Mathematics and Values:
Can Philosophy Guide Projects?

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Abstract

The philosophy of mathematics has provided insight on questions of foundations and mathematical truth; however, it has not been very fruitful in guiding the practice of mathematics. This paper attempts to find points of contact between a Christian worldview and the choice of mathematical projects and methods. Three areas are considered: (i) dubitability in current research, (ii) the intrinsic value of contemporary mathematics to contemporary society, and (iii) the affirmation of human value in the use of mathematics. Finally, a framework for valuing mathematics is proposed as an encouragement to think more deeply about how a Christian might choose a mathematical topic.

1. Introduction

This paper began as an attempt to give an account for my research in the mathematical sciences. As I read in the area of philosophy of mathematics, I was struck by how little influence it seems to have on the actual practice of mathematics. This literature is very helpful in understanding the changing role of postulates, culminating in non-Euclidean geometry and the dethroning of rationalism, or the turn-of-the-century search for logical foundations and how it was overtaken by Godel and by postmodernism in general. But it is not very helpful in guiding or justifying what most of us do in mathematics.

It is particularly distressing that Christian perspectives on the philosophy of mathematics do not offer much guidance. In Christian thought one particularly expects normative guidelines. Certainly, some general motivations have been given: that all truth is God's truth, studying mathematics can glorify God by uncovering the truth he created, and that perhaps we can learn something of the nature of God by studying the nature of mathematics. But how do we decide what mathematics to do, or how to approach it? And have our motives kept up with the dramatic changes in mathematics and society? Are our motives consistent with our Christian worldview?

By considering three issues that arise in the practice of mathematics, I will attempt to demonstrate how answers to these questions can be shaped by overtly Christian values. The three areas are (i) dubitability in current research, (ii) the intrinsic value of contemporary mathematics to contemporary society, and (iii) the affirmation of human value in the use of mathematics. Before addressing them, I will try to lay my philosophical cards on the table by making some worldview-level assertions that relate to mathematics. Finally, I will suggest a framework for valuing specific projects. Hopefully this framework will be provocative. From within the world of mathematics, constructing a value framework may seem irrelevant, since there are well-codified institutions for evaluating mathematical work and a great part of one's education is spent learning to recognize what counts as good mathematics. But if the starting point is God's kingdom, such a framework may be much needed.

2. A Philosophical Starting Point

It seems to me that a Christian world view provides a strong basis for a philosophy of mathematics. It begins by recognizing God as the creator and the source of all truth. There is a very clear Christian answer to why mathematics works. God's creation is orderly; it possesses a uniformity of cause and effect[1] and is a consistent reflection of the unity of God's truth. The omniscience of God includes all mathematical knowledge that we might obtain.[2] Under this perspective, truth is absolute, unchanging, consistent, and unified. Contradiction, or even separate bodies of knowledge that cannot be unified, are disallowed by the unity of God's truth. Mathematical knowledge, with its richness and consistency, is made possible by the richness and consistency of God's nature and creation.

Many Christian writers take the ontological position that mathematical objects exist outside of our conception of them, either as a kind of Platonic form or in the mind of God. I would suggest that it is not necessary to take a position on the question of existence. Within the view of truth sketched above, the key questions about mathematical knowledge can be answered without deciding the ontology of what we use only semantically. The alternative to asserting that numbers exist, however, is not an uninterpreted formalism that denies the truth of mathematics but some other interpretation of mathematics.

The key point is that mathematics is a description of the orderly creation of God. Attempts to explain why the world can be described so successfully using mathematics will inevitably return to the question of God's nature and why he chose to create what he did. Poythress (1974) advances theological arguments that God's nature is quantitative. I prefer to accept the evidence in creation and our ability to understand it using science, for "Shall what is formed say to him who formed it, 'He did not make me'?" (Isaiah 29:16) and "Does the clay say to the potter, 'What are you making?'" (Isaiah 45:9) We can view the study of mathematics, then, as investigating God's truth, without fear that what we are seeking is illusory or contradictory.

