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**Virtue and otherwise in Peircean Abduction:  
Bias, fabrication , and deductive overindulgence**

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## **Virtue and Otherwise in Peircian Abduction: Bias, fabrication, and deductive overindulgence**

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“The best hypothesis, in the sense of the one most recommending itself to the inquirer, is the one which can be the most readily refuted if it is false.”

Peirce (1896)

“Belief has no place in science.”

Peirce (1898)

“There is nothing more irrational than false pretence ... Let us not fall into that vice.”

Peirce (c. 1902)

### **ABSTRACT**

The first-mentioned item of the subtitle of this talk is unavowed by Peirce but compatible with his naturalism and, I would say, a virtue. The second, also virtuous, is strongly endorsed by Peirce. The third is both a clutter of confusion and an irony. The confusion, as we will see, speaks for itself. The irony is that much of the clutter could have been avoided had logic's most trinitarian thinker cottoned on to deduction's own trinitarian character. We have, in this, abduction's most overlooked truth. Peirce's story of abduction cannot be told without sorting out his story of deduction. It is a tangled tale. Because time presses, what I offer here is a sketch of things to come.

**KEY WORDS:** abduction, bias, deduction, Everyman, fabrication, instinct, logical critic, scientific practice, Scientific Progress.

### *1. A turn to the natural*

There is more to be learned about Peirce's abduction in his naturalism than in whatever of Darwin lies in his thinking. It is beyond reasonable doubt that Peirce's philosophy of enquiry sees humans as environmentally situated organic beings who are strongly disposed to replace maladaptive behaviour with practices better suited to their interests. Naturalism accords primacy to the observable and, as regards the phenomena of human cognitive life, to the empirically discernible regularities of human behaviour. The regularities are shaped by what humans are like. So what *is* it to be a being like us, and what it is about us that makes abductive behaviour part of our natural repertoire? Let me count the ways. (Some of them.) We have large brains. We make our way in life by using our heads and our tongues. For this to happen, we have to know things, and we must attain adequate levels of understanding of what we know. To these same ends, we must come to terms with our environments and enter into complex patterns of coordination and succour with our fellows. Millisecond-by-millisecond, we are exposed to massive flows of information and are favoured with mechanisms of filtration to protect our processing-systems from collapse. One of the most important of our filtration measures is the extraction of relevant information from semantic junk. It is a separation which presupposes that we are beings with interests and, to some nontrivial extent, who know what those interests are. In other respects, we are strangers to our own selves. We are beings who do the best we can with

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the cognitive resources available to us and, from early in life, are responsive to cost-benefit considerations. We are, in short, rather impressive cognitive economists, and dab hands at advancing our cognitive interests. There is little doubt that Peirce would have thought that some of our traits have been selected for our species' survival. Where this starts and why is still open to debate. The distinction that matters here lies between the uniform and differential distribution of epistemic ability. (Moreover, Peirce would give no quarter to the idea that the *science* of knowledge is a product of evolution) This, I think, is about all the evolutionary consideration that's needed for what awaits us here.

Upon occasion, matters arise or come to our attention. There are aspects of them that we don't understand. They are things that puzzle us, perhaps even bewilder us. There is something about them that strikes us as somehow out of order. This stirs a causal disposition to initiate a two-stage procedure. In the first, we seek some hypothesis which, if true, would enlarge our understanding of the situation and clear the air. This is not yet abduction. A second step is required, itself a double-hinged one. If we find in our hypothesis reason to suspect its truth, it is open to us to call for its submission to experimental test. Abduction happens when other options are unavailable. The understanding which the enquirer seeks can't be got from any knowledge-base within his current and timely reach. Neither is the enquirer prepared to let the matter go and get on with things that matter more. He wants an answer; so he's left with no choice but to use his head. When the conditions laid out here are fulfilled, the process of abduction initiates and terminates. How do we know these things about abduction? We know them by paying attention. To what do we attend? We attend to what actually happens. We attend to the discernible regularities of hypothetical reasoning.

