji (forthcoming, ch.9) and §6 of Fisch (2008), on which the following two paragraphs are based. [↑](#endnote-ref-11)
12. The nonvoluntary "necessities of a person's will" writes Frankfurt (2004, 50), “guide and limit his agency. They determine what he may be willing to do, what he cannot help doing, and what he cannot bring himself to do.” [↑](#endnote-ref-12)
13. Cf. Brandom (1994: Ch1, §IV) [↑](#endnote-ref-13)
14. See in particular chs. 14-16, 18 and 19 of his (2009a) and ch. 6 of his (2009b). [↑](#endnote-ref-14)
15. Wittgenstein (1974, 81e, §611-12). [↑](#endnote-ref-15)
16. The following paragraphs briefly distill the relatively detailed phenomenology of critical engagement presented in chs.8 and 9 of Fisch and Benbaji (forthcoming). [↑](#endnote-ref-16)
17. While wholly characteristic of prudent normative critical discourse, doing so is is usually not as contrived and deliberately untruthful as I make it sound. Most often the discrepancies between the presuppostions we attribute to the people we criticize, and those they actually hold true owe to our inability to fully appreciate how different they think. a self-distancing born of the sort of empathetic understanding R.G. Collingwood viewed as central to the historian’s ability to appreciate the "logic of question and answer" that motivated those he studies (Collingwood (1939, Ch.V)); the straddling sort of understanding Gadamer so deftly problematized and articulated. The real achievement of effective criticizing is a "fusion of horizons", but in a sense different from Collingwood’s and Gadamer's – namely, the ability to reason like somebody else, not in order to understand that other person's world, so much as to stand back from it and call it to task. [↑](#endnote-ref-17)
18. I am referring to McDowell’s brief digression on Aristotelian ethics toward the end of Lecture IV of *Mind and World*. [↑](#endnote-ref-18)
19. I take the liberty of combining the two proposals because both Friedman and DiSalle view the latter's as complementing the former's. As Friedman puts it:

    DiSalle takes his approach to be complementary rather than opposed to my own: he is adding something to my version of the dynamics of reason rather than contradicting it. I view his approach, similarly, as complenatary rather than opposed to mine, and I have no doubt, in particular, that critical conceptual analyses of the kind DiSalle describes in fact play a central role in the development from Newton to Einstein. (2010, 718) [↑](#endnote-ref-19)
20. See especially Galison (1997), especially ch. 9. [↑](#endnote-ref-20)
21. Friedman (2001) Part Two, ch. 4. in later works the terms "meta-framework" and "meta-paradigm" are dropped in favor of the less misleading "meta-scientific level" – e.g. Friedman (2010, 718). [↑](#endnote-ref-21)
22. However, at critical moments, Galison's presentation takes a far more radical philosophical stand against Kuhnian antipositivism, such as in his philosophical self alignment, in the very last section of *Image and Logic* with "a historicized neo-Kantianism" (described (p. 842) in terms similar to Friedman's notion, late of Reichenbach, of a relativized apriori) and "the justly influential work of Donald Davidson and Hilary Putnam". However even there, he describes his project as "manifestly more historical" (p. 840) than philosophical. [↑](#endnote-ref-22)
23. Notably Gorman (2002) and Collins *et. al.* (2007). [↑](#endnote-ref-23)
24. Harman and Galison (2008, 568-9). [↑](#endnote-ref-24)
25. For a detailed account of the transformative role such didactical deliberations played in the formation of William Whewell's understanding of mathematical physics, see Fisch (1991, ch.2). [↑](#endnote-ref-25)
26. For a usefully nuanced typology of all such interactions see Collins *et. al.* (2007). [↑](#endnote-ref-26)
27. Fisch and Benbaji (forthcoming) ch. 7. [↑](#endnote-ref-27)
28. See Warwick’s insightful study of the training of mathematical physicists in nineteenth century Cambridge. See Warwick (2003) especially chs. 2-5, and Collins's equally insightful account of the taken-for-grantedness of a scientific community's basics in the section entitled "Wittgenstein and Rules" in Collins (1985, 12-16). [↑](#endnote-ref-28)
