

LOGIC, MATHEMATICS, AND THE NATURAL SCIENCES

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1 INTRODUCTION

This paper is in two parts. The first part sets out a ‘minimalist’ position regarding the correct logic by means of which one may pursue the hypothetico-deductivist method in natural science. It is argued that *intuitionistic relevant logic (IR)* is adequate. A requirement of reflexive stability is that such an argument should be conducted within the confines of the very logic whose methodological adequacy is to be established. The second part of the paper (§6) addresses an objection in principle that has been raised by John Burgess (2005a, 2005b) to the reflexive stability of the argument for *IR*.

2 THE QUESTION OF THE ADEQUACY OF CONSTRUCTIVE METHODS FOR HYPOTHETICO-DEDUCTIVISM IN NATURAL SCIENCE

Dummett (1991, at pp. 320–1) writes

Investigation might reveal that a constructivist version of a mathematical theory was perfectly adequate for the applications made of it within natural science. . . . These questions have scarcely been raised, let alone answered, by either mathematicians, philosophers or physicists.

This claim of Dummett’s is not quite accurate. Investigation had already revealed, at least by the time he published the claim, that a constructivist version of a mathematical theory is adequate for all the applications to be made of the theory within natural science.¹ Or, to be more precise: investigation had already revealed that, in the overall context of the hypothetico-deductive method in natural science, constructivist *logical reasoning* is adequate for the applications to be made even of *classical* mathematics. Indeed, the investigation reveals exactly how and why

¹See [Tennant, 1987], Chapter 18, ‘Intuitionistic Relevant Logic is Adequate for Popperian Science’. Those ideas were developed further in [Tennant, 1997], Chapters 12 and 13. The original source of publication of the treatment in Chapter 18 of [Tennant, 1987] is [Tennant, 1985].

one does not err, even by constructivist lights, in deriving and applying strictly classical mathematical theorems; and how and why, for all the use that might be made of strictly classical mathematics, it is a ladder that the constructivist can kick away and ultimately forswear.

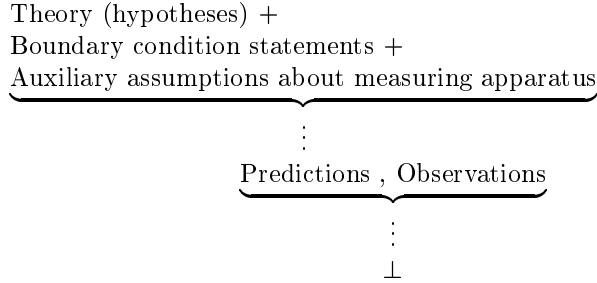
This claim is complicated, and perhaps surprising. The aim here is to make it clearer and completely plausible.

The basic idea is that, in so far as one is concerned to be able to produce all possible refutations of empirical theories, the underlying logic can, without loss of generality, be taken to be very weak indeed. Either Johansson's system of minimal logic or the author's system of intuitionistic relevant logic will do. The mathematical theorems that are applied in the course of physical theorizing turn out, on this account, to be deductive halfway houses: they stand as conclusions of purely mathematical arguments (often strictly classical ones), but stand as premises in the applied scientific context within which an empirical refutation is formulated. Upon constructivizing (and relevantizing) the *reductio* involved in the refutation of an empirical theory, the erstwhile mathematical theorem might be elided. Yet the mathematical axioms will still be in force, helping to sustain the overall *reductio*. And the logic of the *reductio*, is, to repeat, weak.

This provides a satisfactory affirmative answer to the question whether 'a constructivist version of a mathematical theory [i]s perfectly adequate for the applications made of it within natural science'. It also affords a constructive explanation of why it is that one can trust strictly classical mathematics when it is applied in the context of empirical theorizing. All that the classical aspect of the underlying logic is securing is a possibly shorter, more convenient proof. But, as the overall result shows, empirical refutations rely only on the constructive content of the underlying mathematical axioms. Strictly classical mathematical results that are applied within scientific theorizing are—to repeat—ladders that can be kicked away, once they have served their expeditious purpose (in securing the predictions that are eventually refuted).

3 REFUTATION OF EMPIRICAL THEORIES

When an explicitly formulated theory fails a well-defined and well-controlled experimental test, it is refuted. Moreover, *so long as we trust the evidence and the controls that applied to the experimental conditions*, it stays refuted. Note that by saying that it is the theory (that is, the collection of high-level hypotheses) that has failed the test, we are already supposing that the Poincaré–Duhem–Quine problem has been resolved. That is, we are agreed that the boundary condition statements were true (the experiment was properly controlled) and that the experimental observations were correct (we made no mistakes in reading our measuring apparatus, and it functioned correctly, etc.). Since what was observed conflicted with what was predicted (by flawless logical reasoning) from the theory and the boundary condition statements, we have concluded that it was the theory that was at fault:



If we do not revise our evaluations of past evidential statements in the light of any new ones, then future consistent extensions of present evidence will leave the refuted theory refuted.

The slogan ‘Once proved, always proved’ in mathematics can be explicitly restricted (without loss) so as to encompass only *consistent* extensions of our present axiomatic basis. That is to say, there is by default a consistency requirement on the axiomatic basis of mathematics. In the logico-mathematical case: if axioms X enable us to prove theorem A , then future axioms $X+X'$ —which of course we assume to be consistent—enable us to prove theorem A .