Here is something worth knowing about abduction. It is a worthy subject of investigation, but it is not itself, just so, fit occasion for abductive enquiry. It is not puzzling that we are good at good-guessing. There is nothing in any way out of order about it. It is no more puzzling than we are good at breathing. There are times when Peirce appears to think not only that abduction demands its own abductive enquiry, but that abduction is implicated in all of enquiry. There is, however, no evidence of either in the empirically discernible behavioural regularities of cognitive life. Just to be clear, there is a good deal about the mechanics of breathing that puzzles people. That is why some of them take up respiratory studies. The same is true of the mechanics of good-guessing. Some people have interests that draw them to the logic of reasoning. But this is not the point presently at hand. It is that there is nothing puzzling about the *fact* that we are good-breathers or the *fact* that we are good guessers. In neither case is anything out of joint.

In three of the most quoted lines of 19<sup>th</sup> century logical thought, Peirce writes as follows (with line-numbers and horizontal line added by me):

- “The surprising fact *C* is observed. (1)  
But if *A* were true, *C* would be a matter of course. (2)  
Hence there is reason to suspect that *A* is true.” (3) (CP 5.189)

A bit later, he enlarges on this theme, ‘retroduction’ replacing ‘abduction’:

“By Retroduction I mean that kind of reasoning by which, upon finding ourselves confronted by a state of things that, taken by itself, seems almost or quite incomprehensible, or extremely complicated if not very irregular, or at least surprising, we are led to suppose that perhaps there is, in fact, another definite state of things, because, though we do not perceive any unequivocal evidence of it, nor [sic] even of a part of it (or independently of such evidence if it does exist), we yet perceive that this supposed state of things would shed a light a light of reason upon the state of facts with

which we are confronted, rendering it a comprehensible likely (if not certain), or comparatively simply and natural.” (MSS 856)

It is easy to see that Peirce has underdescribed abduction in these passages, even in the one in which we find more words than clarity. Still we have enough of the basic idea and the general hang of things to be getting on with. It is as established a fact as one can get in cognitive life that we are good enough at abduction for it to be retained in the toolbox of our enquiries into things of interest to us. What else might be learned of our cognitive involvements?

## 2. *Bias*

Peirce approaches the phenomena of abductive inference in the course of his reflections on the methods of experimental science.<sup>1</sup> Among the parties accessible to abductive practice, there are three that matter here. One is Everyman, the collective all of us. The second is workaday science. The third is what I will call breakthrough science, that is to say, science at the stage where it lifts itself up in response to some pressing but unmet demands upon it, or it finds cause to overthrow some significant dimension of an orthodoxy. Workaday science is science in action, and it stays alive and valued by its advancement of the understanding, at times in small increments and at others in larger jumps.<sup>2</sup> The difference between the workaday and the breakthrough lies in the shock of the new. Breakthrough science is science at its most ambitiously exploratory and least tested and, one might also say, its most reckless. Often it is Great Science in early adolescence, and gravid with young truth. In all three cases, abductive inference is the motivating link between hypothesis-searches and hypothesis-tests. Hypothesis-searches quest for good guesses which, were they to pan out, could provide some sought-for advancement of the understanding. It is only natural therefore that abducers would search for hypotheses that would survive the rigours of subsequent testing. It is in this sense that hypothesis-searches carry a confirmation bias. Of course, hypothesis-testing is otherwise structured and has a different end. It harbours a disconfirmation bias, answering as it must to tough demands for error-avoidance. The objective of a test is to cause the test-hypothesis all the trouble it has coming to it, but nothing more. Either way, over-excited bias disoblige Peirce’s duty of truthfulness (CP 1.149), since untruthfulness produces bad outcomes.<sup>3</sup>

As long as we admit naturalistic assumptions to our reflections, we must allow for the inherence of bias in nature’s provisions for us. If knowing the truth of things didn’t advance our interests, we might well have learned, or could soon come to learn, the effort-saving advantages of avoiding the true altogether. Unlike virtue, truth is not its own reward; we seek it for the good it does our interests. It lies in our interest to be good enough at guessing-true to keep the guessing-game in business for long enough to know the benefits of survival and biological prosperity and, from time to time, the delights of great civilizations. So it is as natural as breathing that guessing should be imbued with a confirmation bias.

