THE PHILOSOPHICAL IMPLICATIONS OF THE FOUR-COLOR PROBLEM

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In a recent article, Thomas Tymoczko has suggested that the four-color theorem (4CT) [1] either is not a mathematical theorem at all or is an entirely new kind of theorem. He goes on to draw the conclusion that the theorem is not an a priori truth, in the classical mathematical tradition, but an a posteriori truth, and this conclusion forms the main thesis of his article.

Tymoczko is at pains to point out that he is not suggesting that the 4CT is untrue but that it is not "truth" as mathematicians have used the word heretofore and that its different nature springs from the fact that its proof relies on the use of a computer.

Although the issues raised by Tymoczko are undoubtedly important to the world of mathematics, it is difficult to see how he can justify his main thesis; and I will endeavor to show that it is more reasonable to continue to regard all mathematical truths as a priori—no matter how they are arrived at—although it may indeed be necessary to introduce a new mathematical entity intermediate between a conjecture and a theorem.

Tymoczko correctly points out that one of the main attractions of the four-color problem lies in the fact that it is "so simple to state that a child can understand it" (p. 57). Yet he goes on to assert that "the four-color problem is not a formal question" (p. 79).

In point of fact it is difficult to conceive of a more clearly formulated problem. It is every bit as well formulated as Fermat's last theorem or Goldbach's conjecture; and whatever the nature and status of the existing proofs of the 4CT may be, they can hardly serve to convert the problem from a formal question into an empirical one.

There is one unfortunate error of fact in Tymoczko's otherwise remarkably lucid thumbnail exposition of the existing method of proof. He says (p. 60) that in order to establish reducibility it must be proved that "every four-coloring of the ring around a given configuration can either be extended to the configuration, or modified first by one or more Kempe chain interchanges and then extended or modified by suitable identification of distinct vertices and then extended." The type of modification mentioned creates what is referred to in four-color terminology as a "reducer" for the original configuration.

Yet even in the case of classical hand reductions by Birkhoff [2], Winn [3], and others, reducers were never restricted to those obtained by identifying vertices and have always involved other constraints, such as requiring vertices to be adjacent. In point of fact any configuration that is smaller than the original configuration is a potential reducer, provided only that it has fewer vertices and/or edges and is not capable of creating a loop in the modified graph. And, without the use of a wide variety of different kinds of reducers, there is no hope of establishing the truth of the four-color conjecture within the framework of the existing methods of proof.

Most surprisingly, Tymoczko fails to recognize the fact that in so far as there is any weakness in the Haken/Appel proof of the 4CT it lies not so much in the reducibility testing—which is

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almost certainly correct and has been independently corroborated to a large extent—but in the discharging procedure, which gives rise to the unavoidable set of configurations.

It is by no means an easy matter to check that the final working list of 1482 unavoidable configurations embraces all the unavoidable configurations arising from their discharging procedure. Thus the Haken and Appel proof *is* subject to some degree of uncertainty, and Tymoczko is definitely incorrect when he says that no mathematician "has argued against it." At least one mathematician has openly stated that, in his opinion, they "failed to establish a crucial proposition" [4]. And those mathematicians who do not reject the proof in such a summary manner are not necessarily persuaded that the Haken/Appel discharging procedure is indubitably without flaws.

It should perhaps be pointed out that there are now two proofs of the 4CT: the original one by Haken, Appel, and Koch and the more recent one by F. Allaire [5]. Allaire's proof also involves a discharging/reducibility approach but only requires some 50 hours of computer time. It is, moreover, based on an entirely different discharging procedure and a completely independently developed reducibility testing program. At the very least Allaire's proof must rank as an independent corroboration of the truth of the four-color conjecture, and there can be little doubt that even if the Haken/Appel proof is flawed the theorem is nevertheless true.

Tymoczko chooses to compare the use of computers in mathematical theorem proving on earth to the use by Martians of a mathematical genius called Simon (pp. 71–72), who justifies his lemmas and even some theorems (particularly those of a combinatorial nature) by appeals to personal verification, moral rectitude, or political stature. He further compares the use of computers to the practice of some of Simon's followers of using the phrase "Simon says" for establishing the truth of difficult theorems—without actually obtaining Simon's seal of approval.

Even a cursory examination shows that this is a most unconvincing analogy. No one claims that computer results are correct because computers are morally upright or politically sound or enjoy great status, and mathematicians are certainly not in the habit of reporting computer-assisted results that they have not actually run on a computer (quite apart from anything else, someone else may carry out the necessary computer investigation and establish the erroneous nature of their claims).

