Working draft for presentation at the Institute of European and American Studies, Academia Sinica, 11/07.
Please do not cite without permission.

Computers, Mathematical Proof, and A Priori Knowledge

Kai-Yee Wong

Philosophy Department
The Chinese University of Hong Kong

1. Introduction

The computer played an essential role in the proof given by Kenneth Appel and Kenneth Henken of the Four-Color Theorem (4CT).[1] First proposed in 1852 by Francis Guthrie, the four color problem is to determine whether four colors are sufficient to color any map on a plane so that no adjacent regions have the same color. Appel and Heken's proof involves a lemma that a certain 'avoidable' set *U* of configurations is reducible. The proof of this critical lemma requires certain combinatorial checks which are too long to do by hand. The job was done by an IBM 370/168, using over 1200 hours of computer time. In 1977, Appel and Heken, assisted by John Koch, published the proof, and the 4CT has since been considered an established result. No one has seen the entire proof of the reducibility lemma. It was too long to print out; even if it had been, no one would be able to run through it step by step.

The proof of the 4CT has generated a flurry of philosophical discussions about its significance. Some of them focus on the arguments put forth by Thomas Tymoczko's 'The Four-Color Problem and Its Philosophical Significance' (1979). My chief interest will be to comment on the avowed centerpiece of Tymoczko's paper, namely, his claim that the proof of the 4CT is partially empirical in character. According to Tymoczko, the argument for the 4CT is like 'an argument in theoretical

---

[1] This exposition is based on Tymoczko (1979) and Wang (1981).

physics where a long argument can suggest a key experiment which is carried out and used to complete the argument' (Tymoczko 1979: 78) because there is an unavoidable reliance on computers to produce the proof of the theorem. Since belief in the reliability of computers ultimately rests on empirical considerations, the proof establishes the 4CT on grounds that are in part empirical. So, the 4CT is a substantial piece of knowledge which can be known only a posteriori.

Tymoczko's papers was published in 1979, but the issues it raised are still of great interest to many.[2] Tymoczko's arguments are deservedly controversial as they are subtly connected with some fundamental issues in epistemology, such as the characterization of apriority, the roles for experience in mathematical argument, the nature of knowledge through testimony, and so on. This paper will not attempt to do justice to the complexity of this web of connections. Instead I will focus on a central thread in Tymoczko's reasoning and Michael Detlefsen and Mark Luker's contention that it rightly leads to the conclusion that *typically* mathematical proofs are *empirical*.

2. The 'Simon says' Parable

We shall do well to start with a brief look at Tymoczko's characterization of the notion of a mathematical proof as he thinks it has been customarily understood. According to Tymoczko, proofs are *convincing*, *surveyable*, and *formalizable*. Of these three characteristics surveyability is the most important one that drives Tymoczko's arguments. To say that a proof is surveyable is to say that it is a 'perspicuous' construction that can be 'looked over, reviewed, verified by a rational agent' (Tymoczko 1979: 59). Typically mathematicians come to know the conclusion of a proof by surveying it in its entirety.

The computer's proof of the reducibility lemma cannot be checked step by step by mathematicians and it is not even recorded in the archives, so surveyability is not preserved in Appel and Henken's appeal to computers. The proof of the 4CT is thus not surveyable. Given this fact and the notion of surveyabiltiy as a characteristic of proofs, Tymoczko argues, we need to *modify* our concept of proof if we accept the 4CT.

[2] For a recent discussion see, see Arkoudas and Bringsjord (2007). See also Brown (1999), Burge (1998), Coady (1992: ch. 14).

Tymoczko employs the following parable to help make his point:

> Let us consider a hypothetical example which provides a much better analogy to the appeal to computers. It is set in the mythical community of Martian mathematicians and concerns their discovery of the new method of proof 'Simon says'. Martian mathematics, we suppose, developed pretty much like Earth mathematics until the arrival on Mars of the mathematical genius Simon. Simon proved many new results by more or less traditional methods, but after a while began justifying new results with such phrases as 'Proof is too long to include here, but I have verified it myself'….Oftentimes, other Martian mathematicians accepted this results; and they were incorporated into the body of Martian mathematics under the rubric 'Simon says'.