Error, of course, inhibits true belief and accurate guessing. We are better at correcting error than we are at avoiding it outright. It has to do with the efficiencies achieved by the first, as

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<sup>1</sup> Keeping track of Peirce requires negotiated balance on shifting sands. Abduction began as a form of inference, and then became a stage of enquiry. It originated as ‘hypothesis’ in 1865, and became ‘abduction’ in 1878, and blossomed into ‘retroduction’ in 1898, and was referred to coextensively thereafter. His first choice was, I think, *le mot juste*.

<sup>2</sup> Consider for example the Salk vaccine for what was, at the time, called infantile paralysis.

<sup>3</sup> “A scientific man must be single-minded and sincere with himself. Otherwise, his lack of truth will melt away, at once. He can, therefore, hardly be otherwise than an honest, fair-minded man .... It is quite natural, therefore, that a young man who might develop into a scientific man should be a well-conducted person.” (CP 1.149)

opposed to the paralyzing costs of the other. This is not something we should take credit for; it is not the outcome of wise judgement. It is a deeply primitive enablement, this thing we call trial-and-error. Since right guesses are not self-announcing, it is perfectly in order that guesses be subjected to tests that respect our low opinion of undetected and uncorrected error. We have our limitations, needless to say. In all that we do and are capable of there isn't the slightest prospect of perfection. Even in the idealized abstraction known as the best that is humanly possible, the old adage holds its edge. Perfect is indeed the enemy of good.

Given the enduring advantage of having true beliefs, there would be reason to think that like advantage would accrue to their being true *of* that of which they are predicated. And, if that were so, it would in one fell-swoop support the whole case for what I will call "can't-help-it realism". It is agreed on all sides that our capacity for thinking-that is an enduring necessity of cognitive life, and that the ways of nature have brokered thinking-that and saying-that into profitable congress. The predicate 'true' is a valuable byproduct of that fertile union. It inheres in the structures of these arrangements that one cannot think-that without thinking-that the thought-that is actually the case. The same holds of saying-that. Short of self-refutation, one cannot say-that what one says-that is not the case. Talk of such kind transgresses the conventions of conversational implicature. It is a pragmatically inconsistent way of speaking. Pragmatic inconsistency is a violation of a Gricean conversational implicature. Grice was a year short of a century after Peirce's time, but his predecessor would have been much taken with *Studies in the Way of Words*, a point insufficiently acknowledged.<sup>4</sup> For all that anyone knows, perhaps nothing whatever is real. But it is not possible to say even *that* without self-refutation, sending that remark, too, to the lockjaw of present concern. The whole of cognitive life is predicated on our irrepressible can't-help-it realism, and there can be no gainsaying it without semantic obliteration. That, I would say, is a bias we can welcome, and its extirpation something we cannot live with.

### 3. *Gas-lighting*

It is comparatively uncommon for us to have a full understanding of things, even supposing this to be an attainable end in principle. By and large, we are not discommoded by our half-baked grasp of matters of importance and not defeated by the ignorance with which the human understanding must fruitfully co-exist.<sup>5</sup> Nearly everyone is well set-out for cognitive-management, but no good *at all* at running completely error-free lives. Peirce thinks that hypothesis-guessing in science is subject to the statistical chances that attend right-guessing. His own contributions to the statistical analysis of chance led him to the view that the chance of guessing right on any given occasion is extremely small. It has to do with the assumed size of the hypothesis-space. If there were five candidates, it would be possible to avoid guessing by giving each of the five its experimental day in court. But if the selection-space were huge, say practically infinite, guessing couldn't be avoided never mind the odds of one to million of getting it right. Peirce's own estimate for the general case was one in four hundred or so. That alone would encumber abduction with a sky-high likelihood of error. Nothing like this is born out by

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<sup>4</sup> Cambridge, MA: Harvard University Press, 1909.

<sup>5</sup> For the positive role of ignorance in cognitive prosperity, interested readers could consult my "Four grades of ignorance and how they nourish the cognitive economy", *Synthese*, 198 (2021), 3339-3368. Published online: 17 June 2019. Hereafter, *Ignorance* (2021).

what actually happens.<sup>6</sup> Peirce also acknowledges that we are good enough at guessing to make scientific advancement possible, and has the brass to add that we owe our success to the “spontaneous conjectures of instinctive reason” (CP 2. 443), the possession of “*lune rationale*” (MS 873 13-15) and “a certain power of divining the truth.” (*Ibid.* 638, 14-15). There is in such mystification no advance on the plain fact that we are good enough guessing to maintain guessing as standard practice.