The very reason those of us who have worked on reducibility testing are happy about Haken, Appel, and Koch's reducibility results is that they have to a large extent been independently checked by the use of different programs on different computers.

One of the problems is that Tymoczko fails to do the reader the service of supplying a coherent definition of the term "a priori truth," which is commonly taken to mean a truth that perforce possesses universal and necessary validity or a truth whose validity is independent of the impressions of our senses. Tymoczko appears to favor the latter definition (p. 77) but gives no reason for his rejection of the former, although he alludes to it in several places (e.g., p. 78 and p. 80); nor does he appear to realize that the latter definition is not really viable and, at the very least, is extremely nonutilitarian in the absence of some qualification.

These two definitions are not a priori (I use the term in an informal sense) the same; and, as I shall endeavor to show, it is really only the first that is a viable utilitarian definition, although the latter can be modified so as to be compatible with it.

It is certainly immediately obvious that there are many a priori truths that can be verified by recourse to our senses. The example cited by Tymoczko himself (p. 59) is a case in point. The truth concerned is the assertion that the sum of the first one hundred strictly positive integers is 5050. Using Gauss's procedure we can verify this truth by multiplying 101 by 50 in our heads. However, we can also verify it by guiding our pencils across a piece of paper—using our sense of touch—and literally writing down the first 100 integers; and then—by the use of our sense of sight and a few extra pencil marks—we can actually carry out the addition of the numbers in question, once again obtaining the result 5050.

In this case the use of actual pencil and paper and the use of sight to verify the truth in question is not essential, since it can be verified using the Gaussian approach in our heads [6];

but, as I shall show, there are many mathematical truths that cannot be verified "in our heads" and can only be accessed by recourse to our physical senses—to the actual carrying out of a type of experiment.

At this stage it is convenient to consider the four-color theorem in the context of other well-established graph-theoretic theorems that do not make use of computers and which Tymoczko would therefore regard as a priori truths [7].

1. The Four-Color Theorem in Context. The proof of many graph-theoretic theorems (as well as theorems from other branches of mathematics) falls naturally into three parts.

(i) Establishing the fact that the theorem is true provided a certain finite set of graphs, configurations, or—in general—cases possess (or do not possess, as the case may be) a stated property.

(ii) Obtaining an exhaustive listing of these cases.

(iii) Confirming that all the members of this set do possess the required property.

The finite set of cases concerned may, at one extreme, be so small and so simple that the case testing can be done in our heads, or it may, at the other extreme, be so large and/or so complicated that it is impossible to carry out without the help of a computer.

I shall use as an example a theorem due to O. Ore and J. Stemple [8] concerning the four-color conjecture, which may be stated as follows:

Every triangulated planar graph which has less than 40 *vertices is* 4-*colorable.*

Ore and Stemple obtained their result by reducibility testing coupled with a type of discharging that differs both from that used by Haken and Appel and from that used by Allaire. All of Ore's reducibility results were obtained by hand, and the discharging section of the proof involved the testing of 42 cases to ensure that they all discharged to an appropriate extent.

The figures and calculations involved covered a total of 118 pages and were too bulky to be published in a journal. They were thus written out in incredibly neat longhand and lodged in the Mathematics Department's library at Yale University (Haken, Appel, and Koch ran into a similar problem as regards the listing of their unavoidable configurations and had to resort to the use of microfiche). The referees no doubt checked the set of 42 cases carefully, but I do not think that many other people did so and I believe I can count myself as one of the few people who actually went to the trouble of obtaining a photocopy and checking it thoroughly by hand.

The point I wish to make is that carrying out the case testing necessary to establish the truth of Ore's theorem was, and is, an extremely time-consuming and tedious affair and there is literally no possibility of carrying out the case testing in our heads. Yet no one, to my knowledge, has ever suggested that Ore's theorem is not an a priori truth.

In practice, theorems (or potential theorems) involving case testing may fall into four categories:

(i) Those theorems in which the case testing can be done in our heads.

(ii) Those theorems in which the case testing is impossible to carry out without the help of pencil and paper.

(iii) Those theorems in which the testing can be carried out with immense effort by means of pencil and paper—requiring, say, several thousand man hours of effort.

(iv) Those theorems which are entirely beyond the reach of hand calculation and for which the case testing *has* to be carried out by computer [9].

The divisions between these categories are not clear-cut and they tend rather to shade into each other. Moreover, a given theorem has no necessary permanent location in one particular category, and the method we use for its proof is not entirely forced upon us by its category.