Tymoczko thinks that 'Simon says' is not a legitimate development of standard mathematics. This point, he adds, can be made more obvious by expanding on the parable in any number of ways:

> For instance, imagine that Simon is a religious mystic and that among his religious teachings is the doctrine that the morally good Martian, when it frames the mathematical question justly, can always see the correct answer. In this case we cannot possibly treat the appeal 'Simon says' in a purely mathematical context. What if Simon were a revered political leader like Chairman Mao? Under these circumstances we might have a hard time deciding where Martian mathematics left off and Martian political theory began. Still other variations on the Simon theme are possible. Suppose that other Martian mathematicians begin to realize that Simonized proofs are possible where the attempts at more traditional proofs fail, and they begin to use 'Simon says' even when Simon didn't say! The appeal 'Simon says' is an anomaly in mathematics; it is simply an appeal to authority and not a demonstration. (*ibid* 1979: 71-72)

The parable is supposed to show that the appeals 'by computer' and 'Simon says' are 'remarkably similar'. They both are appeals to authority, or a kind of testimony. But how is this supposed to show that if we accept the proof of the 4CT we are 'committed to changing the sense of the underlying concept of "proof"' (*ibid*: 58)? Presumably it is by a simply analogy. 'Simon says' is obviously an appeal to authority. Since standard mathematics essentially relies on no authority but only on demonstrative reasoning, ‘Simon says’ cannot be a legitimate development of mathematics. So, to accept ‘Simon says’ as a method of proof is to change our conception of proof. The same applies to the appeal to computers as they are ‘remarkably similar’. Obviously ‘Simon says’ is too bizarre to be acceptable. But we have reasons to accept the appeal to computers because we have strong evidence for their reliability.

The question is, Why, *in the first place*, the appeal ‘Simon says’ as an authority should be considered an apt analogy of the appeal to computers? We have no idea how ‘Simon says’ works but we do know how a computer works. Not only in the sense that we know what algorithm a computer is implementing. We know how a computer works in the more fundamental sense that it is essential a Universal Turing Machine, or, in the context of computer proof, a UTM that executes a mathematically sound algorithm. To Tymoczko, this is precisely the reason for regarding computers, in terms of their reliability, as a different kind of authority from ‘Simon says’. But why shouldn’t this difference show rather that the analogy *itself* between the two appeals was a *poor* one to begin with? The difference is so fundamental, one may object, that saying that computers are ‘in the context of mathematical proofs, another kind of authority’ (*ibid*: 72) is simply misleading.

We may leave it open whether there is any interesting sense of ‘authority’ in which both ‘by computer’ and ‘Simon says’ can be said to be ‘appeals to authority’. But the point remains that as far as the parable goes Tymoczko’s claim that the proof of the 4CT forces us to modify the concept of proof rests on a questionable analogy. Let us set out Tymoczko’s argument as follows in order to help make clear this point:

(1) ‘Simon says’ is an appeal to authority;

(2) To accept authority as a method of proof is to extend the traditional concept of ‘mathematical proof’ so much that it amounts to a modification of the concept;

(3) The appeal ‘by computer’ is analogous to the appeal ‘Simon says’; so

(4) To accept the appeal ‘by computer’ as a method of proof will force us to modify the concept of mathematical proof;

(5) The appeal ‘by computer’, in the context of the 4CT, is acceptable because

computers, unlike ‘Simon says’, are warranted authority; therefore

(6) The proof of 4CT ‘does introduce a new method into mathematics’ (ibid: 72) which forces us to modify the concept of proof.

Tymoczko simply asserted (2), but he did argue for (5) by pointing out the differences between the two appeals in their reliability. The objection I have made is that these differences, however, threaten to undermine another premise of the argument, i.e., (3). For, as I have pointed out, these differences do not reside in anything like different success rates of past performances but in our knowledge that a computer, in the context of the 4CT, is essential a Universal Turing Machine executing a mathematically sound algorithm.

This consideration also allows us to find fault with (2) even if we concede (3). Tymoczko is right that the appeal ‘Simon says’ is so bizarre that accepting it is tantamount to a modification of the concept of mathematical proof. But it is not clear that why a similar modification should be forced upon us if the ‘authority’ appealed to is a computer. In the case of the 4CT, we have a full understanding of the proof procedure underlying the program and we also know that it is mathematically sound. This is what warrants the appeal to the computer as an ‘authority’. So why accepting the 4CT should ‘force us to modify our concept of proof’ (*ibid*: 78)? (2) as it stands is by no means an evident claim; much depends on how ‘authority’ is fleshed out. (2) is obviously true in the context of a ‘Simon says’-kind of scenario, but not so, as one may object, in the context of the proof of the 4CT.

3. Computer Proof as Experiment

To be fair to Tymoczko, we need to address directly the argument the parable is supposed to help bring home. This takes us to a notion central to Tymoczko’s contention, namely, the ‘unavoidably empirical’ elements in the appeal to computers. According to Tymoczko, the appeal to computers, in the case of the 4CT, involves two claims:

(7) That every configuration in U is reducible if a machine with such a such characteristics when programmed in such and such a way produces an affirmative result of each configuration.

(8) That such a machine so programmed did produce affirmative results for each configuration.

(7) is an instance of the claim that:

(7*) If computer C running program P produces result R, then mathematical statement M is true.

(8) is an instance of the claim that:

(8*) Computer C running P produces R

The truth of (7*) turns on two factors (*ibid*: 74): the reliability of C and the reliability of P, corresponding to the following three claims:

(9) The algorithm underlying P is mathematically sound.