There is also by default a similar consistency requirement on whatever is supposed to constitute the ‘total evidence’ for or against any empirical theory. In every domain, consistency is a prerequisite for truth.

The slogan ‘Once empirically refuted, always empirically refuted’, can be justified analogously as follows: if evidence E refutes theory T , then future evidence $(E+E')$ —which of course we assume to be consistent—also refutes theory T .

With a logico-mathematical assertion P there can be neither retraction nor revision, unless some mistake is revealed in what had been taken as a proof of P . ‘Once proved, always proved’ is the motto here. Knowledge (properly pedigreed) is strictly cumulative over time. And the deductive logic governing such assertions is *monotonic on its premises*. That is, if X is a subset of Y , and X logically implies A , then Y logically implies A . In the logico-mathematical case, the subset X of premises that consistently expands to become the set Y may be thought of as the foundational basis for one’s assertions. Such sets provide the ultimate grounding or support for one’s conclusions A . When X consists of mathematical axioms only, we say that A is a mathematical theorem. (When X is empty, we say that A is a logical theorem.) Our certainty in the mathematical axioms transmits, via logical deduction, to the theorems as well. Certainty transmits along lines of logical implication, from premises to conclusions.

The deductive logic governing empirical claims based on empirical evidence is *also* monotonic, *despite* the fact that the history of scientific theorizing may reveal ‘oscillations’ in the fortunes of either an empirical hypothesis, or an evidential statement. The claim about monotonicity here is *not* to be interpreted as resting on a naïve epistemological analogy between mathematical axioms and evidential statements. *Of course* the former are far more certain than are the lat-

ter. Indeed, mathematical axioms are often *self*-evident and necessary; whereas evidential statements are never so. The crucial difference between the two cases, however, is this. In the empirical case, unlike the mathematical case, the direction of logical implication (with respect to which such monotonicity holds) runs *counter to* the direction of evidential support. When we ‘base’ a (consistent) scientific theory X on empirical evidence E (consisting of true observation statements) we require (roughly)² that X should logically imply E , not vice versa. This logical implication is, to be sure, monotonic on the premises. But the premises X (forming our scientific theory) are what, in this context, require the support! And it is the conclusion E that is supposed to provide it. The support that E affords X is not just a matter of X ’s logically implying E . The latter is necessary, but not sufficient, for the empirical evidence E to support the empirical theory X .

Another condition is that X should (perhaps in conjunction with further assumptions B) logically imply further observational conclusions E^* . The observation sentences E^* should be ones concerning which we have as yet no firm opinion. Such observational conclusions E^* logically inferred from X and B are the predictions of X (modulo B) that can be used to test X . The test will take place by arranging circumstances for which B holds, and then seeing whether E^* holds too. The conditions B are called *boundary conditions* for such experimental testing. When the experiment is conducted, there are, ideally, two outcomes: E^* is seen to hold, or E^* is seen not to hold. In the former case, the theory X is corroborated, for X ’s prediction (that E will hold in circumstances B) is borne out. Moreover, E will also have been *explained* by X in the circumstances B . In the latter case, where E is seen not to hold, the premises X are collectively refuted. We shall return presently to consider how decisive such refutation may be for any particular member of X . Just which members of X ought to be retracted in the light of such a refutation of X as a whole is in general a rather vexed question.

A third necessary condition for the empirical evidence E to support the empirical theory X is that the evidence within E should be gathered from various domains, and that the formulation of X should be economical and abstract enough not to reflect such diversity explicitly, but to do so only implicitly, by way of deductive application via the boundary conditions B . That is, X should provide *unifying explanations of disparate phenomena*. X should be of wide evidential scope. An example would be the way that Newtonian dynamics and the theory of gravitation (as such X) can be applied to provide explanations of such diverse phenomena as apples falling from trees, projectiles following roughly parabolic trajectories, spinning tops precessing, ocean tides correlating with the position of the Moon, the motions of pendula, the orbits of the planets, the efficacy of aerofoils, the vibrations of a plucked string, the formation of sand-dunes, ripples on a pond,

²Quine would prefer to say that we require X to yield ‘pegged observational conditionals’ of the form ‘if O_1 and ... and O_n then O ’. Some of the evidence E has to be accepted at face value, in the form of various O_i . The rest of E would then have to be covered via detachment using these conditionals. Obviously there will be a premium on minimizing the former and maximizing the latter. Cf. [Quine, 1990].

sonic booms, and many other such phenomena.

A fourth necessary condition (which is related to the third) is that X should be *simple*. This is a notoriously difficult virtue to analyse or explicate, but scientists do have strong intuitions about whether particular theories are simple. Theories can fail to be simple in various ways: they can be too *ad hoc*; they can amount to little more than restatement of the evidence; they can postulate too many kinds of hidden entity to perform the explanatory job at hand; they can extrapolate from their data points in ‘unsmooth’ ways. We shall not be too concerned here to detain the reader with any attempt to explicate simplicity further. It is not important, for present purposes, that one be able to do so.

We were considering the logic of scientific explanation, and how the direction of theoretical explanation runs counter to the direction of evidential support. Theory X in circumstances B explains evidence E only if E provides evidential support, in circumstances B , for the theory X . And this amounts to no more than E following logically from the conjunction of X with B . We do not intend to say anything about how the evidence mounts up, or about how certain evidential statements can be more important than others. We do not offer the prospect of any further metatheoretical development of the relation of support, either in the form ‘ E_1 would be better evidence for X than would E_2 ’ or in the form ‘ E would be better evidence for X_1 than it would be for X_2 ’. Again, this is not needed for present purposes. Nor do we intend to say anything about confirmation or probabilification of hypotheses by evidence, and Bayesian conditionalization.