Many theorems in category (i) will not, in practice, be handled in our heads, since, except in the simplest cases, a resort to pencil and paper is more convenient and reliable. Although theorems in category (iii) can be proved by hand, recourse to a computer would often (though

not always) be a natural route to follow. And even in the case of theorems in category (ii) it may well be more appropriate to resort to a computer when there are many elaborate cases to check —once again for the sake of convenience and reliability.

As of now Ore's theorem is in category (ii) and the 4CT itself is in category (iv). But no one has proved that this is a permanent categorization, for these two theorems and the development of more powerful mathematical techniques may, in the course of time, change the existing categorization of theorems out of all recognition. Who can say what might not happen in a hundred or even fifty years' time?

It thus seems rather farfetched to classify theorems as a priori or a posteriori on the basis of their present categorization. Are we to assume that the 4CT is, as of now, an a posteriori theorem but that it may in time become an a priori one? This is surely a most odd use of the term a priori.

Tymoczko concedes (p. 71) that a simple proof of the 4CT cannot be ruled out entirely but contends that it can nevertheless be conveniently regarded as a paradigm for theorems whose proofs must perforce be permanently computer assisted. But even this class of theorems, if it exists, does not drive "a wedge" (p. 75) between mathematical theorems in this class and the remaining theorems in mathematics—neither in terms of reliability nor in a deeper philosophical sense.

The primality of some primes can be verified by hand—by sieving or other techniques—whereas the primality of certain large primes can only be verified by doing the necessary checks on a computer. Are we to assume that the former are a priori primes and the latter a posteriori ones?

Tymoczko seems to have an almost naïve faith in the reliability of human beings as opposed to computers and makes this very rash statement:

> The reliability of the 4CT, however, is not of the same degree as that guaranteed by traditional proofs, for this reliability rests on the assessment of a complex set of empirical factors.

This statement is clearly indefensible, as can be readily seen by considering the theorems in category (iii). If people choose to prove such theorems using traditional methods of pencil and paper, Tymoczko would be forced, on his own cognizances, to regard such theorems as being a priori truths in the traditional mold and, in terms of the above statement, of greater reliability than proofs of the same theorems in which the case testing is done on a computer.

In point of fact the complete reverse is the case, and for such theorems computer-assisted proofs would often be more reliable than traditional proofs.

Human beings get tired, and their attention wanders, and they are all too prone to slips of various kinds; a hand-checked proof may justifiably be said to involve a "complex set of empirical factors." Computers do not get tired and almost never introduce errors into a valid implementation of a logically impeccable algorithm.

I would go so far as to say that any lack of reliability of the present proofs of the 4CT resides less in the use of a computer for the reducibility testing and more in the fact that a computer was not used to create the unavoidable set of configurations arising from the discharging procedure. I will justify this statement in somewhat more detail in the next section, but before doing so it is appropriate, at this stage, to explore the meaning of the term "a priori" in some detail.

The term a priori as applied to any truth may mean:

(i) a truth that possesses universal and necessary validity; that is to say, a truth that is true in all possible worlds, *or*

(ii) a truth whose validity can be established without recourse to sense experience of the physical world.

Tymoczko concedes (p. 78) that, when using the first definition, the 4CT is no different from any other theorem and bases his contention that the 4CT is not an a priori truth on the second definition (p. 77). We can only assume that he regards the first definition as invalid (or as having

reference to some other kind of truth), and in focusing attention on the second definition he appears to confuse the nature of truth and the manner in which we come to know its truth.

He says (p. 60) that "mathematical truths are known a priori," which is tantamount to saying that mathematical truths are known to be true before we have proved them to be true. This might be a happy state to be in, but it is very far from the realities of mathematical research.

If we accept the fact that mathematical theorems *are* a priori truths, then in terms of definition (i) they are certainly true *prior* to and *independently of* the uncovering of an actual proof by an actual human being—but it says nothing about the manner in which we uncover their truth. If, in order to uncover their truth, it is necessary to use pencil and paper or a calculator or even a computer, this can make no difference to the nature of their truth.

What then are we to make of definition (ii)? Surely the only reasonable course of action is to qualify it in some way or other so as to bring it into accord with definition (i). This could be done, for example, by changing it to read:

(ii)′ a truth whose validity can in principle be established without recourse to sense experience of the physical world.

This brings definition (ii) into line with definition (i) and implies that an a priori truth is one that can be uncovered by a sentient being, with a brain sufficiently large for the purpose, without recourse to any experiment. It does not imply that the particular type of sentient beings that happen to have arisen on earth, which we choose to call Homo sapiens, may not need to resort to an experiment to *know* the truth of a specific a priori truth **[10]**. Such experiments do not convert a priori truths into a posteriori ones, and I will consider the true distinction between a priori and a posteriori truths in somewhat more detail in the final section.