(10) P does what it is supposed to do, namely, to codify in a machine language a mathematically sound procedure for deciding M.

(11) C does what it is supposed to do, namely, to correctly execute P.

The question of how to evaluate (10) can be difficult to answer. But clearly (8*) and (11) are empirical claims. Obviously (11) may turn out true or false, depending on a complex set of empirical factors (e.g., whether there are faulty parts, design flaws, 'bug' flaws, damaging cosmic radiations, sabotage, and so on); and thus its truth is 'ultimately a matter for engineering and physics to assess' (*ibid*: 74).

Mathematical knowledge obtained by computer-assisted proofs is therefore grounded, in part, in the results of, a 'well-conceived computer experiment'. It follows, Tymoczko argues, that in the context of the 4CT our acceptance of the appeal to computers introduces a *new* method of proof which involves empirical ingredients. So, his argument concludes, computer-assisted proof means giving up the traditional notion of a proof as something surveyable and the traditional conception of mathematical argument as non-empirical and a priori reasoning.

4. Computers, Archives, and Testimony

A different way to put Tymoczko's argument is as follows: the proof of the reducibility lemma is based on the *testimony* of computers; and since testimony is empirical in character and its reliability can only be established empirically, using computers means redefining the concept of proof.

It does sound plausibly to say that testimony can only be established on empirically grounds and thus is extraneous to the traditional conception of mathematical proof, according to which a proof is something we can go through and see for ourselves its correctness by checking it, using reason alone. A proof gives an *a priori demonstration* of a mathematical proposition. In contrast, when we rely on testimony to come to know a mathematical proposition, we trust that our source (or someone up the chain of sources) has done the 'checking' for us. We don't go by reason alone; we employed empirical means, including others' telling me something. Something like this reasoning may be what underlies the kind of objection raised by Bernard Williams (Williams 1972) against knowing a mathematical proposition by testimony. The objection rests on a claim that commands 'widespread assent', namely, that if someone believes a true mathematical proposition on good authority but cannot mathematically demonstrate the proposition, then he does not know it. This view has been criticized by C. A. J. Coady.[3] Whether Coady's criticisms are plausible we cannot discuss here. But one can easily see why one may want to object to Williams's view by considering how someone may claim to know Protagoras's Theorem without being able to demonstrate it.

Indeed, it seems arguable that testimony has a significant role in the transmission of mathematical knowledge and this in turn may have interesting bearing on the notion of proof. Mathematics investigation, of course, builds on past results. Suppose Kodel, a mathematician, produced a proof with gaps filled by some past results, or, as we shall call them, *archived results*. He has never surveyed the proofs of these results, but obviously he came to believe them on good grounds. In other words, testimony (of the archives) plays a crucial role in his proof. So, Kodel's proof is similar to the proof of the 4CT in their reliance on testimony, only that one is *archive-assisted* and the other *computer-assisted*. If the appeal to computers, as Tymoczko argues, is a change of the traditional concept of proof because of its reliance on empirical evidence, then

[3] See Coady (1992: ch. 14).

shouldn't Tymoczko say the same about Kodel's appeal to archives? The situation now looks a bit strange. If the answer is Yes, it means Kodel's method is not *already* part of the traditional canon of mathematical proof. But this can't be correct. Kodel is utilizing a method that is part and parcel of the common practice in mathematics. Mathematics is as communal a pursuit as other sciences are. Reliance on testimony (in the form of appeal to archives) is more often than not an inevitable part of a mathematician's work. If the answer is No, then, given the similarity between Kodel's proof and a computer-assisted proof as mentioned, Tymoczko owes us an explanation of his different treatment of the two kinds of appeals.

One may say that such an explanation can easily be given if one remembers what Tymoczko has said about the characteristics of proofs. Kodel's can *survey* the archived proofs if he wishes but the calculations done by Appel and Heken's IBM 370-168 is not humanly surveyable. So, where the use of testimony in Kodel's case is a convenience, the testimony of computers, in the case of the 4CT, is indispensable. This justifies a different treatment of the two kinds of appeals.

But the point of the Kodel example is that if we adopt Tymoczko's view, we must say that the use of testimony injects into Kodel's proof empirical ingredients and render the theorem proved a piece of a posteriori knowledge, just as the 4CT is. Whether or not Kodel can survey the archived proofs has no bearing on this point. To see this, we can imagine that another mathematician, Podel, produced a proof with gaps filled by a vast set of archived results. Surveying the enormously complex and numerous proofs for these results is such a mammoth task that no mathematician can finish.[4] The fact that these proofs have been surveyed in the past will not change the situation. For without a first-hand survey of each proof, Podel must rely on empirical evidence that the archives are a reliable source of mathematical results. It is *arbitrary* to claim that Kodel's theorem does not rely on any empirical evidence because his proof is not as *extensive* as Podel's in its appeal to the archives. So the difference (as stated in the above paragraph) between Kodel's case and the 4CT is not a reason to treat them differently.

