*Brouwerian Intuitionism**

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1. *Précis*

The aims of this paper are twofold: firstly, to say something about that philosophy of mathematics known as 'Intuitionism' and, secondly, to fit these remarks into a more general message for the philosophy of mathematics as a whole. What we have to say on the first score can, without too much inaccuracy, be compressed into two theses, the first being that the intuitionistic critique of classical mathematics can be seen as based primarily on epistemological rather than on meaning-theoretic considerations, and the second being that the intuitionist's chief objection to the classical mathematician's use of logic does *not* centre on *the use of particular logical principles* (in particular, the law of excluded middle and its ilk), but rather on the *role* the classical mathematician assigns (or at least extends) generally (i.e. regardless of the *particular* principles used) to the use of logic in the production mathematical proofs. Thus, the intuitionist critique of logic that we shall be presenting is far more radical than that which has commonly been presented as the 'intuitionist critique'.

On the second, more general, theme, what we have to say is this: some restriction of the role of logical inference in mathematical proof such as that mentioned above is necessary if one is to account for the seeming difference in the epistemic conditions of provers whose reasoning is based on genuine insight into the subject-matter being investigated, and would-be provers whose reasoning is based not on such insight, but rather on principles of inference which hold of every subject-matter indifferently. Poincaré urged this point repeatedly, but, in the rapid development of logic in this century, it seems to have been forgotten. I think it deserves more attention than it has received and that, when properly taken into account, it provides an interesting 'new' ground for a mathematical epistemology sharing many of the features of Brouwerian intuitionism.

Poincaré's insight suggests an epistemology which operates according to a principle of epistemic conservation: there can be no increase in genuine

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knowledge of a specific mathematical subject without an underlying increase in subject-specific insight into (i.e. *intuitional* grasp of) that subject. Thus, the need for intuition cannot be avoided in mathematics even after it has supplied a set of axioms. Hence, purely logical inference cannot add to our genuinely mathematical knowledge, and thus cannot be given a very important role in proof.

The view of Brouwerian epistemology presented in this paper is sensitive to Poincaré's concern regarding the plausibility of a mathematical epistemology which allows increases in mathematical knowledge without a correspondingly increased insight into the particular mathematical subject involved. This new (i.e. non-classical) epistemology requires a new conception of inference, for in order for a truth to be proven it requires that it be 'experienced' in a certain way. And this new conception of inference severely restricts the role of logical inference in proof. By (classical) logical analysis or inference, one can extract all kinds of propositions from a given experienced proposition. But only some of these extracted propositions are themselves 'experienceable' in the appropriate way (just as, in the case of empirically perceived truths, only some of their logical consequences are themselves empirically perceivable). And none of them are experienced in the appropriate way solely by their being shown to be related to the premisses by logical means.

This, in brief, is the position to be developed in this paper. As mentioned, it seeks to present mathematical intuitionism as essentially an epistemological rather than a meaning-theoretic view. It also seeks to distance it from the solipsism commonly attributed to Brouwer, and to focus instead on Poincaré's concern over the place of purely logical inference in genuinely mathematical reasoning. The result, we hope, is both an interesting way of thinking about intuitionism, and a renewed appreciation of the importance of Poincaré's point for the philosophy of mathematics.

2. *Poincaré's Concern*

Poincaré presented his point in the form of an observation which he then put forth as a central 'datum' for the philosophy of mathematics. The substance of this observation is quite simple, and can be presented as the result of a thought-experiment to the following effect.

> Imagine two cognitive agents *M* and *L*. *M* has the kind of knowledge or understanding of a given mathematical subject *S* that we typically associate with the master mathematician. *L*, on the other hand, has the sort of epistemic mastery of *S* that is typical of one whose epistemic command of *S* consists in a knowledge of a set of axioms for *S* plus an ability (possibly superb) to manipulate or process those axioms according to acknowledged *logical* means. Query: Is there any significant

difference between the epistemic condition of M and the epistemic condition of L *vis-à-vis* their status as mathematical knowers?

In Poincaré's view, the answer is 'Yes'. Even perfect *logical* mastery of a body of axioms would not, in his view, represent genuine mathematical mastery of the mathematics thus axiomatized. Indeed, it would not in itself be indicative of any appreciable degree of mathematical knowledge at all: knowledge of a body of mathematical propositions, plus mastery over their logical manipulation, does not amount to mathematical knowledge either of those propositions or of the propositions logically derived from them.

On Poincaré's view, then, genuine mathematical reasoning does not proceed in 'logic-sized' steps, but rather in bigger steps—steps requiring genuine insight into the given mathematical subject being inferentially developed. This sets it at odds with logical reasoning which, *by its very topic-neutral character*, neither requires nor even admits use of such insight in the making of inferences. In thus foreswearing all appeal to information that derives from the particularities of the specific subject-matter under investigation, logical reasoning also foreswears the easy, loping stride of one familiar with the twists and turns of a given local terrain, and opts instead for the halting step of one who is blind to the special features of all localities, and who must therefore take only such steps as would be safe in *any*. In Poincaré's view, the security thereby attained cannot make up for the blindness which it reflects. Logical astuteness may keep one from falling into a pit, but having a cane with which to feel one's way is a poor substitute for being able to see.

It was from this general point of view that Poincaré framed his criticism of the 'logicians' (e.g. Couturat, Frege, Peano, and Russell); a criticism which occupied a place of fundamental importance in his overall philosophy of mathematics.

The logician cuts up, so to speak, each demonstration into a very great number of elementary operations; when we have examined these operations one after the other and ascertained that each is correct, are we to think we have grasped the real meaning of the demonstration? Shall we have understood it even when, by an effort of memory, we have become able to reproduce all these elementary operations in just the order in which the inventor had arranged them? Evidently not; we shall not yet possess the entire reality; that I know not what, which makes the unity of the demonstration, will completely elude us . . .

If you are present at a game of chess, it will not suffice, for the understanding of the game, to know the rules of moving the chess pieces. That will only enable you to recognize that each move has been made conformably to these rules, and this knowledge will truly have very little value. Yet this is what a reader of a book on mathematics would do if he were a logician only. To understand the game is wholly another matter; it is to know why the player moves this piece rather than that other which he could have moved without breaking the rules of the game. It is to perceive the inward reason which makes of this series of moves a sort of organized whole. This faculty is still more necessary for the player himself, that is, for the inventor.

(Poincaré (1905), pp. 217–18)

The very possibility of the science of mathematics seems an insoluble contradiction. If this science is deductive only in appearance, whence does it derive that perfect rigor no one dreams of doubting? If, on the contrary, all the propositions it enunciates can be deduced one from another by the rules of formal logic, why is not mathematics reduced to an immense tautology? The syllogism can teach us nothing essentially new, and, if everything is to spring from the principle of identity, everything should be capable of being reduced to it. Shall we then admit that the enunciations of all those theorems which fill so many volumes are nothing but devious ways of saying A is A?

Without doubt we can go back to the axioms, which are the source of all these reasonings. If we decide that these cannot be reduced to the principle of contradiction, if still less we see in them experimental facts which cannot partake of mathematical necessity, we have yet the resource of classing them among synthetic *a priori* judgements. This is not to solve the difficulty, but to baptize it; and even if the nature of synthetic judgements were for us no mystery, the contradiction would not have disappeared, it would only have moved back; syllogistic reasoning remains incapable of adding anything to the data given in it; these data reduce themselves to a few axioms, and we should find nothing else in the conclusions.

No theorem could be new if no new axioms intervened in its demonstration; reasoning could give us only the immediately evident verities borrowed from direct intuition; it would be only an intermediary parasite, and therefore should we not have good reason to ask whether the whole syllogistic apparatus did not serve to disguise our borrowing? . . .

If we refuse to admit these consequences, it must be conceded that mathematical reasoning has of itself a sort of creative virtue and consquently differs from the syllogism.

The difference must even be profound. We shall not, for example, find the key to the mystery in the frequent use of that rule according to which one and the same uniform operation applied to two equal numbers will give identical results.

All these modes of reasoning, whether or not they be reducible to the syllogism properly so called, retain the analytic character, and just because of that are powerless.

(Poincaré (1902), p. 31)

To bolster his general distinction between logical and mathematical reasoning, Poincaré offered an example, a case of reasoning which he took to be paradigmatic of genuinely mathematical resoning and which at the same time he believed to be non-logical (or, to use his term, 'non-analytical') in character; namely, mathematical induction.

After giving several illustrations of the importance of mathematical induction to mathematics, Poincaré turns to the issue of its character, arguing that it is synthetic, rather than analytic, since its conclusion 'goes beyond' its premisses rather than being a mere restatement of them 'in other words'. At the same time, however, he argues that it is entirely rigorous, and so, rightly classified as *mathematical* reasoning. It is precisely this combination of characteristics—syntheticity and rigour—that Poincaré takes to typify genuine mathematical reasoning. And it is the first of these—syntheticity—which he calls upon to distinguish mathematical from logical reasoning.[1]

This, in outline, is Poincaré's view of what are the epistemologically important differences between logical and mathematical reasoning. We do not, however, propose to discuss it in detail here. In particular, we intend no defence either of its understanding of the analytic/synthetic distinction, or of its contention that that distinction, thus understood, provides the correct means of explaining the more basic conviction that there is an important difference between the epistemic conditions of the purely logical reasoner and the mathematical reasoner *vis-à-vis* their mathematical knowledge. Rather, it is this more basic conviction which is of chief interest to us, since we take it to constitute the general problematic that is basic to Brouwer's intuitionism and, in particular, his critique of classical mathematics.

Taken seriously, this problematic promises to have some important effects on one's conception of mathematical knowledge. One such effect is that of implying what might be called a *modal*—as opposed to a *subjectival*—construal of mathematical knowledge. On the subjectival construal, the typology of knowledge follows a classification scheme which sorts knowledge according to the subject-matter of its content. Thus, in order for one's knowledge that p to count as κ-knowledge (i.e. knowledge of type κ), all that is required is that p be a truth *subject-matter* κ. On such a model, mathematical knowledge becomes simply knowledge of a *mathematical* truth (i.e. knowledge of a truth belonging to a mathematical subject-matter).