If we do not take definition (i) as a valid definition of a priori and stick to definition (ii) in its unmodified form, then any philosophical problems raised by the 4CT arose a long time ago when men took to writing symbols in the sand. Computers are really just a highly sophisticated and highly efficient form of automated pencil and paper, and if there is any philosophical distinction of significance between the various categories of mathematical theorems it surely lies between category (i) and all the rest.

Those theorems in category (i) are indubitably a priori truths (whatever definition we use), since a particular type of sentient being actually has verified (or can verify) them by pure unaided reason. The rest do not enjoy the privilege of such purely abstract verification, and in terms of Tymoczko's concept of a priori they are all a posteriori.

Tymoczko (pp. 79–80) spends some time discussing Haken and Appel's heuristic argument, which led them to believe that it was probably possible to prove the 4CT by investigating configurations with a ring size no greater then 14, and he suggests (p. 80) that:

> This probability cannot be accounted for in ontological terms according to which any statement is true or false in all possible worlds.

A much clearer example of a probability statement concerns the search for large primes on a computer. In some cases, while it has not yet proved possible to establish the primality of a given large number, it is possible using Monte Carlo methods to assess the probability that the number in question is a prime. Such probability assignments are well founded and unambiguous, but they hardly raise ontological questions of much moment.

A statement to the effect that a particular integer has a particular probability of being prime does not purport to be an ontological statement, and it is thus not necessary to account for it in ontological terms. The only ontological reality is the a priori primality or nonprimality of the integer in question.

Mathematicians who work in this area distinguish such Monte Carlo methods from what they call "rigorous algorithms for primality testing" **[11]**. And although they may use such numbers for practical purposes (e.g., in cryptography) as if they were primes, their primality remains an openly unproven conjecture.

We should not let the word *probability* beguile us into drawing false analogies between

probabilistic statements in the physical sciences and probabilistic statements concerning as yet opaque truths in mathematics. When physicists assess the probability of an electron's presence in a particular location at a particular time, they mean either that the electron spends an appropriate fraction of its time in the location in question or that an electron has no specific location in space. They are in fact caught in the problem of wave/particle duality and the uncertainty that this brings.

When mathematicians say that a particular number has probability x of being prime, they do not mean that it spends a fraction x of its time as a prime and a fraction $1-x$ of its time being factorizable. Nor do they mean that it is some new kind of number that is neither prime nor factorizable (or both prime and factorizable). All they are doing is assessing the likelihood of its primality being worth pursuing or worth counting on (for cryptography or some other purpose).

There is therefore no need even to consider modifying "the concept of proof" (p. 80) to include probabilistic arguments, since they neither prove nor purport to prove anything and are merely potentially useful in directing our efforts into appropriate channels. Haken and Appel's proof did not depend on their probability arguments, which merely encouraged them to pursue the matter to completion, and they have no bearing on the nature of the truth that they uncovered.

Tymoczko recognizes this point (p. 80) when he suggests that probabilistic methods might with profit be added to our repertoire of tools for tackling theorem proving, since they can serve to avoid wasted effort or encourage perseverance in the case of difficult theorems. The only thing that can be added is that this is their sole role and their sole import as far as theorem proving is concerned.

2. Algorithms and the Four-Color Theorem. Tymoczko singles out three characteristics of mathematical proofs (p. 59), namely:

(a) proofs are convincing,
(b) proofs are surveyable,
(c) proofs are formalizable,

and in addition to contending that the 4CT is an a posteriori truth rather than an a priori one he contends that although it is formalizable it is not surveyable.

This view seems to rest on an unnecessarily narrow conception of what surveyability involves and an insufficient appreciation of the manner in which the formalization and surveying of a proof interact with each other.

There is one point on which there can be little doubt and that is that the four-color theorem (as opposed to the four-color problem) has not as yet been adequately formalized, and I intend to outline the manner in which this might be done and to show that once this has been done it will become much more readily surveyable. In order to appreciate the lack of formalization in the present proofs and the extent to which they can, as of now, be surveyed, it is first of all necessary to consider the subject of algorithms and their implementation on computers.