29. My own work on the transformative impact William Whewell’s early career as a textbook writer had on his philosophical understanding of science, is an interesting example in this respect. See Fisch (1991a, ch.2) and (1991b, §3). [↑](#endnote-ref-29)
30. Staying with examples I have worked on myself, George Peacock's deeply ambivalating engagement during the early 1820's with Charles Babbage's thoroughly formalistic approach to mathematical analysis, and the distabalizing effect Peacock's subsequent *Treatise on Algebra* (1830) had on Whewell's philosophy of mathematics, and the mathematical thinking of both Augustus DeMorgan and William Rowan Hamilton, provide vivid illustrations of the ambivalating impact talking across paradigm dividing lines is capable of having. For a detailed account of the first part of the story see Fisch (1994a), and especially (1999). [↑](#endnote-ref-30)
31. As when preparing a lecture, a report, or writing a textbook. [↑](#endnote-ref-31)
32. Haugeland (1998, 340ff.). [↑](#endnote-ref-32)
33. Lakatos's historiography of scientific research programmes resembles the later Kuhn in subjecting transitions from one research programme to another to purely instrumental considerations of predictive fruitfulness. Commitment within a programme to the programme's 'hardcore', is normative, in the sense that it is rendered immune to empirical refutation by the adhoc construction of "auxiliary hypotheses". Commitment to the programme itself, however, is, for Lakatos, not a matter of empirical testing, but of empirical *fertility*. For his methodology and historiography of scientific research programmes see Lakatos (1971) and (1978). For recent expositions of Lakatos's position in comparison to Kuhn's and to Friedman's, see Gattei (2008, pp. 56ff), and Koertge (2010) respectively. For criticism of Lakatos c.f. Garber (1986), Curtis (1986) and Fisch (1994b). [↑](#endnote-ref-33)
34. One self-conscious exception to the rule is the role given to intermediary figures in the historiography employed in Damerow *et. al*. (2004). [↑](#endnote-ref-34)
35. The term is Earman's (1993, 19) with reference e.g.to Kuhn (1970, 94, 150, 151). [↑](#endnote-ref-35)
36. Friedman (2001, 61). [↑](#endnote-ref-36)
37. Cf. *loc. cit*., pp. 60-63. Galileo's intermediary role lies in preserving the two basic Aristotelian forms of natural motion – naturally accelerated motion toward the center of the earth, and naturally uniform circular motion directed at right angles to it – while discarding "the hierarchically and teleologically organized Aristotelian universe" (61). According to Friedman, Galileo's notion of circular inertial motion rendered "the modern conception of rectilinear natural inertial motion … actually continuous with the preceding Aristotelian conception of natural motion." (ibid) On Galileo as an intermediary figure see also Damerow *et. al.* (2004, Ch.3), especially §3.7.

    To Poincare's *Science and Hypothesis* (a book he maintains Einstein was "intensively reading … immediately before his revolutionary breakthroughs in 1905"). Friedman attributes the neo-Kantian idea that although geometry functions as "a constitutive framework making properly empirical discoveries first possible", the fact that "there is more than one such constitutive framework", can only mean that the choice between them is "based on a convention" (simplicity), and not on "some innate necessity of the human mind" (62). In this way, "Einstein's introduction of a radically new constitutive framework for space, time, and motion again grew naturally out of, and is thus quite continuous with, the preceding framework it replaced." (63) [↑](#endnote-ref-37)
38. On the formation of George Peacock's *Treatise on Algebra* during the 1810's and 20's presented as "a case of ceative indecision", see Fisch (1999). [↑](#endnote-ref-38)
39. See Fisch (1991, 63-7) and references there. [↑](#endnote-ref-39)
40. See Fisch (1985), (1987), and (1991a, 93-8) [↑](#endnote-ref-40)