On what we are calling the *modal* conception, on the other hand, the

[1] Poincaré's repudiation of logical inference as belonging to genuine mathematical reasoning, of course, necessitates the development of a notion of rigour that is different from the usual logical one (according to which a proof is rigorous only if each of its steps of inference is purely logical). Poincaré himself did not say a great deal about this. Still, certain of his remarks suggest a radically new conception of rigour; one which sees rigour as consisting in the elimination of gaps in our *mathematical understanding* rather than logical gaps. On this model, an inference is rigorous only if we have a truly *mathematical* (hence, topic-specific) insight into why the premiss's being true insures the truth of the conclusion. The radicality of this suggestion can be seen from the fact that it not only allows non-logical inferences to be rigorous, but also implies that purely logical inferences (based on topic-neutral knowledge) are not rigorous!

It should also be noticed that Poincaré's point, though closely related to a famous point of Kant's, is none the less different. Kant maintained that some analytic inferences can be epistemically fruitful; if their conclusion is buried deeply enough in their premisses, then digging them out can bring new knowledge. Moreoever, he appears to have allowed that this hold for extensions of *mathematical* knowledge. Poincaré, on the other hand, though he may have allowed that analytic inference could sometimes produce new knowledge of some types, none the less explicitly denied that this is so for mathematical knowledge.

typology of knowledge does not follow a subject-matter classification of the propositions known. It marks as well certain differences in the particular cognitive attitude taken. Thus, mathematical knowledge becomes more than simply knowledge of a mathematical proposition, and is distinguished by a certain *mode* or *kind* of cognitive state as well.

It is, of course, a difficult question to say what exactly it is that is to distinguish that special mode of knowledge known as mathematical knowledge from other modes of knowledge. Indeed, there is room for dispute over this within the modalist camp. Poincaré, for example, believed that it had primarily to do with one's ability to see the role or position that a given proposition plays in the larger subject to which it belongs; so that one comes to know something mathematically by having a global vision of the place of that proposition within some larger epistemic enterprise. Brouwer, on the other hand, believed that to mathematically know a truth was to 'experience' it in a certain way. Others would say that the distinctive feature of mathematical knowing is its freedom from empirical considerations. Still others would insist that degree-of-certainty plays an important role. And so on.[2]

We shall make no attempt to decide between such rival modalist epistemologies here, since the implications of the modal conception of mathematical knowledge with which we are principally concerned are of a more general character than those pertaining to some particular articulation of it. Of particular importance to us in this connection are certain implications regarding how we are to conceive of the *growth* or *extension* of mathematical knowledge under a generally modalist mathematical epistemology. And, as we shall see shortly, the use of logical inference in the production of mathematical knowledge is only compatible with such weak modalist conceptions as take relatively large-scale, coarsely differentiating features (e.g. high degree of certainty or a priority) as the distinguishing features of mathematical knowledge.

As already noted, the key idea of the modalist conception is that to have mathematical knowledge of a given proposition p is to have a certain *kind* of knowledge that p. Thus, if a given kind of knowledge of p is to be extended to another proposition q by means of an inference from p to q, then that inference must preserve the special characteristics of knowledge of p that are responsible for its being of that kind. Therefore, if knowledge

[2] It may be helpful to say just a few words comparing the positions of Brouwer and Poincaré on this point. Both describe the special mode characterizing mathematical knowledge as knowledge by 'intuition'. However, they do not mean the same thing by that. For Poincaré, as was mentioned above, intuition is taken to be constituted by some sort of integrated knowledge—ultimately theoretical rather than practical in character—which enables the mathematical knower to see how a certain proof or theorem relates to other proofs and theorems, and how, in thus relating, it contributes towards the goals of some larger enquiry to which they all belong, and for the sake of which they are pursued. For Brouwer, on the other hand, the epistemically salient and distinguishing feature of intuition is that it is a type of knowledge borne of experience of an ultimately practical nature, and thus basically incapable of extension by logical inference. More on this later.

of a particular kind κ is to be extended by means of *logical* inference, then logical inference must preserve those features of a given piece of knowledge that make it κ-knowledge. To put it another way: if κ-knowledge of p is to be extended to κ-knowledge of q by means of a logical inference from p to q, then the distinguishing features of κ-knowledge must be included among those properties of beliefs that are preserved by logical inference.

This constraint is not trivial or powerless, since there clearly are types of knowledge that are distinguishable by features that are not preserved by logical inference. As a specific example of this, let us consider knowledge by direct sensory experience. I look at the grass outside my window and see that it is green. Turning my chair in the opposite direction, I view the carpeting in the hallway and see that it is grey. *Logically*, I can infer from the knowledge thus obtained that the grass outside my window is green and the carpeting in the hallway is grey. However, owing to the practical difficulties involved (e.g. my inability to direct my eyes in opposite directions at one and the same time, to see around or through corners, etc.), I cannot produce a direct sensory experience whose content is that the grass outside my window is green and the carpeting in the hallway is grey. Thus, logically extending the content of knowledge gained by direct sensory experience does not guarantee that the content thus extended will be accessible via *the same cognitive mode* (in this case, direct sensory experience).

A different, though equally mundane, kind of example can be found by considering such processes as ordinary counting. A ticket-taker at a basketball game knows, by having (partially) counted them as they entered the gate, that there are at least 25 people seated in his section. In order to determine how the people seated in his section are distributed over lower vs. upper arena seats, he decides to count those seated in upper arena seats. He counts zero people seated there. He thus knows by ordinary counting that there are at least 25 people seated in his section and that there is no one seated in an upper arena seat. From this it logically follows that there are at least 25 people seated in the lower arena seats. It is not, however, true that this is known by an ordinary (partial) counting of those occupying lower arena seats, since the ticket-taker arrived at his conclusion without having actually counted (in what we are calling the 'ordinary sense') the occupants of the lower arena seats.

What has just been said of knowledge by direct perception and knowledge by ordinary counting can also be said of other types of knowledge. Indeed, as we shall see later, it applies specifically to Brouwer's conception of mathematical knowledge, which he takes to be constituted by a kind of 'experience' (or intuition). In each case, the crucial issue is whether a given kind of cognitive mode (direct sensory perception, ordinary counting, Brouwerian mathematical intuition, etc.) can be manipulated or controlled in such a way as to be guaranteedly reproduced at all the propositions that are logically derivable from a proposition describing the content of a given such state. And in each case, the answer is 'no'. We do not have the practical capacity to manipulate the having of such kinds of mental states in the full range of ways that we can *logically* manipulate their *contents*. Therefore, logical inference does not preserve cognitive mode.

Logical manipulation of the content of a mental state is thus one thing, and *practical manipulation* of its cognitive mode another. Therefore, the assumption that mathematical knowledge is extendable by logical reasoning is not an innocent one. The only clear capacity of logical inference is that of an abstractive device; that is, a device for *separating off* the *content* of a given cognitive state from its *mode of occurrence*, and submitting that content to various sorts of analyses which issue in the production of new contents. As such, it is not automatically an extension or continuation of the cognitive state from which the content was separated, but rather a focused *reflection on* its content. Being thus focused on content rather than on cognitive mode, it may be expected to carry forward the content of a given piece of knowledge, but without any corresponding guarantee that the content thus forwarded occurs in the same cognitive mode as the original.

Such failure to extend the cognitive mode of a given piece of knowledge is, of course, no tragedy if occurrence in that mode is incidental, or at least inessential, to its overall epistemic character and/or value. And this may be the case in some of our examples (e.g. knowledge by ordinary counting). Brouwer and his 'pre-intuitionist'[3] predecessor Poincaré did not, however, believe that it is so in the case of mathematical knowledge generally, and their belief was rooted in that observation which we are referring to here as 'Poincaré's Concern': namely, that the epistemic condition of one who has gained a logical or axiomatic mastery over a given mathematical subject is inferior to that of one who has a genuine mathematical mastery of it.[4]

[3] 'Pre-intuitionist' was Brouwer's term for the philosophical views of the so-called 'French School', whose chief figures were Borel, Lebesgue, and Poincaré. The 'intuitionist' part draws its justification from the fact that, according to Brouwer, these thinkers regarded the objectivity and exactness of certain 'separable' parts of mathematics (namely, the theory of the natural numbers, including the principle of complete induction, together with whatever can be derived from it without the use of what Brouwer termed 'axioms of existence') as independent of language and logic. The 'pre-' part is intended to reflect the fact that they did not extend this attempt to find an extra-linguistic, extra-logical basis for knowledge of the continuum, a task which Brouwer took to be of critical importance. Cf. Brouwer (1951), pp. 2–3.

[4] Poincaré's Concern can also be seen as a problem concerning the compatibility of epistemic utility (i.e. the ability of proof to serve as a means of extending our knowledge) and what might be called the *logical conception of rigour*. According to this conception of rigour, concealment of assumptions in a proof is to be blocked by making every one of its steps or inferences so 'small' as to not require any insight into the subject of the proof in order to verify it. Only in this way can one be assured that the steps do not conceal material assumptions concerning the subject that will then go undetected.

Making the steps of a proof too small, however, may compromise its epistemic utility. The clearest

They had different ways of accounting for this difference, but both clung to it as a basic fact of mathematical epistemology. In this paper, we shall consider only Brouwer's epistemology, leaving Poincaré's for another occasion. However, as a preparation for presenting Brouwer's ideas, it will prove useful to at least lay out the rudiments of its chief antithesis; namely, the so-called 'classical epistemology'.