It is enough to confine our treatment to the strictly hypothetico-deductive model of explanation. For it is clear that it is a workable model in so far as it goes. The extent to which it does not accommodate all the intuitions that scientists and methodologists may have about how theories relate to the evidence does not concern us. All that is important is that one recognize the fundamental features captured by the hypothetico-deductive model. It is quite conceivable that there should be thinkers and reasoners who exploited and relied on those features and those features alone. Their thought about the external world and its deep regularities, and their quest for theoretical explanations of empirical phenomena, could be just as cogent and urgent as ours. Moreover, their scientific successes could be just as impressive as ours, and they could use their theories just as we do, as guides to life, as a means to anticipate courses of events, as considerations in choice of future actions, and as sources of technological innovation.

We are *not* trying to provide a full account of human scientific rationality. That would be way beyond the scope of our concerns. Rather, we are isolating an essential logical core to our competence as empirical theorizers—a core which *could*, arguably, serve as the total competence of some species of rational agent, in so far as empirical theorizing is concerned, even if it falls short as an account of our own full competence in that regard.

Given this strictly limited and modest concern, we are accordingly under no obligation to essay upon the abductive ‘logic’ of *discovery* or of scientific invention. We offer no account of how a scientific intellect, confronted with a range of

evidence, would come up with a high-level theory that successfully explains it and that can be tested against the further predictions that it makes. We are content to leave that process mysterious and untouched. We are interested only in what happens, logically, after the theory has been formulated. We are interested here only in the deductive logic of theory *testing*.

Similarly, we are content to deal only with a very regimented language, namely, the language of first-order logic. Even if this language should prove (*pace* Quine) to be inadequate for the expression of all our thought about the empirical world, it is nevertheless clear that there can be systematic thought, framed in a first-order language, about the empirical world, and that the essential features of the hypothetico-deductive model of explanation will be in place when the logical deducibility relation³ in that language is taken as the relation involved in prediction and explanation.

Every logically contingent statement that ventures logically beyond the present observational evidence is vulnerable to empirical refutation. But usually it is vulnerable in the company of others. When we have derived absurdity from a set of evidential statements in conjunction with a set of conjectural statements, we have many ways of proceeding from there. We may retract one or more of the evidential statements; and likewise with the conjectural statements. In general, we might be able to contract to any one of several consistent subsets of (evidence+conjectures). How we respond to refutations (proofs of absurdity) is, again, a matter more of the ‘logic’ of scientific discovery than it is a matter of the logic of testing. The refutation consummates the test; something has failed. Whether the failure is to be located among the conjectures venturing beyond the evidence, or in the so-called ‘evidence’ itself, is a matter that need not detain us. Let Δ contain all the premises involved in the refutation. Thus Δ embraces conjectures (hypotheses) and evidential statements alike. The refutation of Δ is decisive, in the sense that any further evidence Γ leaves it untouched. If, now, we hold to the evidential statements in Δ , the hypotheses forming the rest of Δ stay jointly refuted, despite the accumulation of the new evidence Γ . So: as the tribunal of experience recruits more members (observation statements held true) and speaks with one voice (is logically consistent), we can say of the theories that founder on this evidence: once refuted, always refuted.

4 THE ANTI-REALIST CONSTRUAL OF EMPIRICAL CLAIMS THAT CANNOT BE PROVED

The anti-realist content of the unprovable empirical generalization that all F 's are G 's is really this: *Nature will not confound the assertion that all F 's are G 's*. That is, Nature will not yield a case of an F that is not a G . In general, the anti-realist content of any empirical assertion P of which, by virtue of its general or hypothetical nature, we can say *a priori* that it cannot admit of proof,

³Or perhaps some suitably constrained subrelation thereof.

is: *Nature will not refute P* .⁴ For such a belief will only ever be entertained as an explanatory hypothesis, and as a generator of predictions; and, as such, is subject only to refutation, not proof. Refutation, however, would always be modulo some set of assumptions that were firmer than the belief P in question. The Poincaré–Duhem–Quine problem is simply that of how we focus on the particular P that we might take to have been refuted, once we have a disproof of a set of assumptions containing it. But that is a problem for applications. All that our deductive logic can be expected to provide is the various disproofs of these sets of assumptions as the intellectual opportunity or need arises. Now to this end, the construal of any (unprovable) empirical assertion P as being to the effect that Nature will not refute P makes the system IR of intuitionistic relevant logic perfectly adequate for empirical science, with its deductive testing of explanatory and predictive hypotheses against the evidence. If we agree that we may turn any claim of the form $\forall x(Fx \rightarrow Gx)$ into the corresponding form $\neg\exists x(Fx \wedge \neg Gx)$, then we can supply in IR all the disproofs needed for empirical science. For, in the language based on \neg , \wedge , \vee , \rightarrow , and \exists , we have the metatheorem:

METATHEOREM 1. If Δ can be disproved in classical logic, then Δ can be disproved in IR .