Just as many proofs in graph theory involve case testing, so also many proofs are concerned with establishing the efficacy of algorithms. As an example of a graph-theoretic algorithm we may take the so-called "greedy" algorithm, which finds a spanning tree of maximum weight in a graph on n vertices in which each edge has been assigned a specific weight (we may assume for the purpose of the present discussion that each edge has an integer weight or that the set of weights has been scaled so as to make them integer). The greedy algorithm proceeds as follows:

1. Choose an edge of maximum possible weight that
   (a) has not yet been chosen
   (b) does not form a circuit with those edges that have already been chosen.
2. If the number of edges chosen is $n-1$ then stop, else return to step 1. □

The reason for the appellation "greedy" is self-evident, and it is not at all difficult to prove that

this patently naïve algorithm does achieve what it purports to achieve. Moreover, the algorithm is so simple to implement that it can easily be implemented by hand for small and medium-sized graphs. And each time we apply it we uncover a new mathematical truth, namely, that the weighted graph in question has the resulting spanning tree of maximum weight. Moreover, on his own cognizances, Tymoczko would have to accept it as an a priori truth.

But what if the graph is so large that we are forced to resort to a computer? We then need the assurance not merely that the algorithm is sound but that its computer implementation (usually in terms of some high-level computer language) is likewise sound. Tymoczko touches on the subject of evaluating computer programs (p.74) but does not capture the flavor of the current state of the art. The branch of computer science that deals with ensuring that the computer implementation of a particular algorithm in a particular high-level language achieves what it purports to achieve is now highly sophisticated and well understood.

Some of the more recent high-level languages, such as Pascal, that use top down parsing and avoid "go to" statements, practically force the programmer to implement algorithms in a logically impeccable manner.

It is true that computer programs do sometimes have "bugs," but so do attempts to prove theorems by means of pencil and paper. It is also true, as Tymoczko says, that the "flaws in programs may sometimes go unnoticed for a long time" (p.74). But so do flaws in proofs that have nothing to do with computers. The first published "proof" of the 4CT, by Kempe, *was* flawed, and it was not until 10 years later that Heawood uncovered the flaw.

The point that Tymoczko seems to miss entirely is that flaws in the computer implementation of algorithms are nothing other than *errors of logic*, no different in essence from errors that crop up in proofs that have nothing to do with computers.

So if the greedy algorithm is implemented on a computer by means of a program in one or another high-level language, which conforms to the requirements of a valid implementation, then any spanning tree of maximum weight found by means of such a computer-implemented algorithm would surely also be a new a priori truth. The fact that it was found by computer has no effect on its nature whatsoever.

It is true that in some cases algorithms with logically sound implementations that are themselves logically sound do not always produce the results they should when run on a computer. This is because computers are finite and we are thus forced either to truncate or to round off irrational numbers or numbers with a recurring binary representation. For example, many algorithms for inverting matrices do not work very well on ill-conditioned matrices, and much of computer-oriented numerical analysis is concerned with overcoming such problems.

But such an unhappy situation does not apply to the greedy algorithm (for a graph with integer weights) or to the algorithm used for reducibility testing, both of which are restricted to operations involving integers or Boolean truth values.

What then can we say about the determination of reducibility on a computer? The algorithm itself is so simple [**12**] that its correctness is not at issue, and any doubts must reside in its computer implementation. Unfortunately, although the algorithm itself is very simple, its computer inplementation is anything but trivial; and the number of logical checks required to confirm the reducibility of a 13 or 14 ring configuration is so large that the actual programs for the proofs were written in assembler language in order to make them as efficient as possible.

This raises problems as regards any formal check on the correctness of the programs, since the formal checks used at present apply to high-level language programs rather than assembler language programs. Nevertheless the reducibility results in the Haken, Appel, and Koch paper have been indirectly surveyed in a manner very little different from the hand surveying of other proofs involving a large amount of case testing—namely, by means of independently written computer programs run on different computers.

In fact, the case testing for the reducibility results in the 4CT has probably been more thoroughly surveyed than the case testing in some other theorems that are not dependent on the

computer. Moreover, as pointed out above, it is rather the discharging procedure, which has been carried out by hand, that has conceivably not been adequately surveyed.

But even in this regard the theorem has been indirectly surveyed by virtue of Frank Allaire's proof, which uses a different discharging procedure and nevertheless arrives at the same conclusion. When hand-checking a proof, referees sometimes adopt precisely this procedure. They check a particular facet of the proof by means of a closely related but not necessarily identical argument.

So, all in all, it is fair to say that although the surveyability of the 4CT leaves much to be desired it has been fairly effectively surveyed and is no worse off in this regard than theorems concerning other equally intractable problems.