3. *Classical epistemology*

The epistemology underlying classical mathematics (which, for brevity's sake, we shall refer to as *classical epistemology*) emphasizes the *contentual* ingredient of knowledge, and de-emphasizes the matter of its *cognitive mode*. According to it, the mathematical knower may rely on introspective experience (or intuition) of some sort to arrive at the initial propositions of his epistemic edifice, but from that point on he is free to abstract away from or ignore the *non-contentual* aspects of that experience, and concentrate instead on its *contentual* component. What, on this account, is of primary epistemic importance concerning the cognitive mode of a given epistemic event is what might be called its *credential effect*; that is, the degree of certitude it confers upon the proposition expressing its content. But since widely different cognitive modes are capable of having the same credential effect, identifying the epistemic significance of a cognitive mode with its credential effect produces an epistemology which tends to reduce the number of epistemically significant differences between cognitive modes. This, in turn, leads to a view of inference which sees it as having relatively little obligation to preserve the features of the cognitive mode of the premisses (since so few of them are of any epistemic significance). Beyond credential effect, classical mathematical epistemology, at least in some of its variants, may make room for such large-scale characteristics of cognitive mode as its aprioricity/aposterioricity. However, sensitivity to such large-scale features will surely not provide a grid fine enough to make the sorts of small-scale demarcations—in particular, the sort of demarcation between logical and mathematical reasoning described in the preced-

case, after all, of a fully 'gapless' or rigorous proof is that of a circular argument, where the conclusion simply *is* one of the premisses. Such proofs, however, are equally clear cases of epistemically useless reasoning. Thus, a minimal constraint on the epistemic utility of a proof is that it not be circular.

But though non-circularity is a necessary condition for epistemic utility, it seems not to be sufficient. It is doubtful, for instance, that one can turn a circular argument with a small number of simple premisses into an epistemically useful one just by conjoining the premisses and replacing them by the resulting conjunction. The argument obtained by such a procedure is not circular in the strict sense, since its conclusion is not literally the same proposition as any of its premisses. But its epistemic utility is still doubtful, since one can clearly see that affirmation of the conclusion is part of what is needed for affirmation of the premiss. Indeed, were one to 'sharpen' the argument (i.e. to eliminate from the premises what is clearly unnecessary to its validity), one would end up with an argument whose only premiss is the conclusion itself. As a general rule, it seems that any argument possessing recognized sharpenings that are circular is essentially lacking in epistemic utility.

ing section—that Brouwer and Poincaré regarded as being of prime importance to a well-developed mathematical epistemology.

On the classical view, then, proof or inference is a procedure of the following sort: the mathematical knower begins with knowledge occurring in a certain cognitive mode; he then abstracts away from all characteristics of that cognitive mode that he regards as epistemically irrelevant, leaving him with only such of its features as credential effect and, say, aprioricity/-aposterioricity to attend to; having thus narrowly restricted the focus of his epistemic concern, he has correspondingly widened the horizons of inference by making it possible to extend his knowledge to any *new* proposition which can be obtained from the proposition expressing the content of his old knowledge by means capable of preserving (sufficiently much of) his narrowly restricted focal epistemic desiderata (namely its credential effect and aprioricity/aposterioricity).

This relationship between the narrowing of the range of the epistemically significant features of cognitive mode and the corresponding broadening of the inferential horizon deserves a further word of elaboration. For it is really not the narrowing of the range of epistemically significant features of cognitive mode *per se*, but rather the *particular* narrowing to the likes of credential effect and aprioricity that produces the corresponding widening of the range of possible inferences: such epistemic attributes as credential effect and aprioricity are preserved by a wider range of inferential transformations than are such more fine-grained attributes as direct sensory perceived-ness or Brouwerian mathematical intuited-ness. In identifying the crucial feature(s) of warrantedness with properties that are so little dependent on the more fine-grained characteristics of cognitive mode, it increases its 'liquidity' or transferability by decreasing the extent to which the cognitive mode of a properly inferred conclusion must resemble that of the premisse(s) from which it is inferred.

Thus it is that the less (more) stringent the demands on preservation of the features of the cognitive mode of a premiss are, the less (more) restricted are the opportunities for inference. In placing relatively weak demands on the preservation of cognitive mode, classical epistemology thus leaves a correspondingly greater role for inferential justification. This is perhaps its most significant point of contrast with Brouwerian epistemology. As we shall see in the next section, the demands on preservation of cognitive mode coming from Brouwerian epistemology are so strong as to leave very little opportunity for turning a justification for one proposition into a justification for another. And since the ability to use a justification for one proposition to produce a justification for another seems to be the essence of inferential justification, the result is that Brouwerian epistemology leaves comparatively little scope for inferential justification. Thus the comparatively greater need for what might be called 'intuition'.

On the above analysis, then, logical inference (by which mathematical knowledge is to be extended) is essentially a comparative reflection on *contents*, where these contents are taken to be relatively independent of the epistemic processes or activities to which they are attached. It does not *reflect* or *express* the characteristics of the epistemic activities underlying those contents in such a way as to force logical relations to imply practical relations between them. The basic idea of classical epistemology is thus that the epistemically relevant characteristics of a given experience or piece of intellectual activity are separable or detachable from it. Memory, or some like capacity, is called upon to sustain the epistemic effects and potency of a given piece of mental activity long after the activity itself has ceased to exist in experience. That memory-like capacity functions to 'retain' the content and warrant of an experience (or other warranting activity) so that it can be passed on to propositional contents not occurring in that cognitive mode. *Logical* analysis then 'detaches' the contentual results of epistemic processes from those processes themselves, and treats them as independent entities; the result being that logical inference or knowledge is taken to consist in a manipulation of *warranted contents* rather than of *warranting processes*.

In classical epistemology, then, the epistemic effect of a warrant is quite stable—being preserved under transformations that allow the characteristics of the particular process(es) that originally produced the warrant to be greatly altered.

Motivating this classical epistemology of inference is a certain conception of language and of the epistemological enterprise generally that we shall call the *logic-intensive* or *representation-intensive* view. The basic idea behind this view is that though knowledge may perhaps begin with 'intuition' or experience of some kind, it none the less can, and in many cases must and should, be extended without a corresponding extension of that intuition or experience. Thus, though experience may be necessary in order for knowledge to *begin*, it has strictly limited value as a means of *extending* knowledge.[5]

[5] Brouwer himself seems to have held a view something like this for the relationship of mathematics to natural science.

> The significance of mathematics with regard to scientific thinking mainly consists in this that a group of observed causal sequences can often be manipulated more easily by extending its of-quality-divested mathematical substratum to a *hypothesis*, i.e. a more comprehensive and more surveyable mathematical system. Causal sequences represented in abstraction in the hypothesis, but so far neither observed nor found observable, often find their realizations later on.
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> ((1948), p. 482)

Of currently greater significance to use at the present, however, is his idea that the logically driven classical conception of mathematics is related to genuine mathematics *in the same way* that, in the passage just quoted, mathematics is said to be related (or at any rate relatable) to natural science. Thus, logic-intensive classical reasoning is to genuine mathematical reasoning as mathematical of-quality-divested representational manipulation is to empirical investigation. They may be more or less accurate devices for predicting which causal sequences of intuitions will arise, but even when fully accurate they are not to be confused with the actual or potential intuitional verification of such sequences. More on this later.

It is this belief in the limited epistemic exploitability of experience that forms the basis of the logic-intensive or representation-intensive view. It takes experience itself to be a relatively unextendable commodity, either because of practical difficulties or because of the costs associated with doing so. It thus sees our epistemic condition as one in which we are allowed a relatively modest budget of experience or intuition to set the epistemic enterprise in motion and in which there is relatively little opportunity for causally prodding or massaging that modest initial budget of experience into a larger fund capable of meeting our epistemic needs. Therefore, we resort instead to *inference*, which, the classical view holds, offers us the epistemic benefits of extension of experience without the attendant costs and difficulties pertaining thereto.

Sometimes these 'costs and difficulties' may take the form of sheer danger. While considering whether to dry my hair with my acetylene torch, it occurs to me that it may do to my head something like what it did to the pipe I cut with it last night. How do I decide what to do? I'm pretty sure that I don't want it to do to my head what it did to the pipe, but how do I find out if it will? I need to extend my knowledge in such a way as to decide whether the torch will cut my head like it cut the pipe. But I clearly do not want to do so by actually extending my experience in the appropriate way; that is, by actually trying out the torch on my head and observing what happens. The most elementary considerations of utility counsel against this; the disutility connected with failure being too high when compared to the utility connected with success to make such trial-and-error experimentation rational. But how, then, do I extend my knowledge in the desired way *without* extending my experience?

The answer, roughly, is that I substitute a logically manipulated system of hypotheses for the physical act of applying the torch to my head. That is, I revert to a scheme of *representation* wherein the various states of my head and the torch are represented by propositions expressing those states, and the consequences of these states are then retrieved by retrieving the logical consequences of their representing propositions. For the *act* or *experience* of actually placing my head in the torch's flame, I thus substitute the *proposition* whose content is that I do so. And in the place of an experientially determined set of consequences of that act (i.e. the resulting burning sensation, the smell of burning hair and flesh, etc.), I substitute a set of propositions (representing those consequences) obtained by logical derivation from the proposition expressing the content of that act (together, typically, with certain auxiliary hypotheses representing the circumstances in which the act takes place and whatever natural laws may pertain thereto.) I thus rely on a relatively painless *logical* manipulation of *representations* (propositions) rather than a potentially painful *experiential* manipulation of the corresponding *physical states* in order to determine what the consequences of drying my hair with the blowtorch would be.

The happy outcome, as Popper once put it, is that I '... permit my hypotheses to die in my stead'.

An equally important, if less dramatic, illustration of the supposed need for the representational point of view is suggested by an empirico-constructional conception of geometrical thought like that found in Lorenzen (1984, 1985). On this conception, geometrical knowledge has its origins in a body of experiential constructional activities. For a variety of practical reasons, we are called upon to engage in such activities as the grinding of surfaces against one another to render them co-planar, the construction of planar figures using pencil, compass, and straightedge, the folding of planar objects to obtain other planar (or solid) objects, the rotation of these objects in various ways about the axes determined by such foldings, the construction of composite objects having a particular planar or solid character (e.g. that of being square) from component objects having another (e.g. that of being triangular), and so on.