The question remains of whether we are capable of understanding God's truth, in particular, will human mathematics live up to the qualities described above? His creation is not only orderly, but capable of being governed and improved by people. Our *image bearing* is the key to the success of mathematics. In applications, "The *a priori* capability of man's created nature really corresponds to the *a posteriori* of what is 'out there,' because man is in the image of the One who ordained what is 'out there'" (Poythress p. 185). Internally, "It is our image bearing which gives us the mental equipment to do mathematics"[3]. The limitations of our mathematical reasoning that provoked the crisis in foundations can be understood in terms of our finiteness and fallenness while still asserting the unity and constancy of God's truth, including his omniscient knowledge of mathematics. Mathematics is subject to error and revision. We are not perfect proof checkers, and sometimes fail to play by our own rules. We cut corners and fail to formalize, leading to an imprecise notion of proof, and our formal systems are sometimes inadequate in conforming to God's truth and his creation.

While our image bearing gives us the ability to do mathematics, it does not make our mathematical knowledge the one correct description of reality. I would suggest the following answer to how we know mathematics is true, based on Barker (1964). He proposes an analytic, non-literalistic interpretation of number theory. Some number theory is analytic: It is known to be true *a priori* because it is the result of correct counting, which is in no way empirical, but relies only on a correct understanding of number words. Counting is a fundamental mental capability. Basic logic is also inescapably true, and so is considered *a priori* as well. Barker also suggests that counting is not reducible to logic, and arithmetical laws should be viewed as statements about counting, not logic or sets. They are analytic in the sense that only a knowledge of language is needed in order for us to know that they are true; they do not require synthesis with another kind of knowledge. At any rate, some of mathematics is known to be true *a priori* because it follows from counting and logic. This part of mathematics is similar to constructivist mathematics, and its epistemological basis is similar to intuitionism, with counting taken as innate rather than numbers.

The rest of mathematics, such as irrational numbers and infinite sets, have the status of models or theories. As in the other sciences, they are not the only possible formulation. Just as several scientific theories may be useful in explaining empirical observations, several formulations or extensions of mathematics may be useful. This extension of mathematics need not be merely uninterpreted formalism, though parts of mathematics are certainly best described that way. It can have the status of a model: When combined with science it makes statements about the physical world, or it might make statements about the "counting" part of mathematics. These statements are not undubitable because they are part of a model and rely on its axioms. A good example to keep in mind is the Euclidean model of space. In summary, I am taking an *a priori* or intuitionist view of arithmetic and an empiricist view of extended mathematics.

3. Dubitability in Current Research

Contemporary philosophy of science, particularly the work of Kuhn and Popper, and its application to mathematics by Lakatos (1976), leads to a view of scientific knowledge as dubitable and subject to change. Instead of being the result of impartial empirical observation and reasoning, scientific theories are seen as influencing observation and themselves being influenced by a wide variety of societal factors and worldview presuppositions. Knowledge does not just progress steadily; theories are replaced when they become unwieldy and the community sheds one paradigm and adopts another. An illusion of steady progress is created by the desire for unified scientific explanations. Particularly in mathematics, there is the tendency to reinterpret old results to fit them into the new paradigm. Euclidean geometry is still "true" if we interpret the parallel postulate as an arbitrary assumption rather than a self-evident fact.

Experience and faith commitments lead me to embrace a limited dubitability. Like most mathematicians, I do not think that most mathematics, including the area I work in, will be affected by Godel's incompleteness or erased by future "revolutions". However, I expect mathematics to change and be adjusted. Propositions that are widely believed to be true, such as "P is not equal to NP" (there are no fast algorithms to a certain collection of hard combinatorial problems), may be false. Results involving subtle concepts of infinity may be reinterpreted to have different assumptions or proof methods. Applications will certainly change.