It is not surprising that the initial proofs of important and difficult theorems such as the 4CT sometimes appear in a manner that is anything but formalized, and the interaction between formalization and surveyability may well proceed through several stages before such proofs become truly satisfactory ones. It seldom happens, moreover, that mathematicians insist on formalizing a proof to the extent of literally presenting it in terms of, say, Zermelo-Fraenkel set theory. It is usually regarded as adequate to formalize it to a sufficient extent to satisfy the cognoscenti that it could be so formalized if one wished to do so. And there is surely no reason for the 4CT to be formalized to a greater extent than any other theorem.

It is, however, perfectly legitimate to contend that the proof has not as yet been adequately formalized, if for no other reason than that this makes it very difficult to survey it properly. But contrary to Tymoczko's view, this difficulty has very little to do with the reliance of the proofs on the use of a computer; and it is at least arguable that it arises partly because inadequate use has been made of the computer.

There are, moreover, certain steps that could be taken in the direction of formalizing the proof that would make it fairly readily surveyable, and it is appropriate to give some indication of what these steps might be—namely:

(1) The discharging procedure and the resulting creation of the unavoidable set of configurations could be computerized. (This might involve a judicious choice between various possible discharging schemes.)

(2) The progams for both the discharging and the reducibility testing could be written in a high-level language and adequately checked to ensure that they do implement the algorithms they purport to implement.

At this stage there would be no great difficulty attached to surveying the proof. And if surveyors so wished they could even rerun at least some of the computer programs on an entirely different computer to assure themselves that their surveys were sufficiently thorough. As pointed out above, this has already been done to quite a large extent as far as the reducibility testing is concerned.

It is perhaps appropriate to begin to draw this section to a close with some discussion of the obvious first requirement of mathematical proofs—namely, that they should be convincing. At this juncture in history the 4CT has not been properly integrated into graph theory as a whole and stands to some extent as a monument on its own, but there is little doubt that this is not its permanent lot.

Indeed, it already has strong connections with at least some branches of graph theory that have no direct reliance whatsoever on computer programs. Several mathematicians, such as Walter Stromquist, Frank Bernhart, and Frank Allaire, who did research on the question of reducibility also developed a coherent theory of irreducibility that is in complete agreement with the reducibility results that have been obtained thus far on the computer. Moreover, in the light of such irreducibility theory, it became possible to determine anti-configurations for all planar configurations that are not freely reducible. And Frank Allaire was able to make excellent use of such anti-configurations in finding reducers for intractable reducible configurations. Such developments can surely only serve to strengthen the confidence that mathematicians have in the truth of the 4CT.

And in the years to come, when the theorem is even more inextricably intertwined with graph theory as a whole, it will seem not a little quaint to even suggest that it is not an a priori theorem with a surveyable proof. The four-color conjecture served as an excellent stimulus to graph-theoretic research in general and the 4CT may continue to exert a benign influence on graph theory until such time as it has been brought into "the body of the kirk."

Having said all this there may well be a justifiable nagging doubt in the mind of the reader that an issue of some importance has been evaded by not facing up to it directly. Even though there are convincing reasons for regarding all mathematical theorems as a priori truths and even though there is very little reason for regarding the use of computers for theorem proving as the driving of a wedge between one kind of theorem and another, we are still left with lengthy proofs (whether achieved by hand or on a computer) that have been neither adequately formalized nor adequately surveyed and are suggestive rather than definitive—simply because mathematicians have only a finite amount of time and energy at their disposal.

Such "proofs" do indeed have every appearance of being mathematical experiments of less than even the bare minimum of reliability, and since the number of such "proofs" is likely to grow it would do mathematicians no harm to start thinking about them in advance.

Mathematicians already work with lemmas, propositions, theorems, corollaries, and conjectures; perhaps they need a new kind of entity that lies somewhere between a theorem and a conjecture. Perhaps these additional entities could be called agnograms, meaning thereby theoremlike statements that we have verified as best we can but whose truth is not known with the kind of assurance we attach to theorems and about which we must thus remain, to some extent, agnostic.

Such agnograms would have a status somewhat different from that of conjectures but their elevation to the status of theorems would have to await the arrival of a more adequately formalized and surveyable proof. I do not myself feel that the 4CT falls into this category, but it might well be regarded as a forerunner of the many genuine agnograms to come.

In addition to invoking the concept of an agnogram we may also need to wean ourselves away from the natural inclination to think that in order to qualify as a theorem an agnogram must not only be made surveyable but be made surveyable by a single mathematician on his own and be literally surveyed by at least two or three mathematicians working completely independently. Computer scientists have found that when writing lengthy programs it is essential to break them down into self-contained modules, each of which can be checked on its own. If the interconnections between the modules are likewise checked, this serves to ensure that the program as a whole is logically sound.