The range and variation of such practical constructional activity is, however, strictly limited. Compass and straightedge can be practically managed only for planar objects of relatively small size; foldings and various other 'reflection' operations must cope with such things as the tensile strength of the materials involved, the strength and accuracy of the folder, the length of her appendages, and so on. As a consequence of these limitations, we are not well-situated to experientially determine what the result of folding, say, a one-block-on-a-side square of paper, or a one-inch-on-a-side square of titanium will be. In short, our ability to actually extend our constructional activity to a wide variety of sizes and material-types of objects is strictly limited. To put it still another way, extension of knowledge over the full range of situations with respect to which we might desire such extension is simply too difficult to manage if we insist that it involve an extension of our actual constructional activity. Consequently, we seek a means of epistemically *projecting* our experience without actually *extending* it, so that our geometrical knowledge need not be bounded by the limitations of size, time, strength, etc. which limit our activities as actual line-drawers, paper-folders, planar-object-rotaters, etc.[6]

Extending this view beyond geometry to mathematics generally, we

[6] Talk of 'projection', of course, raises immediate questions concerning what it is that is the point or goal of the sorts of elementary constructional activities mentioned above. When engaged in those activities are we experimenting with the actual medium-sized physical objects of everyday experience in order to get a clearer idea of the range of spatio-temporal manipulations through which they can be put? Or are we really attempting to effect, mentally, idealized operations (e.g. true reflections, true circumscriptions, true rotations, etc.) on ideal objects such as true planes, true circles, etc., and merely so designed that we are assisted in these tasks by our actual spatio-temporal fumblings with the geometrically imperfect physical objects of everyday life? Serious and interesting as these questions doubtlessly are, they are none the less not our concern here. For regardless of the true nature and subject of the constructional activity of elementary geometry, the difficulties involved in trying to extend it motivate one to develop a means of projecting it without extending it. It is this process of projection, and its possible relationship to the associated notion of extension, that are of primary interest to us here.

arrive at the classical viewpoint, which may be summarized as follows: mathematical knowledge may begin with a type of intuition or practice, but for a variety of reasons (having to do with the practical limitations concerning such things as our susceptibility to pain and the restrictedness of the time, effort, strength, material resources, etc. that we have to invest in such enterprises as the basic constructional activities of mathematics), this experience is insufficiently 'plastic' to be practically extendable to the full variety of propositions over which we should like our knowledge to range; therefore, in place of the relatively impliant *practical* or *causal massaging* of mathematical *intuition*, we substitute a more pliant scheme of *logical* manipulations of its *contents*.

Thus it is that an experience comes to be represented by a proposition expressing its content. And, as with any good scheme of representation, one then uses more practicable (i.e. less dangerous, costly, etc.) operations on the *representens* to bring about the same basic epistemic effect as the less practicable operations on the *representanda*. This then is the general logic-intensive or representation-intensive conception of epistemic extension that we believe to have been the chief target of Brouwer's attack on classical mathematics.

4. *Brouwerian epistemology*

Brouwer offers the following hypothesis regarding the origins of the classical viewpoint:

> ... some very familiar regularities of outer or inner experience of time and space were postulated to be invariable, either exactly, or at any rate with any attainable degree of approximation. They were called axioms and put into language. Thereupon systems of more complicated properties were developed from the linguistic substratum of the axioms by means of reasoning guided by experience, but linguistically following and using the principles of classical logic ... [This viewpoint] considered logic as autonomous, and mathematics as (if not existentially, yet functionally) dependent on logic.
>
> (Brouwer (1951), p. 1 (brackets mine))

He then goes on to identify as the (or at least a) fundamental mistake of this viewpoint the belief

> ... in the possibility of extending one's knowledge of truth by the mental process of thinking, in particular thinking accompanied by linguistic operations independent of experience called 'logical reasoning', which to a limited stock of 'evidently' true assertions mainly founded on experience and sometimes called axioms, contrives to add an abundance of further truths
>
> (Brouwer (1955), p. 113)

As an antidote to this basic miscalculation of the rôle of logical reasoning in the production of mathematical knowledge, Brouwer offered his so-called First Act of Intuitionism,

... completely separating mathematics from mathematical language and hence from the phenomena of language described by theoretical logic, recognizing that intuitionist mathematics is an essentially languageless activity of the mind having its origin in the perception of a move of time.

(Brouwer (1951), p. 4)

According to Brouwer, mathematics is essentially a form of introspective constructional *activity* or *experience* whose growth or development thus cannot proceed via the logical extrapolation of its content (as classical epistemology maintains), but rather only by its *phenomenological* or *experiential* development—that is to say, its extension into *further experience* of the same epistemic kind. The logical extrapolation of content—that is, logical inference—can never, Brouwer says, 'deduce a *mathematical* state of things' (cf. (1954), p. 524, emphasis mine). In genuine mathematics, theorems are proved 'exclusively by means of introspective construction' (cf. (1948), p. 488). Logical laws are not 'directives for acts of mathematical construction' (cf. (1907), p. 79), but rather derive from regularities in the language (possibly mental) used to express or represent such constructions. And while the regularities of a given such scheme of representation might prove useful in our attempts to remember genuine mathematical experiences, and to communicate them to others, they must not be confused with or equated to means of actually extending that experience (cf. (1907), p. 79; (1908), p. 108; (1955), pp. 551–2). Indeed, we must bear in mind the fact that, even judged solely as instruments for memorization and communication, such schemes for representing experience are subject to limitations of exactitude and correctness (cf. (1951), p. 5).

Mathematical knowledge is thus essentially a form of constructional activity, with the consequence that extension of that knowledge must take the form of extension of that activity, rather than a mere, actionally disembodied, logical extrapolation of its contents. This, at any rate, is the anti-classical kernel of Brouwerian epistemology which is of principal concern to us in this essay.

Brouwer's central thesis, then, is the general and sweeping one asserting the *experience-intensive*—and denying the *logic-intensive*—character of mathematical knowledge and its growth: mathematical knowledge is a form of experience or activity, and growth of mathematical knowledge therefore requires growth of that activity. Thus, if mathematical knowledge of a proposition p is to be extended to mathematical knowledge of a proposition q, the experience or activity whose content is p must be *transformed into* an experience or activity whose content is q. In contradistinction to the classical model of epistemic growth, then, Brouwerian epistemology does not present the prover as reflecting on contents, generating new from old by this logical reflection, and thence transferring the warrant for the old to the new (by appeal to the warrant-preservingness of the modes of contentual analysis employed). Rather—and it is hard to overemphasize the importance of this difference to the present discussion—the mathematician transforms old proof-experiences or proof-activities into new ones and thus witnesses the extension of her knowledge to new propositions when such a proposition emerges as the content of the newly created proof-experience. What is thus crucial and basic is the creation of a new proof-experience. For once such experience exists, knowledge may be extended to whatever its content is. What logical relation the content of this newly created experience might bear to that of the old is a matter of secondary concern. For knowledge-extension proceeds not by the logical extraction of new propositions from ones already known, but rather by the phenomenological transformation of one proof-experience into another—the new content emerging as *the content of* the new experience produced by this transformation. Mathematical inference or proof thus follows the path of the possibilities relating mathematical activities, rather than the chain of connections determined by some logico-linguistic analysis of the (propositional) contents of such activities, as classical epistemology maintains.

This, then, is the substance of Brouwer's *first*—in order of basicness and importance, if not of recognition—critique of classical mathematics. It faults classical epistemology not for its particular choice of logical principles to serve as means of extending mathematical knowledge, but rather for the fact that it accords such epistemological power to *any* set of purely logical principles. For the Brouwerian, a proof is more (and also less) than just a series of epistemic attitude-takings whose contents are logically related. It follows instead an ordering of activities where what might be called the 'actional accessibility' of one constructional activity from another is more important than the logical accessibility of the content of the one from the content of the other.[7]

[7] Heyting characterized the difference between his logic and classical logic as that separating a 'logic of knowledge' from a 'logic of existence'. In a logic of knowledge, he went on to say, 'a logical theorem expresses the fact that, if we know a proof for certain theorems, then we also know a proof for another theorem' (cf. Heyting (1958), p. 107). However, it is not clear that this way of thinking of epistemic incipience is at all close to the way in which Brouwerian epistemology conceives of it. For the Brouwerian, a given proof π' can only be said to be incipient in another proof π when the constructional activity or experience that *is* π is transformable into the constructional activity that would be π'. The activity that is π and the activity that would be π' are, however, different activities; and it would therefore not be correct to say that in doing the activity that is π one also *does* the activity that would be π'. From this and the fact that to know a proof for a theorem is to live or perform the activity that *is* that proof, it would seem to follow that in knowing the proof that is π one does not automatically know the proof that is π'. It thus seems that an intuitionist logic is not, as Heyting proposed, so much a logic of knowledge as a logic of knowledge-by-actually-doing.

It may also be that Brouwer regarded proof-activities as more robustly autonomous than did Heyting. Heyting saw proof-experiences as deomposable along contentual lines; that is, he held the view that if a proof-experience π had a compound proposition p as its content, then for each propositional component $c(p)$ of p, there would be an isolable sub-activity $c(\pi)$ of π such that $c(\pi)$ is a proof-activity whose content is $c(p)$. This, of course, suggests that proofs are logically deformable, and is not clear that the kinds of proof-tranformations that Brouwer had in mind (i.e. proof-

This general insistence on the part of Brouwer to distinguish between the logical extrapolation of mathematical knowledge, on the one hand, and the genuine extension of mathematical knowledge on the other, should not, however, be taken to imply that he denied any and all epistemic significance to logical inference. For he seems to have granted to logical extrapolation a certain limited role as an *instrumental device* (founded on the manipulation of a scheme of representation for proof-experiences which represents them by means of their propositional contents) for identifying, remembering, and communicating propositions for which an intuitionistic proof-experience might be found.[8]

Determining by means of a logical 'calculation' that a given proposition p can be given an intuitionistic proof is not, of course, epistemically equivalent to either giving or being in a position to give an intuitionistic proof for p. Nor is a logical derivation which determines that an intuitionistic proof of q can be obtained from an intuitionistic proof of p the same as either transforming or being in a position to transform a proof-activity for p into a proof-activity for q. Yet despite the fact that a logical 'calculation' that p is provable is epistemically inferior to either having or having the practical ability to produce a proof for p, it does not follow that it is of no epistemic value whatever. It can have value—as a device for determining where to invest one's proof-seeking efforts. Can, that is, to the extent that it is accurate.[9]

transformations that constitute the optimal development—the free unfolding of—our mathematical knowledge) would have followed the lines of such deformation. He did admit that there were intuitionistic proof-activities corresponding to certain of the proofs constructed in an axiomatic system, but this may only have meant that they agreed in content and not in the compositional arrangement of sub-proofs. Indeed, though proof-activities for the Brouwerian can be structured, it is not likewise clear that the elements of that structure correspond to the sub-proof structure of an axiomatic proof, since there is no apparent reason why what structures some complex activity as an activity need follow the lines induced by contentual deformation. We shall return to these matters in the concluding section of the paper.