Three things appear to have gone wrong here. Sometimes Peirce seems to think that good-guessing lacks adaptive advantage. But it is possible, and I think likely, that his reservations apply to the *science* of good-guessing rather than to its subject-matter. Another difficulty lies in his inclination to equate the size of hypothesis-spaces to the size of chance-spaces for predictive success in the calculus of probability. A third wrinkle is Peirce’s over-generalization of experimental procedures in play for the randomized controlled statistical testing of a new drug. Whatever the merits of a good day at Pfizer, still awaiting test are the hypotheses of the mathematics of elliptic curves, concerning which Pfizer’s measures are wholly unavailing. Each of these I judge to have been a misstep, reflecting no trace of what actually happens. But the point to stress here is that all talk spontaneous conjectures of instinctive reason is gas-lighting if taken as the scientific hypothesis of choice for the questions posed by our good-guessing successes. It would have all the information-content of the earnest assurance that we are good guessers because we are good at guessing.

Being as we are, it only stands to reason that we comport ourselves in ways that answer to our interests. In hypothesis-search mode it lies in our interest to find a hypothesis with good prospects of testing well. If that predisposition is over-used or dishonestly unfolded, the test results will be awful. So it lies in our interests to search honestly. The same holds for hypothesis-testing. Error, we know, is often hidden and sometimes extremely difficult to suss out, if possible at all. It cannot answer to the interests to which the present case is a response, to give good-looking hypotheses an easy pass. Here, too, it is only natural that tests be as exacting as honesty and resource-capacity allow.

Perhaps this would be the right place to sound a further friendly admonition about Peirce’s epistemological instincts. Suffice it to say for now that he is a determined exemplar of justificationism at its manic worst. Peirce not only demands that everything of epistemic attractiveness earn that distinction by dint of the justifications that bestow, but that there is nothing of epistemic worth in human cognitive life that lacks for a justification. I will come back to this in section six.

#### 4. *The good of ignorance-exposure*

I want to turn now to matters for which Peirce has an admirable “nose”. He has an excellent sense of the value of ignorance and also the necessity of making stuff up. We see here a considerable change of subject and tone. Young science does indeed sometimes advance our understanding, but not always as we normally take understanding to be. For when science enters the shock of the new, it quite often comes to an awareness of an ignorance it had not previously known itself to be in or with respect to what. Sometimes the importance of the breakthrough lies not in its demonstration of some favoured proposition of a system already in wide reflective equilibrium, but rather in what we could call its *Rumsfeld revelation*.

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<sup>6</sup> A recent shellacking of the sorry experiential state that modern neuroscience is in is administered by Matthew Cobb, *The Idea of Brain: A History*, London: Profile Books, 2020.

“There are known knowns. There are things we know we know. We also know that there known unknowns. That is to say, we know there are some things we do not know. But there are also unknown unknowns, the ones we don’t know that we don’t know”.<sup>7</sup>

Consider now the example of the 1995 proof of Fermat’s Last Theorem.<sup>8</sup> It proved the old theorem alright, but did so in a way that made *no* advance in our understanding of number theory. All the same, it

“considerably enriched the theory of elliptic curves, a theory that had its origins centuries ago in the study of planetary motion. There is still the question of [the theory’s] authority, but the understanding brought about by the proof is acknowledged in large part through its potentiality, *its opening up of new possibilities*.”<sup>9</sup>