Dubitability seems germane to many practical methodological questions. Should one use the computer to conduct numerical tests and draw conclusions from them, do symbolic manipulation, or execute a long proof? Is it wiser to spend time checking results of others or to assume that they are correct and use them? Is it more effective to focus on a well-defined field and work out the implications of prior definitions and partial results or to try decidedly different definitions, problem or methods? How does one choose between the modeling approaches that can be used to describe a physical system and translate it into mathematics?

In my specialty of operations research, dubitability has increased because of the interweaving of computer experiments with proofs. The question of whether an algorithm converges may be answered by proof or by numerical experiments on a computer. Determining the speed of an algorithm on typical problems is almost always done by computer experiments, since the construction of algorithms outpaces the ability to analyze their rate of convergence. After an initial period where computer results were being reported in a variety of ways that were impossible to reproduce or verify, e.g., "it ran in five minutes on my computer", standards were instituted for the reporting of computer results. These standards help to define the particular problem that was solved and the amount of computation required, so that in principle the results could be verified. However, the standards are only being used in the small part of my field that focuses on finding faster algorithms for some very old, well-defined problems. Much of the "cutting edge" looks for algorithms for an expanding list of new problems, or uses the computer to gain other insights into a problem. In these areas there are essentially no standards being used for reporting computer work. How reliable is it? Anecdotal evidence suggests that computer-generated numerical results in this field are often inaccurate and occasionally just plain wrong.

The fundamental shift that seems to have occurred is that instead of verifying results by exact replication, the standard for verification has become an inexact one of similarity. For example, in a recent research paper I showed a computer-calculated optimal policy for a model of a manufacturing system. The policy consists of a two-dimensional lattice with each point labeled as producing nothing, producing type 1, or producing type 2. Another researcher at Stanford solved the same problem, using a slightly different algorithm, and obtained the same policy. I took this as verification of my computer work. In fact, there were many intermediate calculations that I know must have differed, because we used many different approximations. The fact that we obtained the same answer does not guarantee, or even strongly suggest, that we would obtain the same answer on other cases. The algorithms we used will never be checked in a detailed sense because they are not in themselves of interest. The research goals were to test other, approximate methods and to gain insights into what the policies look like. This bit of knowledge has now been added to the vast pool and will probably never be checked further. Is it correct? Well, the results are probably close. But if they are used in certain ways, say as counterexamples to "disprove" a conjecture, then the level of confidence in our mathematical results has taken a great blow. Of course, the failure to verify or duplicate numerical results also creates a much larger vulnerability to scientific fraud.

Dubitability has also increased with regard to analytical results. Davis and Hersh (1986) describe mathematical justifications as *rhetoric* because they are intended to convince a mathematician in that specialized field. In addition to the limitations of logic and proof that make a verification from first principles or logic impossible, there are pitfalls that have nothing to do with foundations. As specialization and the volume of publication increases, the fallibility of what is published also increases. In the mathematical sciences, writers and referees may have less background in mathematics and less commitment to a professional principle of accuracy. Results that are useful in the computer age may be much lengthier and messier than what was viewed as publishable in an earlier age. I have twice encountered results that had made their way into books that were incorrect in significant ways. In one case a proof neglected some cases and the theorem needed to be restricted. In another a lengthy algebraic result, not derived in the book or its references, contradicted a property stated in the book; believing the algebra and other researcher's examples that supposedly refuted the property threw me off track in my research. These examples do not threaten the core mathematical results that have been repeatedly scrutinized. No one thinks such core results will be refuted by checking the algebra. Rather, their effect is to change my levels of belief, or what I accept as true, in my work. To practice mathematics at all I must believe that it is possible to know mathematics; that my reasoning will not lead to contradictions and that I can separate truth from falsehood. Yet I have learned to hold results outside of this core (and much of what I know is outside of the core) at a slight distance. I do not have time to check everything that I use or to check my results in every conceivable way. When contradictions do arise, I am now more inclined to suspect published results that led to the contradiction. I do not have a very clear conception of what should be taken as core, but I would tend to describe it narrowly.