[8] Both here and in the discussion to follow, we do not necessarily use the term 'calculation' to signify the usual sort of effective, syntactical manipulation of symbols. Rather, we intend only the broader idea of a procedure that is something other than the literal thought or reasoning whose progress it (the so-called calculation) is supposed to chart. Thus, in calling logical reasoning 'calculation', we are not intending to suggest that it is symbol-manipulation rather than contentual thought, but only that *as contentual thought* it is different from the contentual thought (namely, genuine mathematical thought) of which it seeks to construct a 'map'.

[9] We are not, therefore, denying that there is such a thing as 'intuitionist logic', in the sense of a general theory of potential intuitionistic assertability. Rather, what we are denying is that the connections between propositions disclosed by such a theory are to be treated as constituting *rules of proof*; that is, rules that may actually be *used* in the construction of mathematical proofs. A connection between p and q established by intuitionist logic only tells us that from an experience of p we may expect to obtain an experience of q if we proceed in an appropriate way. But as Brouwer remarked (cf. (1948), p. 488), '... expected experiences, and experiences attributed to others are true only as anticipations and hypotheses; in their contents there is no truth', and propositions (i.e. the linguistic representation of possible contents of experience upon which the rules of logic operate) do not 'convey truths *before* these truths have been experienced'. What we take to be Brouwer's view of intuitionist logic is thus very different from that which is common today and which seems to have originated with Heyting.

It is this matter of accuracy that stands behind Brouwer's *second* (though better-known) critique of classical mathematics; namely, the critique of the law of excluded middle and allied principles of classical logic.

> ... the function of the logical principles is not to guide arguments concerning experiences subtended by mathematical systems, but to describe regularities which are subsequently observed in the language of the system ...
> Thus there remains only the more special question: 'Is it allowed, in purely mathematical constructions and transformations, to neglect for some time the idea of the mathematical system under construction and to operate in the corresponding linguistic structure, following the principles of *syllogism*, of *contradiction* and of *tertium exclusum*, and can we then have confidence that each part of the argument can be justified by recalling to the mind the corresponding mathematical construction?
> Here it will be shown that this confidence is well-founded for the first two principles, but not for the third.
>
> (Brouwer (1908), pp. 108–9)[10]

It will probably be objected that the above fails to do justice to the fact that Brouwer often said such things as that having an algorithm for producing an equation is epistemically equivalent to actually producing it, and that I have therefore failed to realize that having an intuitionistic procedure for producing an experience and actually having that experience are to be treated as epistemologically equivalent. I plead innocent to this charge and have two replies to make in my defence. The first concerns correctly understanding what might be involved in equating the actual derivation of an equation with having an algorithm for deriving it. It is not clear to me that this is to be read as suggesting the equivalence of actually having an experience and being in a position practically to affect that experience. It might rather be read as saying that the experiencing of a computed equation simply *is* or *consists in* the possession of an algorithm for producing it. Taken in this way, the supposed equivalence of having an alogorithm for producing and actually producing a result does not suggest any equivalence between actually having an experience and being in a position practically to effect that experience. It rather informs us of what sorts of things experiences of numerical results *are*.

The second point, which I shall only allude to here, is related to this matter of possessing algorithms. Let us suppose that having algorithms generally (and not just algorithms for producing numerical results) should be counted as having an intuitionistic experience (or something epistemically equivalent to it). Would it follow that the rules of intuitionist logic should be taken as devices for the construction of actual proofs? The answer, I think, is 'no', for reasons that shall become clear in the concluding section of the paper.

[10] This point was made repeatedly in Brouwer's writings.

> Will hypothetical human beings with an unlimited memory, who use words only as invariant signs for definite elements and for definite relations between elements of pure mathematical systems which they have constructed, have room in their verbal reasonings for the logical principles for tacking together mathematical affirmations? Or what comes to the same: Will human beings with an unlimited memory, while surveying the strings of their affirmations in a language which they use for an abbreviated registration of their constructions, come across the linguistic images of the logical principles in all their mathematical transformations. A conscientious rational reflection leads to the result that this may be expected for the principles of identity, of contradiction and of syllogism, but for the principium tertii exclusi only in so far as it is restricted to affirmations about part of a definite, *finite* mathematical system, given once and for all whilst a more extensive use of the principle would not occur, because in general its application to purely mathematical affirmations would produce word complexes devoid of mathematical sense .... It follows that the language of daily intercourse between people with a limited memory, being necessarily imperfect, limited and of insecure effect, even if it is organized with the utmost practically attainable refinement and precision, will only be suitable for its task of mnemotechnic, economy of thought and understanding

Brouwer's critique of the law of excluded middle therefore had the status not of an argument designed to show that though other logical principles might play a significant role in the construction of a proof it (i.e. excluded middle) cannot. Rather, it had the status of a critique of a 'calculating' device; a device which, even if perfect, could play no serious role in the giving of genuine proofs, but rather could serve only as a means of identifying those propositions that might be given a proof. Brouwer's criticism is that, used as (part of) a device for locating those propositions capable of being the contents of an intuitionistic proof-experience, excluded middle would lead to the identification of certain propositions as having this trait when in fact they do not. Therefore, it is unsatisfactory as (part of) an instrumental device for 'calculating' which propositions have the potential to become contents of intuitionistic proof-experiences.

In addition to the inaccuracy borne of this *unsoundness*, there is another respect in which classical logic is inaccurate. This inaccuracy stems from its *incompleteness* as a device for locating those propositions that are capable of receiving a proof. Brouwer argued this point vigorously, and developed a battery of results from analysis which he took as illustrating it (cf. Brouwer (1923), 1949a, 1949b). Among these, perhaps the most famous is his proof of the Continuity Principle; that is, the theorem stating that every total real-valued function on the closed unit interval is uniformly continuous (cf. Brouwer (1923), p. 248).

Brouwer could thus sum up his criticism of classical logic as an instrument for determining which propositions are capable of being the contents of an intuitionistic proof-experience by saying that 'there are intuitionist structures which cannot be fitted into any classical logical

in mathematical research and mathematical intercommunication, if any application of the principium tertii exclusi which is not restricted to a well defined system is avoided.

(Brouwer (1933), p. 443)

. . . on account of the highly logical character of usual mathematical language the following question naturally represents itself: Suppose that an intuitionist mathematical construction has been carefully described by means of words, and then, the introspective character of the mathematical construction being ignored for a moment, its linguistic description is considered by itself and submitted to a linguistic application of a principle of classical logic. Is it then always possible to perform a languageless mathematical construction finding its expression in the logico liguistic figure in question? After careful examination one answers this question in the affirmative (if one allows for the inevitable inadequacy of language as a mode of description) as far as the principles of contradiction and syllogism are concerned; but in the negative (except in special cases) with regard to the principle of the excluded third . . . .

(Brouwer (1952), p. 14)

In the edifice of mathematical thought . . . language plays no other part than that of an efficient, but never infallible or exact, technique for memorizing mathematical constructions, and for suggesting them to others; so that the wording of a mathematical theorem has no sense unless it indicates the construction either of an actual mathematical entity or of an incompatibility (e.g., the identity of the empty two-ity with an empty unity) out of some constructional condition imposed on a hypothetical mathematical system. So that mathematical language, in particular logic, can never by itself create new mathematical entities, nor deduce a mathematical state of things.

(Brouwer (1954), pp. 523–4)

frame, and there are classical arguments not applying to any introspective image' (cf. (1948), p. 489). The first part of this claim emphasizes the inaccuracy borne of the *incompleteness* of the classical instrument, while the second emphasizes that which results from its *unsoundness*. If the principles of classical logic were to be amended in such a way as to eliminate these deficiencies of incompleteness and unsoundness, then one would have an apt logical instrument; that is, an accurate device for determining which propositions are potential contents for intuitionistic proof-experiences. However, such a device could still serve only to *identify* those propositions that are capable of intuitionistic justification—which is a very different thing from (and epistemically inferior to) actually supplying such justification.

Such, at any rate, is our understanding of the Brouwerian standpoint, which is strikingly at odds with the usual version of intuitionism presented in the literature. On the usual version, the critique of excluded middle is presented as the centrepiece of the intuitionist's concerns and the crux of his criticism of classical mathematics. Our view differs from this in two ways. First it suggests that the question 'Which logic is the logic of mathematics?' (and particularly the sub-question 'Does the law of excluded middle belong to the logic of mathematics?') is of secondary importance. The more fundamental question is 'What role does *any* logic (including the "right" one) have to play in the construction of intuitionistic proofs?' Judged from this vantage, the question 'Which logic is the logic of mathematics' can only be regarded as misleading.

The second respect in which our view differs from the usual one is in its deflation of the significance of the critique of excluded middle—even with respect to the role that it plays in the criticism of classical logic as a locative/mnemonic device. On the view presented here, that critique is to be seen as but one part of a larger two-part critique that is concerned not only with the soundness of the classical/mnemonic device, but also with its completeness. Basically, the critique of excluded middle is a critique of soundness and makes little if any contribution to the assessment of the completeness question, despite the fact that this latter question is just as important to the accuracy of a locative/mnemonic device as the soundness question.

It may even be that the importance of the critique of excluded middle should be deflated still further. For, of the two parts of the accuracy question, the part to which it contributes (namely the soundness question) may be of less overall significance to Brouwerian epistemology. To understand why this is so, we must hearken back to what we identified in the last section as the basic motivation of classical epistemology.

That motivation, it will be remembered, had its basis in the conviction that intuition or experience is a relatively scarce epistemic commodity —that it is not readily accessible in sufficient quantities to beings subject to the practical limitations (e.g. of strength, size, sensitivity to heat, flammability, etc.) that we are. Therefore, the classicist seeks a way of liberating knowledge from its meagre intuitional or experiential origins. His answer is the logic-intensive or representation-intensive stratagem. On this stratagem, warrant is identified with some property (e.g. certainty, or certainty plus such things as a priori status) that is relatively insensitive to the fine points of the cognitive mode of a warrant and focuses more on its content. As a result, it (i.e. warrant) becomes the sort of thing that can be passed on by techniques of inference that preserve relatively few of the details of the cognitive mode under which the premisses of the inference are presented as warranted.