That is all very well, says the objector; but what about the case where one is drawing out a logical consequence in the form of a prediction which has not yet been refuted? How does the anti-realist using only IR match that? The answer is that if one has derived the prediction P from the assumptions Δ using classical logic, then in IR one can at least derive $\neg\neg P$ from Δ . $\neg\neg P$ is the regimentation of ‘Nature will not refute P ’. And this is the appropriate propositional attitude to have, according to the anti-realist, towards the prediction P , which cannot, on the basis of the present evidence, admit of proof. The ‘proof’ by means of which we make the prediction P will of course involve as undischarged assumptions the higher-level hypotheses of our explanatory empirical theory; and since these assumptions cannot admit of proof, nothing that depends on them for ‘proof’ is really proved. The only genuine proof one could ever have for a prediction P would be based on various atomic axioms that will only be available in the future, once events have run their course. Our theories can enjoy no proof in the present, but at best withstand the test of time. If and when the countervailing evidence comes in the form of $\neg P$, however, then there is nothing to choose between the following two logical passages:

⁴The classicist is committed to this immediately by maintaining that ‘All F ’s are G ’s’ is logically equivalent (in particular, entailed by) ‘It is not the case that some F is not a G ’. Popper, for example, urges that one use the latter *in place of* the former when regimenting our scientific theories. For the anti-realist, for whom ‘It is not the case that some F is not a G ’ does not in general entail ‘All F ’s are G ’s’, greater discrimination is called for. Thus when the latter form of words is used for the formulation of a scientific hypothesis, our proposed reading secures the logical licence that is generally withheld.

$$\begin{array}{ccc}
 \Delta & & \Delta \\
 \vdots \text{ via classical logic} & & \text{via } IR \quad \vdots \\
 \frac{\neg P \quad P}{\perp} & & \frac{\neg\neg P \quad \neg P}{\perp}
 \end{array}$$

Another objection worth disposing of here is the following allegation of circularity: ‘You say that the content of an empirical assertion P of which, by virtue of its general or hypothetical nature, we can say *a priori* that it cannot admit of proof, is: *Nature will not refute P* . But to understand the latter, we need first to understand P itself.’ This objection implies that our account is unable to provide the content P independently, so that it can later be embedded in the context ‘Nature will not refute ...’.

But this is to misunderstand the overall division of conceptual labour. The content of P is already available via composition out of the meanings of its constituent expressions. The latter, in turn, have had their meanings conferred on them by the inferential liaisons that they enjoy within the empirical theory (if they are empirical terms) or by the rules of inference that govern them (if they are logico-mathematical terms). That yields us the *sentential content* P . Now, when we advance to consider an *assertion of P* , we are free to append a further analysans to P , in order to capture the special illocutionary force involved.

5 THE FATE OF STRICTLY CLASSICAL THEOREMS OF APPLIED MATHEMATICS

Past debate over the adequacy of constructive or intuitionistic mathematics for natural science has focused on the question whether particular theorems finding application in natural science can be proved by strictly constructive or intuitionistic means. A standard way of alleging the inadequacy of constructive methods would be as follows:

The classical mathematical theorem φ finds application within such-and-such branch of empirical science. (It is used to explain phenomena, derive predictions, etc.) But φ is not a theorem of constructive mathematics. Hence constructive mathematics is inadequate for the demands of actual, current, scientific practice.

The constructivist might challenge the claim of constructive unprovability (as, for example, [Bridges and Richman, 1999] *contra* [Hellman, 1993]). But even if that were to fail, another possible response by the constructivist might then be to show that some constructive theorem ‘close enough’ in spirit to φ might nevertheless suffice, given certain circumstances revealed by inspection of the mathematics in question. For example, it might be argued that it would suffice for predictive purposes if one could approximate the classicist’s real values by means of intuitionistically ratifiable rational values that are accurate to within a reasonable margin.

The situation regarding constructivism is analogous to that regarding relevantism. Relevant logicians have held out the prospect of ‘relevantizing’ mathematics, that is, of deriving all mathematical theorems from the axioms by means of their favored system of (classical) relevant logic. The underlying contention is that intuitive mathematical reasoning is always fundamentally ‘relevant’, hence that its formalization in a suitably chosen relevant logic should not result in any significant loss of mathematical theorems. The problem, however, especially for proponents of systems of relevant logic lying in the neighborhood of the system R of Anderson and Belnap, is that they could relevantize theories such as arithmetic only piecemeal. They lacked any general metatheorem to the effect that every classically proved theorem could be proved from the axioms by means of relevant logic.

The difference, in the intuitionistic case, is that of course we know that certain theorems of classical mathematics will be lost upon adoption of an intuitionistic or constructive logical system. So the intuitionist who wishes to claim that there is no great loss as far as *applied* mathematics is concerned needs to take a closer look at the exact logical needs of the scientific theorist seeking to apply mathematical theorems in pursuit of empirical predictions that may be tested by observation and experiment.