This view of mathematics, including a limited dubitability, seems to have implications for choosing research directions. First, it places a premium on making sure we understand something before we use it. When analyzing models, the focus would be on understanding simple models rather than doing computation with complex models that are potentially more accurate but not understood. Next, it justifies mathematical rigor in the applied areas where such rigor is often left behind, since this rigor may overturn what was considered obvious. Of course, such rigor is very costly and we must have faith in our ability to tell which stones to look under. Finally, I think we should hold the paradigms and research programs of our specialties as dispensable. For example, in my field there are three approaches to modeling a manufacturing system: discrete, continuous with discrete stochastics, and continuous with continuous stochastics. A researcher's choice of a model depends mostly on who their adviser was, not which is best in a particular situation. Pressures to become very adept at just one approach should be resisted. Like proof methods, we need to know a variety of modeling approaches to pick the one that works best in a given situation. On the other hand, questions such as whether to use a continuous or discrete model are often a matter of taste--either will do. In this case, knowledge of the different approaches can reduce duplication of effort. There really is no justification for redoing an earlier paper using an alternate modeling philosophy. More important, it seems to me, is to look beyond the assumptions of a research program. We should ask what other problems or models not being considered are worthy of our attention and carefully consider what makes a problem of interest.

4. Rethinking Intrinsic Value

One approach to justifying mathematical work, in the spirit of G. H. Hardy (1940), is to eschew usefulness as a motive and give justifications very similar to those used for art. Timeless-ness, beauty, and particularly truth are emphasized. I will call this approach intrinsic value. A second approach is to point out the many historical examples where "pure" mathematics was later put to unanticipated use, so that no mathematics can really be labelled pure or inapplicable. Two frequently cited examples are hyperbolic geometry in general relativity and finite groups in elementary particle theory. Essentially the same reasoning--that there may be unexpected applications--is used to justify basic science. I will argue that today's mathematics and today's social values require a rethinking of the intrinsic value argument, as well as a more nuanced view of potential applications.

Hardy's defense includes the assertion that mathematics is a relatively harmless discipline. In the isolationism and the rigid leisure class of 1930's England, such a negative justification was somewhat persuasive. But justifying mathematics today requires a much stronger argument. In the intervening half a century, society--and normative concepts of a good society--have shifted toward an economic perspective of interdependency and productivity. Society no longer accepts the notion that doctors should decide how much is spent on health care, that NASA should decide how much is spent on space, or that scientists should decide how much is spent on research. Symbolic of this change was the cancellation of the superconducting supercollider project, which clearly offered the potential for new knowledge. The productivity pressures being faced in other occupations are beginning to be felt in academia. In mathematics, teaching is being reformed and research funding is being scrutinized for tangible results such as educational impact or collaboration with industry. Corporate and foundation funding is generally even more focused on anticipated applications, rather than the intrinsic value of mathematics. These pressures do not simply reflect temporary budget difficulties. They also demonstrate the shift toward a society in which professions are subject to external scrutiny and the resulting pressures for productivity and societal value.

Certainly these pressures affect how funding is pursued. But should they influence how we value mathematics? In particular, how well do arguments for the intrinsic value of mathematics line up with a Christian worldview? Faculty at a Christian college, having acknowledged God's sovereignty in their life, are under an obligation to work for God's kingdom in their scholarship. Personally, I cannot accept the blanket statement that any research, no matter how arcane, can be kingdom work if it seeks to uncover more of the truth that God has created. The view that any research is worthwhile, even worshipful, seems to overlook the realities of human suffering, limited time, and limited resources. Thus, whatever value mathematics has must be balanced against other priorities. Mathematics is not done in a vacuum.