This way of motivating classical epistemology presents a challenge to the Brouwerian. For if intuitional knowledge really is as rare and hard to obtain as the classicist says, then how can the Brouwerian hope to build a thriving epistemic enterprise while at the same time repudiating the classicist's model of epistemic growth? We believe that Brouwer's critique of the incompleteness of classical reasoning can be seen as speaking to this concern. In arguing for the incompleteness of the classical logico-linguistic method, what is being brought out is that not all the liabilities for epistemic growth lie on the side of the intuitionist. The classicist too has liabilities; there being things that he cannot prove that his intuitionist counterpart *can*. The result is that the motivation of the classicist's logic-intensive approach to epistemic expansion is to some extent blunted, since it is no longer clear that epistemic expansion via logical manipulation of the content of knowledge has greater productive potential than epistemic expansion via extension of intuition.[11]

The account of Brouwerian epistemology as sketched up to this point emphasizes the effects brought about by the prominence it gives to occurrence in the *experiential mode* as an important trait of mathematical knowledge. That emphasis may, however, appear to be lacking in motivation. To address that need, we must now sound some deeper themes of Brouwerian epistemology.

Let us begin by recalling an oft-recited tenet of intuitionism that forms the cornerstone of Brouwer's outlook. This tenet is the deceptively simple, though in truth quite radical, idea that mathematics, in its essence, is a form of mental *activity*. We propose to take this emphasis on the *actional* or *practical* character of mathematics seriously, and thus to investigate the possibility of treating Brouwerian epistemology as based on a *practical* rather than a *theoretical* conception of mathematical knowledge.

[11] Of course it is true that 'theorems' like the Continuity Principle, which may at first sight strike one as bounty on the side of the intuitionist, are *false* for the classicist's viewpoint, and hence scarcely to be regarded as an advantage. The same, however, is true of the 'surplus' of the classicist. Judged from the intuitionist's vantage, it is not true, and hence not to be desired (as the intuitionist's critique of the soundness of classical logical reasoning makes clear).

On this way of looking at it, mathematics is a body of actions or capacities for action, rather than a body of truths (i.e. a *science*, in the traditional sense). Similarly, mathematical knowledge is a type of *practically-knowing-how* to perform certain actions, rather than a rational reflection on various propositions and a subsequent *intellectual-recognition-that* they are true. We also intend to take this distinction between practical and theoretical knowledge as ultimate. That is, we propose to interpret it in such a way as to imply the inconvertibility, at least to the point of epistemological equivalence, of the former sort of knowledge into the latter.[12,13] The mental activities of the intuitionist, like the attitude-takings or 'acceptances' of conventional epistemology, may be thought of as having propositional contents. But, in being *lived* or *experienced*, those contents are epistemically 'registered' in a way that is not reducible—at least not without epistemic loss—to any kind of purely intellectual grasp of them. The emphasis on this 'lived-ness' or 'experienced-ness' is a way of expressing the practical character of the knowledge involved. We 'live' our activities. Thus, since mathematical knowledge is ultimately an activity or capacity for activity, it will ultimately manifest itself through our experiencing of our practical lives.

(NB. In addition to this, the emphasis on experience may also be partly an attempt to express the idea that there is somehow something of greater value in a kind of knowledge that brings with it a capacity to *do* something than in a kind of knowledge which consists solely in an intellectual 'acknowledgement' or 'acceptance' of a proposition. Genuine knowledge—so the idea would go—enlivens and enables. It moves to action. It is more than just the doffing of one's intellectual hat to a proposition. Practical knowledge therefore penetrates to a level of our cognitive being to which theoretical or purely intellectual knowledge typically does not.[14])

In an epistemology thus dominated by a practical conception of knowledge, it should come as no surprise that such accoutrements of the theoretical or scientific conception of knowledge as the use of logical inference and the axiomatic method are devaluated, and concern for the convertibility (or, to use the term that we have been using, the 'transformability') of one activity or practical capacity into another put in their place. Thus, on the epistemology being sketched here, an area of

[12] Why not see the logical mastery of a scheme of implications as a type of practical knowledge? Surely the intuitionist would not want to deny that the extraction of logical implications is in some sense a form of mental activity. He must, therefore, hold the view that there is a basic difference between the mental activity that constitutes his mathematical construction-making and that mental activity which constitutes the extraction of logical implications. In the final analysis, of course, the Brouwerian will be obliged to offer some account of what that difference is.

[13] It should perhaps be pointed out that characterizing logical analysis as mental activity does not automatically offer the classicist a way around Poincaré's Concern. For, as mentioned in the preceding footnote, that problematic could just as well be stated as a difference between two types of mental activity—logical and truly mathematical.

[14] One might think of this as one way of sounding the Kantian theme which emphasizes the primacy of practical over theoretical reasoning.

mathematical thought (the correlate of a *mathematical theory* under the traditional conception) is to be thought of as a body of actions organized by a scheme of actional connections reflecting some sort of practical disposition to pass from one *act* to another, rather than a body of truths organized by a network of logical relations. Likewise, in place of a plan for epistemic growth which sees it as a march from one intellectual 'acceptance' to another via the steady logical exploitation of the propositions thus accepted, towards a goal of 'complete' acceptance (that is, acceptance of the complete set of truths pertaining to the subject-matter of the science in question), there is a course of practical development which is seen as consisting in the practical transformation of one act into another in such a way as to bring one's overall mathematical activity into closer conformity to a network or 'stream' of actions which is taken to represent the ideal of an abundant mathematical *life*.

Both the goal and procedure of epistemic development thus change when one moves from a theoretical to a practical conception of mathematical knowledge. In place of a goal of 'complete' theoretical knowledge, we have the ideal of an abundant practical *life*, reckoned not (or at least not primarily) in terms of the logical properties (e.g. consistency, completeness) of the set of propositions known, but rather in terms of the practical power which its activities represent. And, in place of epistemic extension of the domain of our intellectual 'acceptances' from one proposition to another via logical inference, there is the extension of our practical capacities which is based on the acquisition and realization of dispositions which link one mathematical activity to another. Thus, our 'local' or individual proof-activities come to be bound together into a global whole (a *life*) by a scheme of relations which are not constituted by the logical relations which prevail among their propositional results, but rather by their actional or behavioural affinities to one another. Different local proof-activities are thus to be seen as exhibiting not only a logical relationship between their contents, but also a global relationship of 'fit' or 'continuity' which reflects a *practical* disposition to move from performance of one act to performance of another in such a way as to draw nearer to the ideal of an abundant mathematical *life*—defined too in terms of *practical* accomplishments and capacities. Correct global orientation at a given locale (i.e. for a given local proof-activity) is thus a matter of that local activity's being dispositionally related to other local proof-activities in such a way that, allowed to develop in a natural way, they would grow into a body of proof-activities having the sort of practical potency that is seen as being constitutive of mathematical maturity.

One feels, of course, the need for some description of the abovementioned dispositions which characterizes them as something other than a set of dispositions which, allowed to develop naturally, would lead to global configurations of proof-activities (mathematical lives) having the desired sort of potency and integrity. One needs a description of them which reveals why they should be expected to lead to a body of proofs having the desired global integrity. Perhaps Brouwer's singling out of the unfolding of the bare notion of two-ity in the mind at perfect rest, with no 'sinful' designs on the conquest of nature, and no 'cunning' or even 'playful' attempts to manipulate the stream of inner experience, can be seen as bearing on such a concern: those proof-activities which are dispositionally related to other proof-activities in such a way as to grow into the right sort of global practice are those of the mind at perfect (causal-manipulatory) rest, with no designs on causal dominion over nature or even over one's own stream of inner mathematical experience.

The practical-knowledge model just sketched is but one attempt to flesh out, in a Brouwerian manner, the central theme of our argument: namely, that, as Poincaré pointed out, mathematical reasoning appears to differ fundamentally from logical reasoning, and that in order to account for this difference one must seemingly reject the classical logic-intensive epistemology for mathematics. It is not, however, the only way of proceeding, as we shall now briefly attempt to indicate by sketching a Brouwerian theoretical-knowledge model of mathematical knowledge.

On this model, the emphasis on the 'experienced-ness' or 'lived-ness' as the distinctive cognitive mode of mathematical knowledge, which figured so prominently in the practical-knowledge model, is replaced by an emphasis on the 'locality' of one's theoretical knowledge. Basically, the idea is this: one's knowledge of a mathematical truth p is *mathematical* to the extent that it is based on a 'local' familiarity (in the sense discussed above in section 2) with the mathematical subject(s) to which p belongs. This emphasis on the 'local' character of mathematical knowledge seems to be but another way of putting Brouwer's point concerning the 'autonomy' of mathematical thought, which was that we ought to be careful to distinguish the connections between propositions which arise from the *linguistic representation of* mathematical reasoning from the connections between propositions which characterize that reasoning itself, and therefore not attribute to mathematical reasoning 'regularities in the language which accompany it' (cf.Brouwer (1955), pp. 551–2). 'Regularities of language' are to be expected to be of a global character, since languages are intended to constitute global schemes of representation; that is, schemes of representation designed not with the representation of some one body of thought in mind, but rather of *all* bodies of thought generally. It is therefore not to be wondered that the linguistic representation of mathematical thought should induce a global logical structure on its theorems. Nor is any harm done by this so long as it is remembered that this induced logical structure (which truly deserves to be called logical because of its global character) is to be taken as a structure imposed by the representing device, and not as the structure of the thought being represented. As

Brouwer said, 'Mathematical language, in particular logic, can never by itself . . . deduce a mathematical state of things' (cf. (1954), p. 524).