---

[4] If this sounds far-fetched, consider the now accepted proof of the classification of all simple finite groups. The proof, carried out over many years by a large number of mathematicians, spans across about 15,000 pages on journals. This example is from Brown (1999: 158).

I have tried to show that Tymoczko's view is incoherent. If the proof of the 4CT does *initiate* a conceptual change, then the concept of proof cannot *already* be empirical in character. But by dint of reasoning similar to Tymoczko's own, we should conclude that archive-assisted proofs and thus a large part of traditional mathematics is already of an empirical sort. One may also take this as the basis of a reductio argument against Tymoczko's view if one takes it that one must get something wrong in order to see empirical ingredients in Kodel's or Podel's proof.

5. Gauss's Proof and Empirical Proof in Traditional Mathematics

Yet one may not agree that one must get something wrong in order to see empirical ingredients in Kodel's or Podel's proof. As a matter of fact, some have argued that much of traditional mathematics is partially empirical. Commenting on Tymoczko's paper, Detlefsen and Luker (1980) argue that if one follows the logic of Tymoczko's reasoning where it leads, one is forced to see empirical ingredients in proofs that are generally held to be paradigms of a priori mathematical arguments, such as the following one attributed to the young Gauss, which Tymoczko himself has used as an example of such paradigms.

Gauss proved that the sum of the first one hundred positive numbers was 5,050 in the following way: write down the numbers from 1 to 100 in pairs as shown below:

1	2	3	….	49	50
100	99	98	….	52	51

Observe that the numbers of each pair together make 101 and that there are 50 pairs. Conclude that the sum of the first one hundred numbers is 5,050.

According to Detlefsen and Luker, an episode of calculation – the one by which we determinate that 101 multiplied by 50 is 5,050 – is needed in the reasoning from the 'observation' that the sum of each column is 101 to the 'conclusion' that the sum of the first 100 numbers is 5,050. Without this episode of computation, one cannot arrive at the conclusion with *conviction*. Now if we adopt the view (to which Tymoczko subscribes) that a unit of reasoning contains within itself everything that is needed for conviction, then this computation is a necessary part of the proof. Four assumptions, Detlefsen and Luker adds, are required for confidence in the result of any computation (Detlefsen and Luker 1980: 808)

(a) that the underlying algorithm to be used is mathematically sound;

(b) that the particular program to be used is a correct implementation of this algorithm;

(c) that the computing agent correctly executes the program;

(d) that the reported result was actually obtained.

Since the validity of (c) and (d) rests on evidence of an empirical sort, the reasoning embodied by Gauss's proof, of which some episode of computation is a necessary part, must be seen as *utilizing empirical premises*. Detlefsen and Luker go on to argue that since (c) and (d) is analogous (11) and (8*) above, Gauss's proof is straightly analogous to the 4CT in its appeal to experience. They then conclude that, employing his reasoning consistently, Tymoczko is forced to see empirical ingredients in Gauss's proof too.

Since Gauss's little proof is surveyable if anything is, Detlefsen and Luker's conclusion that it is partially empirical, if correct, casts doubt on Tymoczko's analysis of the connection between empirical proof and unsurveyability. Indeed, Detlefsen and Luker claims that 'the surveyability of proof does not guarantee that it will not be based on empirical considerations' (*ibid*: 814). This is because even a first-hand survey (checking with paper and pencil, for instance) made by a mathematician 'may still be based on empirical considerations such as those mentioned earlier in this paper [i.e., (c) and (d) above]' (*ibid*: 814). So, even 'the appeal to first-hand survey introduces the same kind of method into mathematics as is introduced by the appeal to computer' (*ibid*: 816). They conclude that, though Tymoczko is right in thinking that the 4CT is partially empirical, he is totally off the mark in his claim that the proof of 4CT initiates a *new* concept of proof: 'empirical proof is a relatively widespread phenomenon' (*ibid*: 809) and much of traditional mathematics has always utilized empirical considerations.

6. Chisholm, Memory, and the Empirical-Reliability-of-X-Argument

It is easy to see that Detlefsen and Luker's argument commits them to a highly restrictive account of a priori mathematical proof, which seems to allow hardly any mathematical argument to be counted as a priori or 'by reason alone' unless it is immediately evident. For if Detlefsen and Luker are correct to count the calculation in Gauss's proof as empirical, then almost all arithmetic, multi-stepped calculations should be treated similarly because the multiplication of 101 by 50 is a quite simple calculation. Note also that Detlefsen and Luker's view also commits them to a highly restrictive account of a priori, or demonstrative, reasoning *in general*. For it seems clear, as I shall argue presently, that, if following their strategy consistently, one should see empirical ingredients in almost *all* instances of ordinary demonstrative reasoning generally held to be 'a priori' as the term is intuitively characterized.