While expectations have risen in the last half century, the changing face of mathematics has also affected the argument for intrinsic value. The maturing of mathematics and its focus on computation have had significant affects. Keith Devlin (1997) addresses the issue of maturity in his article "The End of Mathematics", where he distinguishes between "enlarging our fundamental understanding of the universe" and "tidying up the loose ends". Large extensions, fueled by applying mathematics to new domains, cannot proceed indefinitely. Indeed, he writes that "The Golden Age of mathematical expansion may well be over." He also makes an argument of diminishing returns in mature sciences such as chemistry. In mathematics, with fragmented subdisciplines and a huge volume of publications, it becomes very difficult to argue that a particular piece of research contributes in any meaningful way to the pursuit of truth. Certainly the notion of diminishing returns applies to the current situation in mathematics.

The resurgence of computation in mathematics also seems at odds with valuing mathematics as the pursuit of fundamental truths. The computer has coupled mathematics much more closely with applications. Now, it is possible to ask fundamental questions about computational mathematics. In my specialty of operations research, a good example is whether or not there is a polynomial time algorithm for linear programming. However, most research in these areas is driven by the goal of computing, not the pursuit of fundamental truths. Even the fundamental questions in these areas might not qualify as "deep". Would a mathematician without a computer be interested in them?

Another aspect of intrinsic value is the unchanging, or timeless nature of mathematical facts. Mathematics has an immutability to it. Taking the philosophical position that mathematics discovers God's eternal, unified truth, mathematics can have profound value. When this reasoning is applied to current work, however, two problems appear. First, most mathematicians are working on what I described earlier as extended mathematics, not arithmetic. Its status, I argued, is more that of a model than *a priori* truth. It may be internally consistent and have the standing of a successful scientific theory, but cannot claim to be the unique description of a pre-existent truth. In this view, rather than saying God created infinite sets, one would say God created a world that can be understood through infinite sets. The more sophisticated the mathematical object, the more difficult it is to assert that it has an intrinsic, appointed place in the creation. A second problem is that these objects, and the questions being posed about them, often appear to be contingent on current developments in mathematics or applications. History is likely to judge much less of today's work as "timeless" than in previous eras when the community was smaller and the questions more basic. Of course, even the most obscure mathematics can be timelessly true. But more fundamental, general results are timeless in a stronger sense. As the work turns to more detailed or esoteric questions, some of the timelessness is lost.

In contrast, the aesthetic aspect of mathematics seems as strong today as ever. This may be due to the inexhaustibility of creativity. One might say there are more elegant proofs than important theorems. Beautiful results can be obtained in all branches of mathematics, even if the motivation was quite pragmatic. A concern might be the increased complexity, or "messiness", of the problems being solved today. Long proofs, and particularly computer-aided proofs, are less likely to command aesthetic respect. On the other hand, fractal images and computer visualization have brought great attention to the beauty of mathematics.

To summarize, intrinsic value cannot be used as a blanket justification for all mathematics being done today. It must be applied selectively and balanced against other priorities in God's kingdom. If we view these priorities as primarily *social*, we must make intrinsic value arguments that ultimately talk about benefit to society, though in the broadest sense, including artistic and other kinds of value. Hence, the ability to communicate the work beyond the specialists takes on added importance. One of the criteria that guides mathematical work is relevant here. Topics are more credible when they can be simply stated and recognized by non-specialists. Certain simply stated problems seem to be relevant because they are an unavoidable test of the machinery. Studying these problems helps explore fundamental patterns, building confidence in the mathematical system or uncovering new methods for other problems. This type of problem should not involve generalization or new definitions, but merely ask questions about objects that are considered important for other reasons.