The line of argument proposed in this paper is intended to accomplish two aims. First, it is intended to bypass or circumvent the sort of dispute between the classicist and the constructivist that has taken place between Hellman and Bridges. As Bridges (1999, at p. 440) puts it,

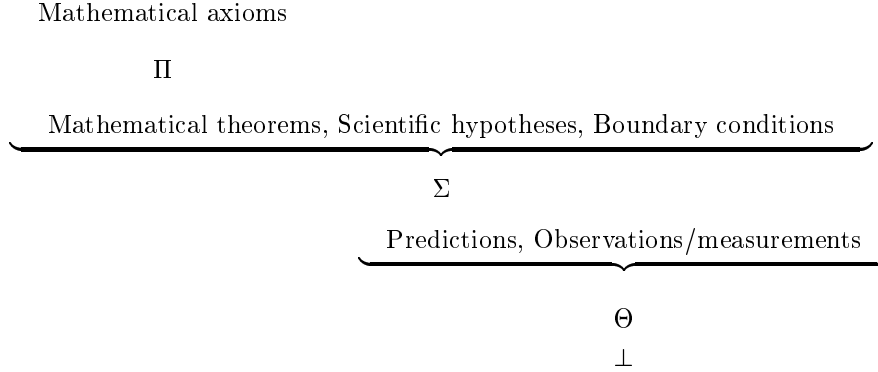
constructive mathematics is none other than mathematics carried out with intuitionistic logic.

And, one might add (by way of generalizing this dictum),

constructive hypothetico-deductive theory-testing is none other than hypothetico-deductive theory-testing carried out with intuitionistic logic.

The second aim is to show in one fell swoop, rather than piecemeal, that there is absolutely no loss of intuitionistic (resp. classical) mathematical content upon adopting the relevant logic IR (resp. CR).

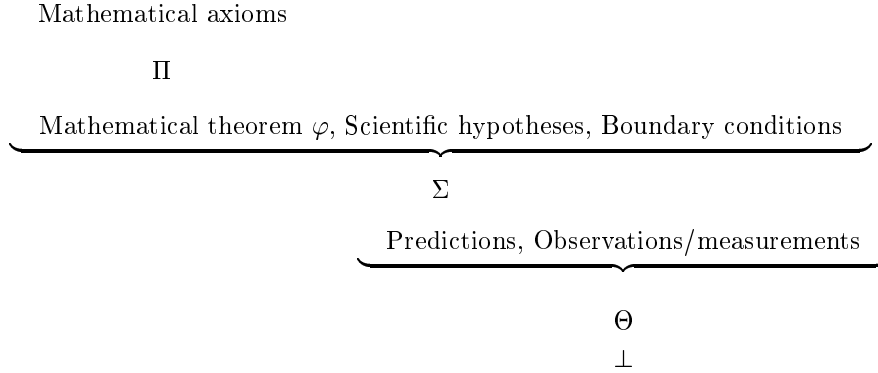
Let us take a closer look, then, at the logical needs of the scientific theorist. The aim of the hypothetico-deductivist is to derive, from the scientific hypotheses being tested, predictions that may conflict with, or disagree with, or contradict one’s observations and measurements. The overall structure of such a *reductio ad absurdum* is as follows:



A mathematical theorem might be needed, say, to provide a characteristic form of solution for the particular kind of differential equation that might be used in the statement of a scientific hypothesis. Such an hypothesis might deal with the time-evolution of various co-varying physical magnitudes, stating in mathematical terms the precise relationships among them, and how they change with respect to one another. The strictly mathematical proofs Π furnish the mathematical theorems (from one's mathematical axioms) that are desired for such applications within natural science. From the point of view of the practising scientist, unconcerned with foundational matters in mathematics, it is good to have a ready supply of mathematical theorems 'off the shelf', whose *a priori* credentials have been taken care of by the mathematicians. The theoretical scientist who simply 'applies' the available mathematics is usually concerned only with the next level down in our schema: with the subsequent derivations Σ of predictions (about the outcome of possible experiments), based on the scientific hypotheses and statements of whatever boundary and initial conditions will be controlled for in those experiments. It is then left to the experimentalists to carry out such experiments, and to gather and analyze the resulting data. Any eventual conflict between prediction and observation would be brought out at the lowest level in our schema, in the derivation Θ of absurdity (\perp). If the prediction concerned, say, features of the statistical distribution of measured values, then the data-processing involved in showing that the prediction is confuted might be quite complex; but Θ would still be a relatively low-level proof within computational mathematics. If, however, the prediction concerned some simple observable, such as 'It will land here', and the observation turned out to be 'It landed way over there', then Θ would consist of but a single step of inference to \perp .

Unlike the mathematician, who is concerned only with the subproof Π , and unlike the theoretical scientist, who is concerned only with the subproof Σ , and unlike the experimentalist, who is concerned only with the subproof Θ , the philosophical methodologist is concerned with the whole schema consisting of Π , Σ and Θ . A question that naturally arises is whether, whenever a *strictly classical* theorem φ

stands as the conclusion of some classical subproof Π , and as an assumption for some (possibly classical) subproof Σ :



on the way to a derivation of \perp via the proof Θ that draws on the outcomes of observations, *there might not be an alternative, fully constructive, deductive route to \perp from the same set of overall assumptions.*

The answer to this question is affirmative, and fully general. Any *reductio ad absurdum* that proceeds via a ‘strictly classical’ mathematical theorem can be effected with intuitionistic relevant logic. That is, the system *IR* will furnish a proof of \perp from the combined premises: mathematical axioms, scientific hypotheses, initial and boundary conditions, and observations/measurements. In such an *IR*-proof, *the passage via the strictly classical theorem φ will have been avoided altogether.* The classical theorem is never, strictly speaking, necessary for the precipitation of \perp ! Indeed, it is never, strictly speaking, necessary for the derivation of any decidable prediction. Since predictions are always of decidable matters of fact, we can summarize by saying that *strictly classical mathematical theorems are in principle otiose for the purposes of natural science.*

This is a startling result; but it is guaranteed by our constructively provable Metatheorem 1.