Whatever structure is exhibited by genuinely mathematical reasonings thus appears to be of a more 'local' character, determined by the subject of the constructional thinking in question. Such constructional thinking may, of course, be divisible into steps or parts in a variety of ways. But the question is which decompositions into steps actually correspond to the step-structure exhibited by a genuinely mathematical piece of reasoning, and which merely represent different not-genuinely-mathematical ways of systematically decomposing the same complex thoughts. Brouwer's emphasis on the 'autonomy' of mathematics with respect to logic, suggests that though logical structures may sometimes be superimposed on complex mathematical reasonings, they just represent a sort of 'tacking together' of mathematical affirmations (cf. (1933), p. 443) that is 'co-extensional', as it were, with the genuine mathematical reasoning on which it is superimposed. They do not, however, generally reflect the structure of that reasoning *considered as genuinely mathematical reasoning.*

There is thus an epistemological basis for a Brouwerian repudiation of classical logic even if one inclines to a theoretical rather than a practical conception of mathematical knowledge, and one also wishes to avoid any appeal to private phenomenological characteristics of such theoretically conceived knowledge. And the key element of that basis is nothing other than Poincaré's point concerning the 'locality' of genuinely mathematical knowledge. Hence our emphasis on Poincaré's point as furnishing a basis for Brouwerian epistemology.

5. *Intuitionist logic*

We would like to close by making a few remarks about an implication of our position that seems likely to puzzle; namely, the great disparity between what we have portrayed as the Brouwerian intuitionist's attitude towards logic, on the one hand, and that of the present-day intuitionist, on the other. The latter extends a much greater role to the use of logical inference in mathematical reasoning than does the former. In this the final section of the paper, we shall briefly consider some of the assumptions made by the present-day view in order to determine what would be required for their justification. Particularly, we shall centre our attention on those assumptions concerning the manipulability of mental mathematical constructions that are needed for the defence of intuitionist logic, and consider their plausibility as structural characteristics of a domain of mental constructions of the practical or theoretical varieties described in the last section. In the end, our finding is negative: the assumptions concerning the manipulability of mental mathematical constructions that seem to be needed for the defence of intuitionist logic are not plausible when considered as features of the practical or theoretical knowledge described in the last section.

To begin our discussion, let us consult the *locus classicus* of present-day intuitionism—namely, Heyting (1956), where the standard conception of mathematical constructions as proofs is introduced as follows:

> ... a mathematical proposition **p** always demands a mathematical construction with certain given properties; it can be asserted as soon as such a construction has been carried out. We say in this case that the construction *proves* the proposition **p** and call it a *proof* of **p**.
>
> (p. 102)[15]

Having thus characterized the basic condition of proof as possession of a construction, Heyting then went on to give a more detailed description of the specific assertion-conditions pertaining to the different kinds of compound propositions that can be formed by applying the various logical operations to simpler propositions. Naturally, this description was guided by a certain view of the dynamics of construction-possession; that is, a view of the general laws according to which possession of a given set of constructions induces possession of others. Thus, an inductive scheme stating how possession of constructions for logically compound propositions is related to possession of constructions for simpler propositions is given.[16] Using '$\pi(\chi,A)$' to stand for 'χ is a construction which proves A', that scheme is something like the following:[17]

(i) $\pi(\chi, A\&B)$ iff $\chi = <\chi_A, \chi_B>$ and $\pi(\chi_A, A)$ and $\pi(\chi_B, B)$.
(ii) $\pi(\chi, A \vee B)$ iff $\chi = <\chi_A, \chi_B>$ and $\pi(\chi_A, A)$ or $\pi(\chi_B, B)$.
(iii) $\pi(\chi, A \rightarrow B)$ iff for all constructions κ, if $\pi(\kappa, A)$, then $\pi(\chi(\kappa). B)$.
(iv) $\pi(\chi, \neg A)$ iff for all constructions κ, if $\pi(\kappa, A)$, then $\pi(\chi(\kappa), \perp)$, where $\perp$ is some agreed-upon intuitionistically refutable proposition.
(v) $\pi(\chi, \exists x Ax)$ iff there is a (term-) construction τ and a (proof-) construction κ, such that $\chi = <\kappa, \tau>$ and $\pi(\chi, A(\tau))$.
(vi) $\pi(\chi, \forall x Ax)$ iff for each number n, $\pi(\chi(n), \mathrm{A}(\mathbf{n}))$, where '$\mathbf{n}$' is the standard numerical term corresponding to n.

Basic to these conditions, which purport to state the laws that regulate the interaction between construction-possession for simple propositions and construction-possession for compound propositions, are a set of

[15] Similar remarks can also be found in Heyting (1934), pp. 14f.

[16] Cf. Troelstra (1969), pp. 6f; Dummett (1977), pp. 12f; van Dalen (1979), pp. 133–4; McCarty (1983), pp. 122–6.

[17] Obviously, our statement of the assertion-conditions for the conditional and the universal quantifier leave off the well-known 'second clauses' (stating that the right-hand side has been proven) that Kreisel (1962) urged as necessary in order to insure that intuitionistic decidability of the intuitionistic notion of proof. This should not, however, be taken to suggest that we regard them as unnecessary, but only that their presence or absence is of no essential concern to the present discussion.

structural constraints induced upon the domain of intuitionistic constructions by the following general conditions: (1) that any two constructions can be 'paired' in order to yield a new construction, (2) that constructions constituting proofs for compound propositions can be decomposed in such a way as to yield constructions for select simpler propositions, and (3) that there are constructions that can be applied to constructions in order to yield constructions.

The need for the first condition is illustrated by clause (i), taken in the right-to-left direction (which sanctions the introduction rule for the & operator). There, the possession of separate constructions for A and B is parlayed into possession of a construction for $A\&B$ by 'pairing' the individual constructions for A and B into a single construction which then serves as a construction for $A\&B$.

The need for the second of the above-mentioned structural conditions may also be illustrated by reference to clause (i), this time, though, taken in the left-to-right direction (which sanctions the elimination rule for &). There, one begins with a single construction for the compound proposition $A\&B$ and decomposes it into component constructions which are constructions for A and B. Thus, decomposition of constructions for compound propositions into constructions for simpler propositions (in the case of &-compounds, actual components of the compound proposition for which it is assumed that one has a construction) is assumed to characterize the realm of intuitionistic constructions.

The need for the third of the above-mentioned structural conditions is well-illustrated by the clause for the conditional operator. There it is assumed that the construction χ is the sort of thing that can itself be *applied to* constructions for A in order to obtain constructions for B. Thus the domain of intuitionistic constructions is taken to contain certain constructions which themselves are the sorts of things that operate on constructions with the result of producing other constructions.

The effect of conditions (1)–(3) is thus that of creating a kind of 'algebra' for the domain of intuitionistic constructions. The task facing us is that of determining how plausible the 'algebra' thus conjectured is when it is taken as an algebra for the constructions of the practical-knowledge or theoretical-knowledge models of the last section.

For the sake of concreteness, we shall present our argument with specific reference to the operation of logical conjunction. Arguments similar to the one we are about to present could be given with respect to the other operations as well, but the problems which we are concerned to bring to light arise in their clearest and simplest form in the case of conjunction. Also, the focus on conjunction seems to be strategically well-taken since present-day intuitionists appear to regard it as the least problematic of all the logical operations. Thus, any problems detected in connection with it are likely to point to basic difficulties. Let us consider, then, the claim that a proof of *A*&*B* may be obtained from a proof of *A* and a proof of *B* by a supposed operation of 'proof-pairing'.

It is easy to miss the problem presented by such a claim. For it is tempting to mistake what is, in actuality, a claim concerning the behaviour of a *natural* kind of mental activity (namely, the intuitionist's mental mathematical constructions) for a claim concerning some sort of purely logical or conceptual possibility. In other words, it is tempting to think that the claim in question can be defended by simply pointing to the conceptual possibility of binding a proof of *A* and a proof of *B* into some sort of unit, or considering them 'together' in thought.

On the practical-knowledge model sketched in the last section, the mental mathematical constructions of the intuitionist form a *natural* kind; that is, a kind of activity whose laws of combination are governed not by mere conceivability, but rather by the laws governing the natural kind of the activity involved. The mere fact (if it is a fact) that we can always conceive of a construction of *A* and a construction of *B* being bound together into a single construction of *A*&*B* is not enough to show that the natural kind of mental mathematical construction also operates in this way. We can perform logical operations on the contents of our constructions, but they may run transverse to rather than being coincident with those laws governing the stream of constructional activity as a natural epistemic kind. 'Pairing' is thus a mere tag for an undescribed and rather dubious (because it runs contrary to the generally observed autonomy of constructional activity from the machinations of logic) mental operation that is supposed to allow us to take any two separate constructional acts and turn them into a single (complex) act whose content is the conjunction of the contents of the separate experiences.

Similar remarks apply to the theoretical-knowledge conception. The fact that one has a local proof of *A* and a local proof of *B* does not imply that one either does or can have a local proof of *A*&*B* (though this might of course hold for certain particular *A* and *B*, and even for certain limited *kinds* of *A* and *B*). This is particularly clear if *A* and *B* are drawn from different local settings, but it is to be generally expected even for *A* and *B* drawn from the same local setting, since the reasoning according to which the local proof of *A* and the local proof of *B* are to be bound together is not local but rather global reasoning. Hence, it is not by local insight that the two proofs are bound together, and this may be enough to deprive the compound proof of status as local reasoning.

The attractiveness of conjunction-introduction and the other rules of 'intuitionist logic' may be owing in large part to the fact that, since Heyting's original formalization of intuitionist logic, most of the work done on the subject has been of a technical rather than a philosophical nature and that this work has yielded clear, precise, and effectively executable *syntactical counterparts* for the mentalistic operations on proof-constructions (namely, proof-pairing, proof-decomposition, and construction-application) that would be required by a genuine logic of intuitionistic reasoning. Being effectively executable, these syntactical operations are, of course, intuitionistically acceptable, and so it may be that this has led some to lose sight of the fact that, though they are of an intuitionistically acceptable character, they are none the less not procedures for operating on intuitionistic proofs *per se*, but only on certain of their *syntactical representations*.