5. Affirming Human Value

Many mathematician's view of their work seems quite devoid of moral values. They might have an uneasiness about working in the defense industry, a vague hope that their research will contribute to the progress of society, and a commitment to truth. When applications are considered, one might think that the field itself--medical, defense, etc.--has moral content but the mere act of applying mathematics to it is morally neutral. This common response is what Poythress calls the dogmatism of neutrality: Mathematics is objective and morally neutral, so its *application* does not raise any ethical questions that are not inherent in the area to which it is applied. In fact, there are a number of ways in which using mathematics has profound implications. One concern is dehumanization. For mathematicians, Davis and Hersh 1986 are surprisingly alarmist on this issue: "When a man steps up to a bakery counter and takes a number, he is sped on his way, and that is good. But when he takes a number, he becomes, in part, a number, and there's the rub."[4] Dehumanization is not new, and the notion of information systems and computer technology as dehumanizing and replacing human contact can be argued both ways. More relevant to a world view is the belief in measurement that accompanies social applications.

The use of quantitative methods to make social decisions, which grew rapidly in conjunction with the computer age, brings with it a unique set of attitudes and dangers. Davis and Hersh consider the example of standardized testing for college and career placement (p. 93). The very process of testing reveals a belief that the quality in question is quantifiable, measurable, and predictable. In an attempt to measure *something* the test creates a new artifact, test-taking ability, that affects the educational system, spawns its own industry, e.g., SAT preparation, and shapes our view of people. Just as a materialist view limits the world to that which can *in principle* be quantified and observed, social tools such as placement tests engender a view of the world that is limited to that which can *in practice* be measured or mathematized. "As we mathematize the world, we proceed to lose or to throw away those parts of the world that cannot be mathematized. What isn't mathematized seems not to exist, even never to have existed." (p. 98) The very act of collecting data about people conveys the sense that they can be quantified and measured. Applying statistical theory to them, such as recognizing that characteristics measured from a large group of people fit a normal or bell curve, suggests that people can be studied using the same methods as the physical sciences.

The methodology of the decision sciences points out a particular dilemma. In many government and business settings, mathematical models do a fairly good job of identifying the key choices, which become decision variables of the model, and constraints (logical, physical, resource limitations, etc.). To guide decisions, a criterion or objective is usually selected. The model is then used to find the choices that give the optimal value of the objective, such as the maximum profit. The appeal of the model and the mathematical nature of the process tend to give credibility to the results. However, the criterion contains all the difficulties of decision making. Different people involved are likely to have conflicting criteria. The modeler may end up choosing the criterion because no one else understands the model. In my field of manufacturing scheduling, for example, the criterion of minimizing the makespan (the time until the last of a set of jobs is completed) was once proposed solely because it was mathematically tractable. It has since led to a whole school of research with little or no relevance to manufacturing. Another difficulty is that optimizing a single criterion may not reflect the way that options are ranked by the people involved. For example, a manager may not want optimal profit but merely profitability, and have other criteria for ranking among profitable options. Mathematicians who do optimization seem almost professionally trained to ignore these issues. But values simply cannot be avoided when mathematics is put to prescriptive, decision-making use.

Along with all scientists, mathematicians face the danger of naturalism, thinking that only the natural world exists because we spend our lives studying it, and scientism, thinking that scientific knowledge is the only kind of knowledge. The rare and specialized skills of a mathematician make it easy to be more aloof from the affairs of the world than an honest response to God's command to love your neighbor allows. A mathematician may feel that he or she is not quite as good as other people at doing things outside their profession. On the other hand, outsiders are dramatically less adept at doing mathematics. If the mathematician wants to excel, then he or she has a strong motive to stick to mathematics. The academic culture encourages aloofness through specialization, the solo nature of most research, and the limited rewards for interdisciplinary work. Even the current efforts at curriculum reform focus on how to teach mathematical problem solving, not how to relate mathematics to other topics in engineering, biology, or business.