The Wiles proof runs to 108 pages and, with the assistance of a SWAC computer, hinges on more than two dozen results from 1903 alone. Some of its background material dates from the Pythagorean triples of Babylonian times. How many of those few who have acquired a deep understanding of the Wiles would be able to recapitulate those two dozen critical results? The question is rhetorical. It answers itself. No one. Are we to say, then, that even those who pretend to a solid grasp of the Wiles that they actually don’t know what’s in it? That would be going too far. For the most part, the mechanisms of information-processing operate automatically, out of sight of the mind’s eye, beyond the reach of the heart’s command and unnegotiated by tongue or pen (or keystroke). This is the way in which the cognoscenti know the Wiles proof. The knowledge they have of it is in large part beyond the reach of full articulable recall, and it is not to be found in the 108 pages of the 1995 solution.<sup>10</sup> There is no room for it all in the front of the conscious mind or the pages of *Annals of Mathematics*. Moreover, most of what is cited in the proof is known by hearsay, and the origination of it was the product of multi-agents. This latter is reflected in the habit of citing its author as Wiles & Co. But, if restricted to his own preferred methods for experimental testing, Peirce would be bound to say that the Wiles proof confirmed nothing. Peirce’s methods are right for their specialized purposes, but in further fields of enquiry they are an unwelcome restraint of trade. Indeed a driving factor in Scientific Progress is the talent possessed by its great pioneers for making up breakthrough ideas – not just guessing at their undiscovered pre-existence – but creating them *ab initio*. How, then, are they to be tested?

### 5. Truth-making in science

The non-empirical sciences are an interesting case. They raise this question about testing. How indeed are *their* results tested?<sup>11</sup> The (very) short answer is that they are tested in the only way open to them. They are tested by their market-share in cognitive economies of expert

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<sup>7</sup> Donald Rumsfeld, American Secretary of Defense news briefing, February 12, 2002.

<sup>8</sup> As every High-Schooler knows, the theorem asserts that no three positive integers  $x$ ,  $y$ ,  $z$  satisfy the condition that  $x^n + y^n = z^n$  for values of  $n$  greater than 2. Its spectacular proof lies in Andrew Wiles, “Modular elliptic curves and Fermat’s Last Theorem”, *Annals of Mathematics*, 141 (1995), 443-551.

<sup>9</sup> Akihiro Kanamori, “Mathematical knowledge: Motley and complexity of proof”, *Annals of the Japan Association for Philosophy of Science*, 31 (2013), 21-35; p. 32, emphasis mine. Further details can be found in *Ignorance* (2021). The present state of Sinichi Mochizuki’s Inter-universal Teichmüller theory is also briefly discussed there.

<sup>10</sup> For more on the role and importance of the tacit and implicit, interested readers could consult my “The role of the common in cognitive prosperity: Our command of the unspeakable and unwritable,” *Logica Universalis*, 2021, DOI 10.1007/s11787-021-00289-y.

<sup>11</sup> This is discussed in my *Paradox an Paraconsistency: Conflict-Resolution in the Abstract Sciences*, Cambridge: Cambridge University Press, 2003.

opinion in wide reflective equilibrium.<sup>12</sup> Even more striking is science's strong affection for conceptual licence. It is a point at which we find Peirce somewhat at sixes and sevens, for this tough-minded experimentalist is also wholly at one with the idea that conceptual licence is essential for health even in the deductive sciences. See what he says at CP 2.222:

“For every symbol is a living thing, in a very strict sense that is no mere figure of speech. The body of symbol changes slowly, but its meaning inevitably incorporates new elements and throws off old ones.” (CP 2. 222)

Lest there be any doubt of his meaning, here is he in 1907:

“Almost all the theoretic inferences are positively *creative*, that is, they create, not existent things, but *entia ratione* which are quite as real.” (MS 773, 2-3)

Here is an *essential* insight for any adequate understanding of human cognitive practice, and it puts fully deserved pressure on the distinction between a concept's growth to semantic maturity and a concept's retirement in favour of a replacement concept which retains the old name.

Without the solitary Riemann in his *Habilitationschrift* of 1854, we would not have had the n-dimensional manifold and, without that, could not have had differential topology or differential geometry. The n-dimensional manifold was called into play in one of the most fruitful hypothesis-searches of modern mathematics, giving birth to another idea gravid with young truth.<sup>13</sup> Unless it finds a principled place for the lone genius, no account of hypothetical reasoning can fulfill its promise. Taken together, the importance of CP 2. 222, MS 638, 14-15 and “Über die Hypothesen, welche der Geometrie zu Grunde liegen” is difficult to overestimate.<sup>14</sup> Not only does Peirce acknowledge and applaud as a matter-of-course the reconceptualizations of concept-users' own *communal* doing, he also allows for the solo-authored creation of concepts which had not been known of beforehand.<sup>15</sup> Riemann fits this bill perfectly.