Intuitively, what happens is this. One takes the fully formalized proof

$$\Pi(\varphi)\Sigma(\text{Prediction})\Theta\perp$$

and one constructivizes it, using the constructive method furnished by the constructive proof of the Gödel–Gentzen–Glivenko theorem. Call the resulting proof Ξ . (Note that Ξ will not contain any subproof establishing φ as a theorem, since, *ex hypothesi*, φ is a strictly classical theorem.) Then one normalizes Ξ (a process which in general takes superexponential time, as a function of the length of Ξ); and finally one extracts the relevant kernel of Ξ (i.e., a proof in the system *IR*) by applying the linear-time transformation described in [Tennant, 1992] (for the propositional case) and [Tennant, 1994] (for the first-order case). What this

transformation does, essentially, is delete all applications of the Absurdity Rule (*Ex Falso Quodlibet*), and slightly massage applications of remaining rules (such as conditional proof, and proof by cases) so that they count as applications of those rules as stated for *IR*.

The constructivization and relevantization of *reductio* proofs described above is made possible by (i) the embeddability, via a suitable translation, of classical logic into intuitionistic logic, and (ii) the relevantizability of proofs with epistemic gain. The fully general form of (ii) is given by the following metatheorem.

METATHEOREM 2. If φ can be deduced from Δ in classical [resp., intuitionistic] logic, then either φ or \perp can be deduced from (some subset of) Δ in the classical [resp., intuitionistic] system *IR*.

It is an important feature of proofs in the systems *IR* and *CR* that they are in normal form (if natural deductions) or cut-free (if sequent proofs). Moreover, the natural deductions in either system may not contain applications of the absurdity rule; and the sequent proofs in either system may not contain any dilutions (or weakenings). The two systems otherwise involve only slight tweaking of the rules for the logical operators, so as to offset the deductive sacrifices that would otherwise have to be made upon eschewing the types of inferential moves just mentioned. (For a fuller treatment of both the natural deduction and sequent formulations of *IR* and *CR* see Tennant 1997, ch. 10.)

6 MEETING AN OBJECTION OF BURGESS

In [Burgess, 2005a], pp. 734–740, Burgess sets out a summary of the foregoing position, which he calls *perfectionism*, and enters an important (but rhetorical) objection. (The gist of his criticism is reprised, *con brio*, in [Burgess, 2005b].) The label ‘perfectionism’ is perfectly welcome, if it helps to identify the position in its essentials.

Burgess rightly points out that deductive progress in mathematics is cumulative, involving the interpolation of lemmas *en route* to mathematical theorems from one’s axioms. Typically we chain together proofs, choosing lemmas so as to reduce the deductive workload—massively. That way, theorems are put within reach that would otherwise be practically inaccessible, were we to limit ourselves to cut-free proofs not involving inferential passage to, and then from, our chosen lemmas. Those are the facts of mathematical life; that is how mathematicians actually proceed.

So, given that all the proofs in the relevant systems *IR* and *CR* are cut-free, it would not in general be practicable to insist that proofs of mathematical theorems should actually be carried out within *IR* or *CR*. Let us focus on classical mathematics and the system *CR*, for the present purpose of discussing Burgess’s objection to the reformist’s contention that *CR* is the correct (because suitably *relevant*) system of logic for classical mathematics. (Note that at this point the discussion focuses on mathematics; but we shall not lose sight of the broader theme

of scientific reasoning in general, and whether IR is adequate for it.)

The reformist (or ‘perfectionist’, in Burgess’s terminology) claims that there is no loss incurred by restricting oneself to CR —on the contrary, there is possible epistemic gain to be had. (See Metatheorem 2 above.) Burgess maintains that this claim is ‘unsurprisingly not uncontroversial’ (p. 739, fn. 6). His evaluation of the dialectical situation is as follows (pp. 739–740):

... in general, Cut can be eliminated only at the cost of making proofs infeasibly long.[fn] So even though *in principle* almost anything classically provable will be perfectionistically provable or else something even better will be, *in practice* the proofs mathematicians actually give not only *do* fail to adhere to the restriction of perfectionism, but *must* do so if they are to be kept to a humanly comprehensible length.

The perfectionist might then reply that in practice mathematicians could go on working as they do now, since Tennant’s work shows that in principle everything or almost everything they are doing could be justified from a perfectionist standpoint. The trouble with this response is that it relies on a theorem ... for which only a classically and not a perfectionistically acceptable “proof” has been given. How far can a logician who *professes* to hold that perfectionism is the correct criterion of valid argument, but who freely accepts and offers standard mathematical proofs, in particular for theorems about perfectionist logic itself, be regarded as *sincere* or *serious* in objecting to classical logic? This question will be left open for the reader to ponder

Now, all that is relevant to an assessment of the disagreement between Burgess and myself is the following set of high-level assertions:

1. The reformist (relevantist) R is claiming that *the correct logic* is some system \mathcal{R} (or set \mathcal{R} of proofs) properly contained in the system \mathcal{C} of the conservative (classicist) C .
2. R , the reformist, (informally) proves a metatheorem $\Phi(\mathcal{R})$, to the effect that the proofs in \mathcal{R} suffice in principle for all the mathematics that C wishes to prosecute. (This is Metatheorem 2 above: any proof Θ that the conservative C produces has an austere counterpart $\rho\Theta$ in the reformist’s system \mathcal{R} .) Let the most natural formalization, in \mathcal{R} , of R ’s informal proof of his metatheorem be called Π .
3. The reformist R agrees with the conservative C that many a result that C can prove in \mathcal{C} enjoys, in \mathcal{R} , only infeasibly long proofs.