In any inference involving more than a few steps, we need to rely on our memory to store and retrieve premises. When an inference gets more complex, we must resort to paper and pencil or similar means to facilitate the derivation, and so recourse to perception is also required. It is clear that the reliability of our memory and perception is not amenable to demonstration by reason alone. Does this shows that if, in the course of a deduction, we appeal to our memory and perception, the conclusion deduced will depend for its support on particular assumptions about our memory and perception, such as that there has not been a slip of memory or misrepresentation of symbols? It will be hard to see how one can answer 'No' if one accepts Detlefsen and Luker's claim that even such a simple computation as 101x5 is based partly on empirical considerations.

Let's call the form of reasoning behind Detlefsen and Luker's view the *Empirical-Reliability-of-X Argument*: if X is something we must rely on in order to see the correctness of a proof or demonstration, then (where the reliability of X is an empirical matter) the empirical evidence for the reliability of X is *part* of the proof or demonstration, or, in slight different terms, the proof or demonstration must be seem as utilizing empirical premises. We have argued that substituting memory (and/or perception) for X, we get the conclusion that ordinary demonstrative arguments customarily regarded as a priori are partially empirical.

Implausible as this conclusion may seem, the argument is, in a way, echoed by

Roderick Chisholm. In his long standing general treatise *Theory of Knowledge* (1989), Chisholm proposes an account of the a priori which rules out all results of complex proofs. He defines ‘p is known a priori by S’ as: ‘there is such an e such that (i) e is an axiomatic proposition for S,[5] (ii) the proposition e implies p and (ii) S accepts p’.

He then asks whether complex proofs produce a priori knowledge,

> What if S derives a proposition from a set of axioms, not by means of one or two simple steps, but as a result of a complex proof, involving a series of interrelated steps? If the proof is formally valid, then shouldn’t we say that S knows the proposition a priori? I think that the answer is no.

He cites John Locke in explaining this answer:

> Complex proofs or demonstrations, as John Locke pointed out, have a certain limitation. They take time. The result is that the ‘evidence luster’ of the early step may be lost by the time we reach the conclusion: ‘In long deductions, and the use of many proofs, the memory does not always so readily retain.’ Therefore, he said, demonstrative knowledge is ‘more imperfect than intuitive knowledge’.

He adds,

> if in the course of a demonstration, we must rely upon memory at various stages, thus using as premises contingent proposition about what we happen to remember, then, although we might be said to have ‘demonstrative knowledge,’ we cannot be said to have an a priori demonstration of the conclusion. (Chisholm 1989: 29-30)

Chisholm (in the first passage cited) refers only to the results produced by *complex* proofs. He attributes the decrease or loss of ‘evident luster’ to the less readiness of memory in retaining the premises or steps. So, presumably only long deductions, whose ‘evident luster’ is affected by the less readiness of the memory, should be questioned regarding their status as a priori demonstrations. However, in the last passage cited above, Chisholm seems to provide us a somewhat different reason for

[5] Chisholm defines ‘axiomatic’ thus: *p is an* axiomatic proposition if and only if p is a necessary truth such that for every S, if S accepts *p*, then *p* is certain for S. See Chisholm (1983: 28).

questioning the ‘a priori’ status of a wider set of deductions. The reason there is that *contingent premises* (about our memory) has been *used*; while in the earlier passage the reason is that memory gets ineffectual in retaining premises over many steps, and nothing is said about memory itself figuring in the premises (and thus making them contingent). I think the two kinds of reasons are significantly different. But let us put this aside. What I have wanted to say is that these reasons seem to support the claim about ordinary demonstrative reasoning being based on empirical considerations, which I have shown to be a consequence of Detlefsen and Luker’s reasoning. Chisholm’s view is admittedly less general as he talks only about demonstrations utilizing contingent propositions about *memory* as premises. Still he can be seen as putting forth a version of the Empirical-Reliability-of-Memory Argument for empirical proofs generally held to be demonstrative and a priori.

We have yet to follow through the logic of the Empirical-Reliability-of-X Argument. The validity of ordinary formal reasoning relies on the proper functioning of our brains. Normally, unless there are reasons to think otherwise, we assume the reliability of our brain processes. Yet such an assumption can only be grounded ultimately on empirical evidence. After all, the reliability of our memory and perception assume the proper functioning of our brains. So, the argument concludes, most ordinary formal deductions or demonstrations are based in part on empirical considerations regarding the reliability of our *brains*. This is the ‘brain’ version of the Empirical-Reliability-of-X Argument.