Peirce clearly acknowledges our capacity to create new ideas and ensconce them in innovative hypotheses. But Peirce is a realist, and so is Riemann, and so for that matter am I. (We can't help it.) Here, too, questions arise, good ones. One is whether we have at hand a principled way of determining the difference between opening up a concept to a fuller understanding of it and changing the subject to a new concept under the abandoned concept's established name (as Peirce thought the “instrumentalists” had done with the concept of pragmatism). A tougher question is whether we have the means of determining how much a concept's intelligibility can be enlarged without violating its haecceity.<sup>16</sup> Even tougher still is the problem posed for realism by making strange stuff up. Given that the latter is indispensable to the advancement of science, most notably mathematical sciences, we are faced with the task of determining whether a mathematical realist can acquiesce to mathematical creativity without

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<sup>12</sup> An excellent treatment is William G. Lycan, *On Evidence in Philosophy*, New York: Oxford University Press, 2019.

<sup>13</sup> Without differential geometry there wouldn't be the tensor calculus on Riemannian manifolds which opened the way to general relativity theory.

<sup>14</sup> Bernhard Riemann, *Gesammelte Mathematische Werke und Wissenschaftlicher Nachlass*, H. Weber, editor, Leipzig: Druck und Verlag van B. G. Teubner, 1876.

<sup>15</sup> This is not to preclude what is frequently the case with newly made concepts. For Frege, Dedekind, Russell, and Riemann too, a brand new concept can be a unique synthesis some of whose elements reflect aspects of concepts presently at hand.

<sup>16</sup> Further details can be found in my “Does changing the subject from A to B really enlarge our understanding of A?” *Logic Journal of the IGPL*, 24 (2016), 456-480.

losing his licence to practice realism. Can he do so without dishonouring the sentiments of epigraph number two? Since there is no room for it here, I must defer this to a later occasion.<sup>17</sup>

So far we have been following Peirce in finding abduction's motivation in something that puzzles or bewilders us, something that is not quite a matter of course. This was not Riemann's situation. He was faced with a well-known difficulty in complex function theory. It wasn't so much that the problem seemed somehow off-course. The problem was that the analysis of particular kinds of complex functions by means of lengthy and irksome calculations had impeded the articulation of a *general* theory of complex functions. There is nothing remotely puzzling about the *fact* of long and tiresome calculations in mathematics. They are matters of course there. And it wasn't until Riemann saw the light that anything significant was in any way out of the ordinary or a tax on comprehension. Gauss, his teacher and only supporter, couldn't understand the result. If somehow he saw the good of it, he did so tacitly and implicitly.

There is something valuable to be learned from this. From the very beginning, Peirce has misconceived the motivational structure of hypothetical reasoning. He was entirely right to see the deep importance of it and its pervasiveness in cognitive economies. But he was over-focused on the peculiar and the out-of-step in ascertaining how it stirs the hypothesis-engaging engines of scientific practice. Hypothetical reasoning has at least as much to do with rising beyond the commonplace unknowns as rising above the odd or off-course ones. This relieves hypothesis-selection of a burden it needn't always bear, the burden of finding hypotheses whose truth would restore choppy waters to normalcy. This, of course, is not what always happens.

It is also not as well-known as it deserves to be that Peirce's conditions for abduction considerably exceed responses to bewildering scientific facts, and take on the large justificatory burden for all of Science's many provinces.<sup>18</sup> Let us close the proceedings with a brief glimpse.