Nor do such syntactical or formal operations on proof-representations *suggest* any clear parallel operation at the level of genuine mentalistic proof. This can, perhaps, be made clear by considering the syntactical counterpart of proof-pairing; namely, syntactical concatenation. Concatenation is an operation that calls for the sequential arrangement of concretia in space-time. However, one certainly cannot produce a 'compound' mental proof by laying two 'smaller' proofs end-to-end. Indeed, it is unclear what it would *mean* to lay mentalia end-to-end. Even adding the (by no means obvious) supposition that mental states are to be regarded as concretia of some sort (e.g. brain states), what would syntactical concatenation suggest as a parallel at the level of mentalia? Spatial contiguity of brain states can hardly be expected to be what 'concatenation' of mental proofs would come to. Nor can temporal contiguity, since there are many temporally contiguous mental states that cannot be made into any kind of meaningful mental unity at all (e.g. a state corresponding to a mental mathematical construction followed by a state corresponding to my being startled by a loud noise).

It therefore seems clear that the syntactical operation justifying conjunction-introduction in *formalized intuitionist logic* cannot be taken as justifying conjunction-introduction as a rule of *genuine intuitionistic proof*, since though 'concatenation' may be clear as an operation on syntactical entities, it gives no indication of what its counterpart at the level of mental proof might be.[18]

The above remarks are directed at a conception of intuitionistic conjunction which sees it as based on the possibility of pairing proof-constructions of A and B to form a new proof-construction whose content is $A\&B$. There is, however, an alternative way of thinking of intuitionistic conjunction (cf. Dummett (1977), p. 12 and Martin-Löf (1983), *passim*). On this view, a proof of $A\&B$ is not to be seen as some third, compound entity (χ_A, χ_B), distinct from both χ_A and χ_B (the proofs for A and B, respectively), yet formed by somehow combining them into a single construction whose content is $A\&B$. Rather, it is to possess the separate,

[18] Some present-day authors who accept intuitionist logic are aware of some of the difficulties involved in treating intuitionistic proofs as mental processes rather than as syntactical entities, even though they do not seem to us to fully appreciate the consequences of what they are suggesting. Cf. Martin-Löf (1983, 1984, and 1987) and Sundholm (1983).

individual constructions χ_A and χ_B in a certain way; to have them, as it were, 'simultaneously'.

This way of thinking of conjunction avoids the need to show that there is a mental pairing operation that preserves local character (in the case of the theoretical-knowledge approach to intuitionist epistemology) or that preserves the natural kind of intuitionist constructional activity (in the case of the practical-knowledge conception) when it is used to join separate proofs to form a compound proof of their conjoined contents. For it allows there to be a construction of the compound proposition $A\&B$ without having a single construction whose content is the conjunction of the contents of a construction of A and a construction of B. On this account, possession of separate constructions for A and B is all that is required, provided, of course, that that 'possession' is understood as providing equal access to both constructions at a given time.

Such an account may be adequate so far as conjunction-elimination is concerned since all it requires is that a proof of $A\&B$ be the sort of thing that affords one access to both a proof of A and a proof of B—and 'simultaneous' possession of proofs for A and B is exactly that sort of thing. Conjunction-introduction, however, is another story. There, it seems that a proof of $A\&B$ is to be more than a mere provider of equal access to individual proofs of A and B. It is to be something that moves from simultaneous possession of separate proofs for A and B to a proof which synthesizes their respective contents. Indeed, it is that alone which qualifies it as a candidate for genuine inference. For genuine inference demands that there be some 'movement' (i.e. some change of mental state) in going from the premisses of an inference to its conclusion. Hence, whatever state it is that is taken to constitute an epistemic grasp of the conclusion must be different from that which is taken to constitute a grasp of the premisses.

This condition is satisfied for conjunction-elimination even when possession of a proof for $A\&B$ is taken to consist merely in what we have been calling 'simultaneous' possession of proofs for A and B. For the conclusion of an inference by conjunction-elimination demands only a grasp of a proof for A or a proof for B, whereas the premiss demands a simultaneous grasp of both. But the same is not true of conjunction-introduction. There, the premisses already demand simultaneous possession of a proof for A and a proof for B rather than mere separate possession of a proof for A and a proof for B, since one needs to hold the proofs together as a unit of some sort in order to ascend to the conclusion. Having a proof of A and a proof of B, but not being able to bring them together, would not allow one to do anything other than offer a proof of A and, separately, a proof of B. Thus, there is a difference between holding a proof of A and a proof of B separately, and holding them together as a unit of some sort (i.e. holding them 'simultaneously'), and it is the latter which is required by the premisses of a conjunction-introduction.

This being so, and it also being the case that genuine inference demands 'movement' of the sort described earlier, we may infer that the conclusion of an inference by conjunction-introduction demands more than just the simultaneous possession of proofs of A and B—at least to the extent that conjunction-introduction is to constitute a form of genuine inference. The extra that is needed is a synthesizing of the contents of the proofs of A and B that introduce the premisses. The intuitionist's mental mathematical proof-construction (as opposed to object- or term-constructions) are *intentional* states; that is, mental states that are *about* (constructed) objects, and which thus have a propositional content.[19] Hence, ordinarily, or perhaps we should say *canonically*, to have a mental mathematical (proof-) construction for a proposition σ is to be in an intentional state μ having σ as its content, and occurring in a certain mode M_μ which reflects its 'local' origins (in the case of the theoretical-knowledge model), or its membership in the natural kind of intuitionistic constructional activity (in the case of the practical-knowledge model). Derivatively, or non-canonically, to have a mental mathematical construction for σ is to be in a mental state μ, the being in of which facilitates access to (i.e. puts one—at least ideally—in a position to produce) a canonical mental mathematical construction μ whose mode is M_μ and whose content is σ.

In either case, however, having a mental mathematical proof-construction of $A\&B$ requires either being in or having access to an intentional state whose content is $A\&B$. This makes having a proof-construction for $A\&B$ different from merely having a single means of producing both an intentional state whose content is A and an intentional state whose content is B, which is what 'simultaneous' possession of a proof-construction for A and a proof-construction for B comes to. Hence, to be a genuine form of inference, conjunction-introduction must be seen as moving from 'simultaneous' possession of proof-constructions for A and B to a proof-construction having $A\&B$ as its content. This being so, the view of intuitionistic conjunction which maintains that to have a proof-construction of $A\&B$ is just to have a proof-construction of A and a proof-construction of B cannot be accepted.

We have thus examined the two contemporary conceptions of intuitionistic conjunction and found them both lacking any plausible account of how conjunctive inference might be blended into the intuitionist's mental procedures. And what has been said of conjunction would appear to apply to other parts of intuitionist logic as well. We conclude, therefore, that the role of intuitionist logic in intuitionist mathematical reasoning is quite

[19] Indeed, one might understand this as the source of the intuitionist's insistence on the construction of mathematical objects—without such construction, objects simply cannot be 'given' to the mind is such a way as to allow genuine mental states (i.e. states having content). The interplay between 'percepts' (= objects of construction) and 'concepts' that such a view suggests may constitute another important respect in which intuitionist epistemology is Kantian in character. Since writing this paper, I have discovered a similar point in Tieszen (199?).

suspect, and that the 'algebra' of constructions upon which that logic is supposed to be based is rather dubious as a description of the ways in which complex constructions are formed intuitionistically.

6. *Conclusion*

The account of Brouwerian intuitionism sketched in this paper is one which seeks to give it a new emphasis. Instead of the privacy or interiority of Brouwer's 'autonomic interior constructional activity' (cf. Brouwer (1955), p. 551), we have stressed its *autonomy* and claim to find in this autonomy a basis for his radical rejection of logic which avoids the solipsistic pitfalls of an approach based on interiority.[20] We have, moreover, presented two different ways that one might go about developing this emphasis on autonomy: one a practical-knowledge model which stresses the autonomy of structure which proof-construction, as a natural kind of activity with its own natural kind of 'movement', enjoys, the other a theoretical-knowledge model where autonomy derives from the fact that what powers the flow of knowledge is local insight rather than global inferential possibilities. Both provide bases for minimizing the role of logical inference in mathematical reasoning.

Both, too, are arrived at as transcendentally deduced hypotheses designed to explain a prior 'datum' of mathematical epistemology—namely, the observation that there is a seemingly important difference between the epistemic condition of the genuinely mathematical reasoner and Poincaré's 'logician' (i.e. one who arrives at her conclusions via a series of logical machinations). This observation does not so much call into question the feasibility or plausibility as the very *desirability* of an intuitionistic logic. For it suggests that, should the intuitionistic logician's advice be followed, the plausibility of intuitionist epistemology would be put in jeopardy, since it would then lose the ability to account for the difference between the epistemic quality of reasoning that is based on a genuine mathematical knowledge and inference that is not.

We believe that Poincaré's observation has been unjustifiedly ignored as

[20] Cf. Brouwer (1948), pp. 480–2. In addition to dropping this emphasis on the interiority or privacy of constructional mental activity, we also drop Brouwer's scheme of distinctions separating various causal-active and passive-reflective ways of extending one's mathematical experience. In his view, the epistemic quality (which may for him have really been a form of aesthetic quality) of the 'unfolding' of the fundamental notion of two-ity is affected by the degree to which wilful causal manipulation is involved in its production. 'Shrewd' or 'cunning' causal manipulation, whose aim is the 'sinful' one of trying to control the stream of one's experience out of a preoccupation for one's own pleasure produces experience of the lowest quality. Better quality results from a less calculating type of causal activity—termed 'playful' by Brouwer—whose aim is to extend experience 'without inducement of either desire or apprehesion or vocation or inspiration or compulsion'. There is 'constructional beauty' in such playful causal activity, and it affords a 'higher degree of freedom of unfolding' and greater 'power, balance, and harmony' than shrewd causal manipulation. Still, playful causal unfolding of experience is inferior to the wholly free unfolding of experience, where one finds the 'fullest constructional beauty'.

a desideratum of mathematical epistemology. Likewise, we judge Brouwerian intuitionism, with its emphasis on privacy dropped and refocused on the autonomy of mathematical reasoning (with the result that it is able to respond to Poincaré's concern), to have been underestimated as a mathematical epistemology. Our hope is to have taken some steps towards correcting these oversights, and to have revealed some of the interesting and challenging features of the views of Brouwer and Poincaré which, for the most part, have escaped the notice of contemporary philosophers of mathematics.

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