Both Burgess and I agree on 1, 2 and 3. But we disagree on further reflections that Burgess enters on the heels of 2. In the passage quoted above, the crucial complaint that Burgess makes is as follows:

... $[\mathcal{R}]$ relies on a theorem ... for which only a classically and not a perfectionistically acceptable “proof” *has* been given. [My emphasis—NT]

In (2005b), Burgess expresses this objection in a slightly different fashion:

The rules of the cut-free system cannot be the whole story about what guides usage. For if they were, then the cut rule—used in practice throughout intuitionistic as well as classical mathematics—would not be accepted until the cut elimination theorem had been given a cut-free proof. But all the proofs of that theorem that have actually been given use the informal counterpart of the cut rule. To be sure, these proofs are constructive, and implicitly provide a recipe for converting a proof with cut into one without, which recipe could in principle be applied to the very proof of cut elimination itself. But the recipe results in a superexponential increase in the length of proofs, *and would give a cut-free proof of cut elimination having a number of steps probably greater than the number of elementary particles in the visible universe*. In practice no proof this long ever will be given by anyone. [My emphasis—NT]

The debate between the reformist R and the conservative C now takes more definite shape. Burgess’s critical view (on C ’s behalf) now consists of the claims 1, 2 and 3, but with the following rider to 2: **the austere formalization $\rho\Pi$ (in \mathcal{R}) of Π must be infeasibly long.**

This rider I deny, on behalf of the reformist R . Burgess offers no *proof* (by whatever the prevailing standards of rigorous but informal mathematical proof we are abiding by) for the claim that the specific proof in \mathcal{R} of the metatheorem $\Phi(\mathcal{R})$ must itself be infeasibly long. In direct rebuttal, I offer the conjecture that its formalization, as a natural deduction *in normal form*, within a system of iterated inductive definitions, would actually be eminently surveyable. The reformist R is perfectly entitled to use such a proof system; it has been offered as the natural formalization of a significant fragment of intuitionistic mathematics. One must not confuse (as Burgess appears to have done)

- (a) the known risk of exponential blow-up in length of object-system proofs upon normalization (or cut-elimination), the precise extent of which of course depends on the structural niceties of the particular abnormal proof that one is trying to normalize, with
- (b) the unsubstantiated conjecture that the particular proof Π , at the metalevel, of the metatheorem $\Phi(\mathcal{R})$, would itself, upon normalization, become unfeasibly long.

This unsubstantiated conjecture is, I believe, unsubstantiable—indeed, refutable. A refutation would consist in simply producing a normalized proof $\rho\Pi$ of $\Phi(\mathcal{R})$, in the form of a user-friendly computer-printout of a proof-term, or a token of the

natural deduction on a large enough webpage—up, down and across which the human verifier could scroll at leisure.

Metatheorem 2 follows easily from the fact that all proofs Σ of φ from Δ can *feasibly* be normalized, and then relevantized, so as to produce a proof $\rho\Sigma$, in the system \mathcal{R} , of either φ or \perp from (some subset of) Δ . I believe that this transformability result admits of feasible proof, in normal form, in some appropriate (relevantized) metasytem \mathcal{R}^* of iterated inductive definitions. In support of this contention I can only offer, at this stage, three admittedly inconclusive considerations.

First, *pace* Burgess, I do not see a plethora of cuts that need to be eliminated from within the usual inductive metaproofs of cut-elimination that are to be found in textbooks of proof-theory. They strike me as being already pretty close to normal form.

Secondly, such proof-theoretic intuitions as I have developed in frequent pursuit of fully formalized proofs *in normal form* of interesting mathematical theorems make me optimistic about the prospect of furnishing an \mathcal{R} -proof of the metatheorem.

Thirdly, there is the further consideration—not irrelevant or inconsequential in this setting—of the automation of proof-search. Fully automated proof-search proceeds in such a way that the output is always a proof in normal form. Computers could easily be recruited to the task of both producing and verifying a proof-term in normal form whose conclusion is $\Phi(\mathcal{R})$, should it turn out that the task were too much for the mental powers and digital dexterity of a human mathematician. In pursuit of this goal, one could legitimately resort to interactive theorem-proving, in order to increase one's chances of success. The proof arrived at would still have its (computationally verifiable) formal credentials unimpeached, despite the inputs from the human user that might have been practically necessary in order to enable its discovery.

Should \mathcal{C} (or Burgess himself) scoff at this foreshadowed prospect of intellectual prosthetics, let me offer a *tu quoque*. Consider points 1–3 above with the following interpretation:

- (i) \mathcal{C} contains just the informal proofs, in mathematical English, of the kind that one finds in mathematical textbooks and journals. These are proofs in which one resorts to abbreviatory devices such as 'w.l.o.g.', 'by a similar argument', 'the remaining cases are dealt with similarly', and helps oneself liberally to 'single steps' of logical argumentations that are massively compiled, when considered as transitions within a formal logical system.
- (ii) \mathcal{R} contains only fully formalized natural deductions in the system LK , say, of Gentzen.