7. Apriority and Two Roles for Experience

What do we make of the Empirical-Reliability-of-X Arguments? Each of these arguments concludes that certain sort of demonstrative proofs commonly taken as a priori are in fact partially empirical. For those who find such a conclusion implausible, the argument serves better as the first half of a reductio argument that refutes Tymoczko’s or Detlefsen and Luker’s or Chisholm’s view. I go along with this line of response. Lest this may seem too quick and cheap, I shall say something to back it up.

Let us list the different positions (in the context of computer proof and demonstrative reasoning) as follows:

P1 (a) Our reliance on a computer's reliability and our reliance on our brains' reliability are the same; so, our reliance on a computer's reliability does *not* force the warrant for relying on a computer to be empirical, just as our reliance on our brains' reliability does *not* force the warrant for relying on our brains to be empirical.

(b) Our reliance on a computer's reliability and our reliance on our brains' reliability are *different*, but this difference does *not* force the warrant for relying on a computer to be empirical.[6]

P2 Our reliance on a computer's reliability and our reliance on our brains' reliability are *different*, and the differences force the warrant for relying on a computer to be empirical.

P3 Our reliance on a computer's reliability and our reliance on our brains' reliability are the *same*, and so the warrant for relying on our brains, just as the warrant for relying on a computer, is empirical.

Tymoczko subscribes to a version of P2 and Detlefsen and Luker a version of P3. I think that something along the line of P1 is the correct view. The follow remarks adumbrate some considerations in support of this claim.

a.

The explication of a priority is a highly complex issue. There are more than a few conceptions of the notion, but almost invariably one can find in them a certain distinction between two roles – a *justificatory* and a *non-justificatory* role – that experience is held to play in knowledge and cognition. The justificatory role has to do with the *evidence, justification, warrant,* or *entitlement* of belief; the non-justificatory role with the *understanding or acquisition of concepts.*

A sharp distinction of these two roles for experience is the principal idea in what may be called the broad Fregean conception of apriority. On this conception, something can be known a priori if there is a way to come to know it such that the warrant underwriting that knowledge does not rely on sense experience or perceptual beliefs for

[6] The differences between P1 (a) and P1(b) are significant and interesting. I however will put them aside. See Burge (1998) for an elaborate defense for P1 (b).

its justificational force. To say something is known a priori in this sense is *compatible* with the agent's knowledge being dependent on experience in some way.[7]

It is easy to see why one may want to hold a distinction between these two roles of experience. Sense experience is needed for me to acquire the concepts of *triangle*, *leave*, and *side* before I can be said to believe that a triangle has three sides or that the leaf I am looking at is triangular in shape. But only in the case of the second belief experience is needed to provide me with justification sufficient to make my belief knowledge. For the belief that triangle has three sides, experience plays a role only in the acquisition of concepts, but not in my entitlement to that belief as knowledge. In general sense experience needed for the acquisition of a concept does not thereby become part of any warrant one may have that would make a belief expressed with the concept knowledge. Otherwise, the concept of 'knowing a priori' – knowing something independently of experience – would become vacuous, and it would be a trivial truth that all knowledge is a posteriori. So, I take it to be uncontroversial that knowing something a priori is compatible with experience playing a non-justificatory role in enabling the acquisition of concepts.

For our purposes the important question to ask is *whether a similar distinction can be drawn that gives a broader interpretation of a non-justificatory role for experience*. I believe that there is such a distinction and drawing it will prove crucial for a proper evaluation of the purported empirical character of the 4CT and similar issues. On this interpretation, experience (in the context of cognition and knowledge) has a non-justificatory role to play which includes but is *not* limited to the acquisition of concepts. A non-justificatory role for experience in this broader sense I shall call an 'enabling role'.[8]

An enabling role for experience in this broader sense is pivotal in Tyler Burge's arguments for the possibility of a priori knowledge obtained through testimony. On the standard account, in a paradigm case of testimony, I infer from an empirically

---

[7] As Tyler Burge points out, on many important respects this conception is indebted to Leibniz. Leibniz explicitly indicated that one might depend psychologically on sense experience in order to come to know any truth, and so according to him, 'a truth is a priori if the justificational force involved in the knowledge's justification is independent of experience' (Burge 2000: 13).

[8] The term is due to Williamson (2007). As used by him, it has only the narrow sense of 'enabling one to grasp or to acquire a concept'.

warranted belief that some reliably source asserted *p* to the conclusion that *p* is warranted. The warrant enjoyed by my belief that *p* rests on the empirical warrant enjoyed by the premises. Burge thinks differently. He argues that coming to knowing something on the basis of testimony *need* not render that knowledge a posteriori. On his account, unless there are grounds for doubt, accepting the word of others is *rational*, and so rational acceptance of an interlocutor's words is an *a priori default position* (Burge 1993; 1998). He calls the *non-empirical* principle behind this idea the Acceptance Principle:

> A person is a priori entitled to accept a proposition that is presented as true and that is intelligible to him, unless there are stronger reasons not to do so, because it is prima facie preserved (received) from a rational source, or resource for reason, reliance on rational sources – or resources for reason – is, other things equal, necessary to the function of reason. (Burge 1993: 469)

It is thus possible for an agent to obtain in an a priori way knowledge by being told a proposition by another person if that person possesses a priori warrant for the proposition. It is not difficult to see that Burge's view relies on a distinction between (i) the case in which sense experience such as hearing the word of others plays only a causal role in *enabling* me to come to accept a belief and, more important, to *access* the a priori warrant possessed by my source and (ii) the case in which sense experience provides the justificational forces for my belief. In those cases where I know a proposition a priori by testimony, sense experience plays only an enabling role. That experience is not part of the a priori warrant that my rational source has for the content of the belief, neither is it part of the warrant which I received from that source and which underwrites my belief.

b.

We have seen that on the broad Fregean conception,

> (K) Knowing (something) a priori is incompatible with a justificatory role for experience but is compatible with an enabling role for experience.

We have also seen that (K) is plausible when the enabling role is limited to the acquisition of concepts. But things are by means as neat as one would want them to be when that role is construed more broadly. 'How broad should or can one construe the enabling role?' is a highly complex and important question, and cannot be answered without re-examining a number of positions of fundamental importance in epistemology. (As Field has argued, one cannot begin to think about any explication of apriority without bring into question a set of background assumptions about epistemology.[9] Burge's account illustrates very well how one can have a very broad, excessively so according to some, construal of the enabling role of experience provided that one is prepared to bring into question a whole lot of widely held opinions about rationality, content preservation, and testimony. His Acceptance Principle is a case in point.) Without going into this difficult question, the remarks below will show (I hope) how this question may bear on the issues this paper concerns.

Let's look at the case of the appeal to memory. It is often the case that memory constitutes a part of the warrant of a remembered belief, as in the case in which I remember vividly that I was convinced by Professor A's demonstration of a certain logical statement *p* (call this episode of remembering M). Contrast this with the case in which I remember a certain logical statement *q* and utilize it to fill a gap in a proof for a certain conclusion *c* (call this episode M*). In the first case, as Borge Steffen puts it (Borge 2003: 109), my cognitive attention is not merely focused on *p* but also on the *attitude* of my earlier self towards the proposition. Here M makes a substantive contribution to the justification of a remembered belief. In the second case, my memory serves not to supply propositions about particular mental events. Rather its role is to supply for the derivation a certain step, namely, *q*, that is a part of the demonstration that entitles me to believe *c*. This conclusion *c* is underwritten by the demonstration consisting of *q* together with other steps that *q* helps link together logically. The memory that supplies *q*, M*, is not what the demonstration is about, neither does it make any substantive contribution to the warrant provided by the demonstration. My belief that *c* is warranted because I have proved it. My warrant needs no further *justificational forces* to be supplied by M*. The role of M* is only enabling, not justificatory. It serves to give me *access* to that warrant, but not part of my entitlement to *c*.

This analysis helps us see why Chisholm is mistaken in thinking that if 'in the

---

[9] For his part, apriority is an evaluative notion explicable in terms of 'default reasonableness'. See Field (2000).

course of a demonstration, we must rely upon memory at various stages, thus using as *premises* contingent propositions about what we happen to remember,' then 'we cannot be said to have an a priori demonstration of the conclusion' (Chisholm 1989: 30). Chisholm seems to have conflated the justificatory and the enabling roles of memory. Typically, in a complex demonstration (a proof or a deduction) memory is called upon to play an enabling role. Contingent propositions about what we happen to remember do not *thereby* become part of the premises or make a substantive contribution to the warrant provided by these premises. Therefore, Chisholm is not entitled to his conclusion that the premises of such a demonstration do not provide an a priori demonstration of the conclusion.

Mental calculation relies solely on memory to provide the means of checking the correctness of a logical or mathematically sound algorithm or operation. Some considerations have just been provided to show why our warrant for relying on our memory need not be empirical. The use of paper and pencil extends our means of checking of longer deductions. I think precisely the same reasoning can be used to argue for the *merely* enabling role of sense experience and perceptual belief – or, more generally, that of brain processes, if one has in mind the Empirical-Reliability-of-the Brain-Argument – in first-hand calculations using paper and pencil. Now my suggestion is that in the context of a well conceived computer-assisted proof we see the appeal to computers as a yet further extension of the means of checking and executing a formally sound algorithm which serves mainly to enable our access to the a priori warrant for a mathematical result.[10] But one might say that there is a significant difference between a first-hand proof and a computer-checked proof such as the proof of the reducibility lemma. Whereas the justification provided by the former is *not* dependent on experience *essentially*,[11] the justification provided by the proof of the 4CT is dependent on experience essentially because it is too long for any human to check. I can agree with this if 'essentially' is used here just to mark out the complexity of the computer's proof. But the epistemic significance of 'essential dependence on experience' in this sense can be doubted. Given that it makes epistemic sense to see computers as a way to extend our means of checking proofs, I

[10] See Teller (1980) for a somewhat similar approach.