And the factors that come into play in a lengthy program are much the same as the factors that come into play in lengthy proofs of mathematical theorems. If such lengthy proofs are broken down into modules and each module is surveyed on its own and the interconnection between the modules is likewise surveyed in detail, there is no reason that the proofs should not be regarded as proofs of actual mathematical theorems. (Mathematicians, of course, already do this to some extent by using lemmas and corollaries.) And, if the surveying of the proof has to be carried out by a team of mathematicians each examining a different module rather than by a single individual, this should not, of itself, be regarded as invalidating the proof.

3. A Priori and a Posteriori Truths. It has been suggested above that:

> An a priori truth is one with universal and necessary validity that can in principle be established without recourse to sense experience of the physical world.

We have, however, not as yet defined what we mean by an a posteriori truth. Traditionally, a posteriori truths have been regarded as truths that can only be known by means of experiments. But for our purpose this is a little too strong.

We have already argued that many a priori truths are known by means of a form of experiment and may, in at least some cases, be inaccessible to us human beings in any other

way. In just the same way it seems pointless to insist that all a posteriori truths must be the result of actual experiments. As a de facto phenomenon we certainly do come to know many truths about the physical world without carrying out actual experiments, and we make excellent use of such knowledge for practical purposes. A more appropriate definition would thus seem to be:

> An a posteriori truth is one whose truth is contingent upon the nature of the universe to which it applies and cannot in principle be known without carrying out at least some experiments.

There is a significant difference in the use of the phrase "in principle" in these two definitions. In the first it means "in principle though not in practice." But in the second it implies both "of necessity" and "by assumption," the assumption being that more than one kind of world is possible and that we must therefore of necessity carry out experiments to uncover the truth about the nature of our particular world.

What then *is* the difference between a priori truths and a posteriori ones if the touchstone of an a posteriori truth is an actual experiment and, at the same time, some a priori truths are inaccessible without an experiment? The difference lies surely in their antecedents. The basic a priori truths can be arrived at without recourse to experiments since they are truths that must appertain in all possible worlds, whereas the basic a posteriori truths can only be known by recourse to experiments since they are not necessarily true for all possible worlds. We may note, moreover, that the experiments involved in the two cases are very different in nature. The experiments we carry out to try to uncover a priori truths involve the manipulation of numbers and logical symbols. But the experiments used to uncover a posteriori truths involve the measurement of length, pressure, volume, mass, temperature, time, and many other physical quantities.

It should thus be clear that agnograms are neither a priori truths nor a posteriori truths, but conjectures to which we can attach a high degree of credence. And even if the 4CT is regarded as an agnogram it cannot be so classified simply because its proof depends on the use of a computer, and it certainly cannot be regarded as an a posteriori truth. Physicists do not accept into their ensemble of a posteriori truths the results of inadequately verified experiments; if mathematicians likewise feel unable to accept into their ensemble of a priori truths the results of inadequately surveyable proofs, it would be somewhat invidious to call such results a posteriori truths.

Making a distinction between a priori and a posteriori truths does, of course, raise many philosophical problems, and not everyone would agree that the distinction is a watertight one. Eddington's epistemological theory was based on the contention that the physical world as we know it is in a very deep sense the only possible world that can exist and attempted in his "Fundamental Theory" [13] to determine the basic characteristics of the universe from first principles.

His work has never appealed to a very wide audience and may well be hopelessly flawed in numerous respects. But the philosophy behind it may ultimately be more appropriate than any other alternative. If it ever became possible to determine a dimensionless quantity, such as the electron/proton mass ratio de novo, in a manner acceptable to physicists as a whole, this would certainly make the distinction between a priori and a posteriori truths very difficult to maintain.

And if the four-color problem has inadvertently caused philosophers to reflect anew on these issues, then it has served a purpose in a wider field than we graph theoreticians ever dared to hope.

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Notes and References

1. The momentous first proof of the four-color theorem by Haken, Appel, and Koch was announced in the *Bulletin of the American Mathematical Society*, 82 (5) (September 1976), was presented by Haken to a packed

audience at a meeting of the American Mathematical Society in Toronto in September 1976, and is published under the title "Every Planar Map is Four-Colorable" in the *Illinois Journal of Mathematics*, 21 (84) (September 1977) 429–567. Tymoczko's article "The Four-Color Problem and Its Philosophical Significance," appeared in the *Journal of Philosophy*, 76 (2) (February 1979) 57–83.