It is remarkable what blow-up in the length of 'proof'-tokens is involved in the simple (and supposedly—for Burgess—unproblematic) transition from \mathcal{C} -proofs to \mathcal{R} -proofs, for various quite simple mathematical theorems. So if computer-unaided feasibility were his *crie du jour*, Burgess could well be hoist with his own

petard. For then the conservative C working ‘in’ the system \mathcal{C} of *fully formalized* proofs (involving, to be sure, cuts galore) might well be a figment of the *classical* methodologist’s imagination.

Whether one were to succeed by oneself, unaided by cybernetic deskmates, in writing down a normal-form proof of $\Phi(\mathcal{R})$, or needed to resort instead to automated proof-search and -verification in order to produce a token (soft or hard) of such a proof, the direct or indirect success involved would be an adequate dialectical counter to Burgess’s assertion that **the austere formalization $\rho\Pi$ (in \mathcal{R}) of Π must be infeasibly long.**

Suppose, then, that a feasible austere proof $\rho\Pi$ is available for the metatheorem $\Phi(\mathcal{R})$. What would this enable the reformist R to maintain, philosophically or methodologically? I believe it would put R in a very interesting position, which deserves to be expounded briefly.

First, R could maintain that the system \mathcal{R} really is the *correct* logic, on meaning-theoretic grounds, and on grounds of its (now feasibly!) proven adequacy for mathematics and science. Secondly, R could graciously concede point 3 above, and *draw his interlocutor’s attention to the striking fact* that—despite \mathcal{R} ’s really being the correct logic—honest and sincere deducers nevertheless resort to cut (i.e., to the interpolation of lemmas between their axioms and their theorems). R can now offer a *pragmatic explanation of this striking fact*. They reason using cut, according to R , because his very own metatheorem—feasibly proven!—shows that whatever they can prove using cut can be proved (austerely, and perhaps with epistemic gain) in \mathcal{R} , *even if only in principle*. If, in fact, it were to be infeasible, for particular \mathcal{C} -proofs, to render them as austere and possibly gainful \mathcal{R} -proofs, we now understand why reasoners resort to \mathcal{C} -proofs. For many a desired theorem, they *have* to, in order to get any semblance of the deductive job done. Moreover, since they are usually working within a system whose consistency is a well-tested article of faith, they know with that degree of certainty that any result proved by means of a \mathcal{C} -proof Θ will be true in all interpretations of the axioms used in Θ .

In an analogous way the pragmatist nominalist can explain why we incur commitment to numbers as theoretical objects. It is because their introduction, into our ontology, by the linguistic devices of numerical reference and quantification enables us to prove, very efficiently, empirical predictions from empirical premises whose number-eschewing proofs would be tortuously long. Undertaking these Platonic commitments enables the mathematizing reasoner to furnish shorter and often more elegant proofs of predictions from his physical theory. This is the central idea of Hartry Field’s (1980). It is worked out in considerable detail via conservative extension results. These aim to show that grafting mathematics onto a ‘synthetic’ theory enables one to prove only such synthetic claims from synthetic premises as are provable (albeit much more laboriously) *within* the synthetic theory *without* resorting to the deductive shortcuts afforded by the mathematics.

Similarly, I have argued, in [Tennant, 1996],⁵ that when R is the intuitionist

⁵Burgess appears, in his review (2005b) of *The Taming of The True*, to be unaware of this aspect of my overall view.

and C (as usual) is the classicist, R can give a pragmatic explanation of why C resorts to strictly classical inferential moves not countenanced within intuitionistic logic. Such moves are explained as expressive of high-level, *realist*, metaphysical commitments—commitments to the *determinacy of the world* in the contextually relevant regards. Undertaking these commitments enables the classical reasoner to furnish shorter and often more elegant proofs of results that the intuitionist would be able to prove only in a much more laborious (albeit more informative) way. Moreover, the intuitionist can prove, intuitionistically, an impressive array of results of the general form ‘If there is a C -proof of \perp from Δ , then there is an \mathcal{R} -proof (i.e., an intuitionistic proof) of \perp from Δ ’. These are known as *relative consistency theorems*, and the important point is that they are *intuitionistically provable*. They are the intuitionist’s way, from within the ‘limitations’ of his own vantage point, of explaining why no great epistemic disaster is going to befall the classical mathematician who uses the full system C of classical logic in order to reach results that have no constructively acceptable warrant for assertion.

We see, then, that the ‘austere eschewer’ R —whether he be a nominalist eschewing numbers, or an intuitionist eschewing classical moves of inference, or a relevantist (‘perfectionist’) eschewing cuts, *ex falso quodlibet* and kindred irrelevancies or potential sources of irrelevance—has a uniform way of explaining why it is that the conservative opposition incurs commitment to numbers, reasons classically about them, and uses cut when doing so. The ‘philosophically suspect’ maneuvers are all made in the name of greater efficiency and greater scope in our mathematical and scientific theorizing. The challenge is to find an austere vantage point from which one can explain, in austere terms, how this has come to pass; and from within the ‘limitations’ of which one can show that no great epistemic disaster will befall those who are tempted to go beyond the austere canons \mathcal{R} of correct reasoning, and to adopt the more expansive system C as a methodologically optimizing extension. This extension is motivated by a firm belief in the consistency of mathematics; a commitment to abstracta; and a realist conviction that the world is determinate in various regards. The challenge, to repeat, is to make austere sense of all this. And I do not believe that Burgess’s unsubstantiated conjecture about the feasibility of a particular metaproof is an insuperable obstacle to doing so.

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