[11] As Arkoudas and Bringsjord put it, see Arkoudas and Bringsjord (2007: 189).

cannot see why the kind of dependence on experience involved in a warrant provided by a computer proof should be accorded special epistemic significance merely because of the complexity involved. One might say that the proof of the 4CT is not only more complex but unsurveyable. But I take it that to see the computer as an extension of memory, paper and pencil, and archives *qua* a means of checking is to see it *precisely* as a way to extend our capacity to *survey* proofs and deductions. If one accepts this account, one should be able to see that the 'essential dependence of experience' in the appeal to computers pertains (or more accurately *can* pertain, see the remark below) *only* to the enabling role played by experience, that is, to its role in enabling our access to the a priori warrant provide by the proof. If one accept this account, one should see why P1 is tenable and more plausible than the other two positions.

c.

A concluding remark. I have claimed that the appeal to computers does not render the justification provided by the proof of the 4CT a posteriori, nor does it render the mathematician's knowledge of the 4CT empirical. It is worth noting that I have not made a case against knowing a posteriori a mathematical truth or an a priori truth. I have used 'a priori' primarily as an epistemic qualifier that predicates of items of knowledge. In particular, I have not claimed that it is impossible to know an a priori truth on a posteriori grounds based on testimony. On the contrary, it seems to me undeniable that we *often* come to have justified beliefs of an a priori proposition on the basis of empirical considerations. (Note that standard discussions take an a priori proposition as a proposition *knowable*, not in fact known, independently from experience.). Take the example of a student of logic well-versed in the history of modern logic who has never got round to study the actual proof of Godel's Incompleteness Theorem. If he can be said to know the theorem, he came to know it on the basis of empirical evidence (by way of testimony). Although paradigm cases of knowledge obtained through testimony are empirical knowledge (of an a priori proposition or otherwise), I side with Burge (1993, 1994) in thinking that knowledge relying on testimony *can* be a priori knowledge, or knowledge underwritten by an a priori justification. The 4CT or archive-assisted proof is a case in point. Having said this, I think it is possible that a computer can be utilized in such a way that it serves to attest a certain mathematical result for an agent S – who have no understanding of the working of computer or the mathematical soundness of its algorithm but who have sufficient evidence for the belief that the computer has spoken plausibly as an attester to that kind of result – and thereby provides S's belief with sufficient but empirical entitlement that makes it knowledge. But for those who accept my argument, the case of the 4CT is not of this kind for mathematicians.

References

Arkoudas, Konstantine, and Bringsjord, Selmer. (2007). 'Computers, Justification, and Mathematical Knowledge'. *Minds and Machines,* 17, 185-202.

Borge, Steffen. (2003). 'The Word of Others.' *Journal of Applied Logic,* 1, 107-118.

Brown, James R. (1999). *Philosophy of Mathematics :An Introduction to the World of Proofs and Pictures,* Routledge, London; New York.

Burge, Tyler. (2000). 'Frege on Apriority.' *New Essays on the A Priori,* Oxford University Press, Oxford, 11-42.

– (1998). 'Computer Proof, A Priori Knowledge, and Other Minds (Volume 12: Language, Mind, and Ontology).' *Nous-Supplement: Philosophical Perspectives,* 12, 1-37.

– (1993). 'Content Preservation.' *The Philosophical Review,* .

Chisholm, Roderick M. (1989). *Theory of Knowledge,* 3rd Ed., Prentice Hall, Englewood Cliffs, N.J.

Coady, C. A. J. (1992). *Testimony,* Clarendon Press, Oxford.

Detlefsen, Michael, and Lucker, Mark. (1980). 'The Four-Color Theorem and Mathematical Proof.' *Journal of Philosophy,* 77(12), 803-820.

Field, Hartry. (2000). 'Apriority as an Evaluative Notion.' *New Essay on the A Priori,* Oxford University Press, Oxford, .

Teller, Paul. (1980). 'Computer Proof.' *Journal of Philosophy,* 77, 797-802.

Tymoczko, Thomas. (1979). 'The Four-Color Problem and its Philosophical Significance.' *Journal of Philosophy,* 76, 57-83.

Williams, B. A. O. (1972). 'Knowledge and Reasons.' *Problems in the Theory of Knowledgd,* The Hague, 1-11.

Wang, Hao. (1993). *Popular Lectures on Mathematical Logic,* Dover Publications, New York.

Timothy Williamson. (2007). 'Knowledge of Conditional'. Unpublished paper given in March 2007 at Philosophy Department, The Chinese University of Hong Kong.

..