#### 6. *A full-service logic for deductive consequence*

Peirce brings to logical theory the instincts of what I earlier called a manic justificationism. There is nothing in sight of his logic that escapes the necessity of justification. A standing problem for justificationists is where to draw the line against infinite regress. From the early 1880s onwards Peirce paid that matter no mind. He acknowledged the question but shrugged it off, arguing that retroduction is self-justified. For given retroduction's record of success, it must be allowed that it itself fulfills its own conditions. Peirce's attachment to justification may be manic, but his conception of it is weak to the point of nullity. For no justificationist who knows his onions would concede to Peirce the assumption of retroduction's cognitive *success* without seeing the justifying evidence. I see here an undisclosed and possibly inadvertent openness to what I have called the convergence of the normative on the normal. The N-N convergence thesis is a default. In the absence of reasons to think otherwise, it provides that how to reason in the normatively right way on some given occasion is to reason in the way that normally prevails on such occasions. Expressed this way, it is little more than a rule of thumb. But it conveys the general idea and gives enough of the hang of things to be getting on with. I will simply add that it is the *normal* that wears the trousers here. The right way to reason about

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<sup>17</sup> It is taken up in my *Deduction and its Discontents: The Three Faces of Logical Consequence*, forthcoming in 2022; chapter 9 "Realist truth-making in creative mathematics". Hereafter, *Discontents* (2022).

<sup>18</sup> Recommended reading is Ahti-Veikko Pietarinen and Francesco Bellucci, "New light on Peirce's conceptions of retroduction, deduction, and scientific reasoning", *International Studies in the Philosophy of Science*, 28 (2014), 353-373.

thermodynamics is to reason in the ways that are normal there. N-N convergence is a rival of justificationism, not a specimen of it. I am inclined to think that to some extent, Peirce is a justificationist in name only.

A rather remarkable feature of the three components of Peirce's Logical critic is that each is self-justifying and also justified deductively. Of the three, only deduction has a sole source of justification, namely itself. (CP 2.786, MS 293). Induction is justified deductively and inductively (WI 280-283, MS 328.) Retrodution is justified deductively (CP 5.146, MS 293), inductively (CP 2.726, MS 630) and retroductively (WI 280-281, MS 328). All this in spite of the fact that

“While Abductive and Inductive reasoning are utterly irreducible, either to the other, or to Deduction, or Deduction to either of them, yet the only *rationale* of these methods is essentially Deductive or Necessary. If then we can state wherein the validity of Deductive reasoning lies, we shall have defined the foundation of logical goodness of whatever kind.” (CP 5.146)

This is not quite the justificationist debauch it appears to be. Peirce takes steps to restore order by drawing yet another distinction, this time between *logica utens*, and *logica docens*. Like the English word ‘logic’, ‘logica’ is ambiguous as between a kind of practice and a theoretical enquiry into the nature and good functioning of the practice (hence the emphasis on *rationale* in CP 5.146.) This distinction in turn bisects the *utens-docens* divide. It is less clear in this instance just what is being distinguished, but a reasonable candidate is that between untutored cognitive practice, the practice of Everyman, and the informed practices of the high learning. Suppose that we had to hand the best theory of the cognitive practices of Everyman and also the theory that best accounted for the practices of the highly learned. What are the possibilities of cross-over? Would the theory of the learned also fare well in Everyman's waters? Could the theory of Everyman adjust itself to the particulars of learned practice? In one way, this is an important question. For it puts enormous pressure on what is to count as a *rule of reasoning*. I will come back to this after a final mention of two further distinctions, the one dividing reasoning into the corollarial and the theorematic (MS L 75, 95, NEM), and the other breaking deduction into two parts. Concerning deduction, its first part, analysis, explicates the concepts embedded in the premisses of the reasoning. The second part encompasses demonstration, which regulates the derivation of what follows logically from them. (MS 842, 35, 43, MS 843, 44). Analysis marks a significant departure in our understanding of deductive inference, for Peirce is saying that the deducer is not only free, but has a duty, to *reconceptualize* the premisses in ways that loads the input with new information that's passed on to the output. By these means, Peirce wants to free valid deductive inference from the charge of triviality. Frege did the same sort of thing with ‘constructive’ definitions and diagrammatic proof. In each case, it was a fevered response to a longstanding charge that there is nothing new to learn from deductively derived conclusions. On the other hand, the corollarial-theorematic distinction is a like adaptation of ancient provisions for direct and indirect proof.