2. "The Reducibility of Maps," *American Journal of Mathematics*, 35 (1913) 114–128.

3. "On Certain Reductions in the Four-Color Problem," *Journal of Mathematics and Physics*, 16 (1938) 159–171.

4. G. Spencer-Brown, *The Laws of Form*, E. P. Dutton, New York, 1979, p.xii. Unfortunately Spencer-Brown does not say what the proposition is, so it is impossible to assess the validity of this contention.

5. "Another Proof of the Four-Color Theorem—Part I," *Proceedings of the Seventh Manitoba Conference on Numerical Mathematics and Computing*, 1977, pp.3–72. Part II of this paper was also presented at the conference but has not yet been published.

6. By the time we are able to do such problems in our heads we have, of course, made extensive use of our senses in learning a wide range of a priori truths.

7. Not all mathematicians would, of course, accept all the current graph-theoretic proofs as valid, and I speak merely in terms of the general consensus among mathematicians. In particular, the intuitionist approach of L. E. J. Brouwer and the constructionist approach of E. Bishop rule out many forms of proof that are widely used. It would, however, take us outside the scope of this article to consider these approaches, which are not really germane to Tymoczko's arguments anyway. For a recent discussion, see A. Calder, "Constructive Mathematics," *Scientific American*, 141 (4) (October 1979) 146–171.

8. "Numerical Calculations on the Four-Color Problem," *J. Combinatorial Theory*, 8 (1970) 65–78.

9. Haken himself has suggested, in "An Attempt to Understand the Four-Color Problem," *Journal of Graph Theory*, 1 (1977) 193–206, that in the light of the 4CT proof this category may well contain a significantly large class of theorems and that it is at least worth investigating whether or not other well-known intractable conjectures can also be solved by computer. There may be, of course, some a priori truths that are inaccessible to any form of proof, but these are not even potential theorems.

10. The reference to a hypothetical being with an adequately large brain may perhaps be regarded as philosophically suspect, but there is another way of looking at the problem.

The nature of the interface between a human being and the external world is a somewhat hazy one. We might argue that the surface of our skin delineates this interface—but what about the air in our lungs and the food in our digestive tract, and what if we are wearing clothes, as is normally the case. Do they alter the interface or not? They certainly make us *look* very different to an outside observer.

We might alternatively argue that it is the collection of living cells from which we are made that distinguishes us from the world outside—but what about our hair (which is dead) or the water and enzymes in our body (which are as essential to our existence as the living cells themselves), and what about any cancer or pre-cancer cells that may be present, or the bacteria in our digestive tract?

It seems at least a permissible point of view to argue instead that the interface between us and the external world is ever changing and somewhat indefinite. In terms of this view, carpenters' hitting nails into pieces of wood do indeed extend beyond their skin, are something more than just their living cells, and are, at any rate as an operational concept, people-with-hammers.

So, also, in the case of a scientist looking through a microscope at some bacteria, it is reasonable to regard the interface between the scientist and the external world as lying between the microscope and the specimen and not between the scientist's eye and the microscope. Such people are, in fact, people-with-microscopes. Likewise, a good musician playing a violin is often "at one" with the instrument and is very much a person-with-a-violin.

Such examples can be multiplied indefinitely, and it seems clear that by means of our tools and instruments we can extend the interface between ourselves and the outside world out into the world, both in terms of what our senses experience and in terms of our ability to alter the world around us.

And, since human beings in the act of checking a long mathematical proof by hand are, for the most part, acting as rather inefficient computers, the resort to a computer simply converts them into people-with-(more efficient) computers, which can certainly act as stand-ins for sentient beings with an adequately large brain. It may even be possible one day to wear a micro-computer directly on our heads.

The reason that the 4CT was not proved before the advent of computers may thus be simply because there were no sentient beings with adequately large brains around before that time. To maintain that a computer-assisted proof of a mathematical theorem does not lead to an a priori truth is thus suspiciously close to maintaining that bacteria do not exist because scientists can only see them with the help of a microscope.

11. See, for example, H. C. Williams, "Primality Testing on a Computer," *Ars Combinatoria*, 5 (June 1978) 127–186.

12. F. Allaire and E. R. Swart, "A Systematic Approach to the Determination of Reducible Configurations in the Four-Color Conjecture," *J. of Combinatorial Theory Ser. B*, 25 (3) (December 1978) 339–361.

13. *Fundamental Theory*, Cambridge University Press, 1949. See also A. S. Eddington, *The Philosophy of Science*, Cambridge University Press, 1939.