In the abstract of this talk I remarked upon the irony that deductive logic's most trinitarian-minded theorist missed the threefoldness of the very subject of his preoccupation. It is one thing for propositions to *have* a given proposition as a deductive consequence. It is another thing for someone to *spot* that proposition as a consequence of those antecedents. It is yet a third thing for someone to *draw* that proposition as a consequence of those antecedents. The first, consequence-having (whose converse is deductive entailment), is a binary relation over truth-valued formulas. The second, consequence-spotting, is a ternary relation over truth-valued

formulas and cognitive beings like us. When a spotting occurs, the spotter knows that the consequence is indeed entailed by its antecedents. The third, consequence-drawing, is also a ternary relation over truth-valued formulas and a cognitive agent. When a drawing occurs, the agent is caused to know that the true entailment of that proposition by those same premisses is an entailment from true antecedents and, on that basis, the agent detaches the conclusion for such uses as he may see fit. In one of those cases, he draws an inference and installs the conclusion in his repertoire of beliefs. There are other inequivalent options which needn't detain us here.<sup>19</sup> The triviality accusation now has a solution. We can find it, for example, in conditional proofs, in which the spotting of the consequence entailed by its premisses is enabled by the introduction of further steps of reasoning in accordance with the proof rules when properly invoked. It is prudent to keep it in mind that, comparatively speaking, hardly any of our good reasoning is deductively valid. But when validity is needed, it is a considerable boost of good outcomes to use validity to such advantage as is in it to provide. Those attending to the what-actually-happens rule would see this in a flash. This leaves Peirce oddly positioned. Either he is a fact-contravening deductivist about reasoning, or he is, more plausibly, a deductivist about the logical theory of reasoning of all kinds.

There are certain questions about reasoning which cannot be asked of consequence-having. Consequence-having is a propositional relation only, and the question whether it is good reasoning is a clumsy category-mistake. One *cannot* ask of consequence-having whether it is normative for reasoning. Of all the conditions that having must meet, none is a "necessary condition of good reasoning". Truth-preservation is a necessary condition of *valid* reasoning, but validity in turn is not a necessity for the cognitive good. If we were to consult any of the going proof-theories of the present day, we would see that its proof-rules meet the burden of truth-preservation, but leave the rest of proof-goodness wide-open. I suppose that modus ponens is the best-known of this august number. But the last thing we could say about MP is that it is an unrestrictedly good rule of inference. Gilbert Harman made a big noise about this in 1970, and it reflects badly on logicians how slightly they have attended to his insight.<sup>20</sup> Suppose that you believe that this proposition entails that one, but have not taken a position on either of them. If it now comes to you that the first one is true, you have cause to turn your mind to the other. Should you now see that the other does not rise to what you require for attestation, three options await. I leave them as an exercise for the interested reader. The pith of Harman's shrewd observation lies in his subtitle. Although writing about induction, his lesson applies equally to deduction. Theories of reasoning belong to *epistemology* (or, as I would prefer, to cognitive economics), whereas theories of the relation belongs to *logic*.<sup>21</sup> This nicely catches the deep categorial divide between having consequences, and spotting and drawing them. It would be safe odds that not one of the principal findings of Peirce's logic of deduction would shed any light on deduction itself. And spotting and drawing would be better served by sending Peirce's rich and tangled undertakings to cognitive economics, for streamlining and refinement in ways that withhold normative high-handedness until all the good has been squeezed out of the empirically discernible behavioural regularities of our spotting and drawing ways. So let us now come

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<sup>19</sup> We should not, however, slight their differences. The most important are argument-closure, theorem-derivation, and knowledge-producing demonstrative closure. There is further discussion in *Discontents* (2022).

<sup>20</sup> Gilbert Harman, "Induction: A discussion of the relevance of the theory of knowledge to the theory of induction", in Marshall Swain, editor, *Induction, Acceptance and Rational Belief*, pages 83-89, Dordrecht: Reidel, 1970.

<sup>21</sup> I think it likely that Peirce would have surrendered the logic of consequence-having to the tender mercies of mathematics. It wouldn't have worked. Following his father, Peirce took mathematics to be the science that draws necessary *conclusions*.

together in interdisciplinary harmony with cognitive science and the logic of entailment in support of a full-service theory of logical consequence. The target is deserving and we should mend our ways to enlarge the prospects of its capture. One thing is already clear. Hypothesizing and making things up will have a large presence there.\*

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