All of these areas point to the need for a broadly contextual framework for thinking about mathematics. For the Christian, such a context is even more essential because truth is holistic and God-given. This holistic view of truth and an intimate relationship with God is the grounding from which a person's life and work can emanate. Fleshing out such a ground or framework is largely an individual matter of understanding one's vocation. Several principles may be helpful in shaping a more individual response. The Christian recognizes that neither the objective level of truth nor the subjective level of relationship can be fulfilled without the redeeming work of Jesus Christ. Christ provides not the key to doing mathematics but the key to understanding its relevance.

At the objective level, knowing about God's truth, his creation, and his love, we can understand in a deeper way what we are doing when we study mathematics. In this framework mathematics can be studied without falling into a false perception of autonomy. Mathematics conforms to God's truth and assists in understanding reality. Recognizing the transcendency of God's truth and his creation relative to our mathematical knowledge prevents naturalism from creeping into our mathematized world view. Acknowledging transcendency also fosters a humility regarding mathematical ways of knowing that reengages us with other disciplines and aspects of life.

At the subjective level, knowing our role in Christ's redemptive work puts a career in mathematics in a relational context. Mathematics is relevant not only because it conforms to truth, but as a human activity that can affirm human value. We can pursue excellence and give glory to God, treat people with dignity and compassion, and include in our teaching our understanding of the nature of mathematical knowledge. Finally, when we solve problems or do research, we can seek to let the welfare of others influence our choice of topics. How a Christian worldview might guide our choice of topics is considered in the final section.

6. Choosing Mathematical Topics

In this section I propose some criteria, motivated by a distinctly Christian worldview, that could be used to choose among mathematical topics in research or the curriculum. I do not claim that they are complete or balanced. However, I suggest that applying an evaluative framework derived from a worldview, not a set of criteria internal to mathematics, is a worthwhile exercise.

Dissemination. This is a very common expectation; however, we might be inclined to disregard its moral value precisely because it is an expectation placed on us by our deans. Other than training our minds, our work influences the world through other people. Telling them about it is just as important as doing it. In addition to communicating clearly, we should recognize that advocating for a good idea is an important job, remembering that models and paradigms compete. Balanced against this imperative is the recognition that most of the deluge of published material will not be used in any significant way or read by many. Hence, we should seek the form of dissemination appropriate for our work, which may be the classroom, a departmental seminar, a presented paper, a conversation with a colleague, or a real application.

Next are four criteria, at least one of which might be expected of our work.

(1) Understanding the nature of mathematics. Some topics, of course, are good training for doing more mathematics. Others point out the nature of mathematics and its limitations, such as dependency on axioms or formal incompleteness. Cultivating this understanding is particularly important to developing a Christian world view that sees mathematical and scientific study as an activity done in relation to God that conforms to his truth. History, foundations, and philosophy of mathematics would be pertinent to this objective.

(2) Simply stated problems. As noted before, certain simply stated problems seem to be relevant because they explore fundamental relationships in the mathematical system.

(3) Actual application. By this I mean not “applied mathematics” but the use of mathematics to gain knowledge or produce something in another field. Such work is inherently interdisciplinary. The value of work under this criterion is determined by the end that it serves.

(4) Potential application. Much mathematical work, including applied mathematics, is justified by

asserting that it may be used in the future. Most of it never is. At the risk of sounding like a budget cutter, I suggest that we often could know this beforehand, or at least know which work is less likely to be used. For applied mathematics, where the intended area of application is known, we might ask: What sequence of further work would conceivably lead to its implementation? Who proposed the model being studied and how close are they to the application? For pure mathematics we might ask: Is the field unified? Are constructive results and computation imaginable, or only existence results? Does anyone outside of the field know about it, and if not, why?

These criteria can also provide a context for discussion of curriculum revision. For example, finding the maximum of a function using the method that we teach in calculus may only meet criterion (1); it reinforces the relationship between a function's graph and its derivatives, but is not used in applications to find a maximum unless the function has parameters or lots of variables, which we don't consider. In contrast, finding the zero of a function by Newton's method may meet criterion (1), (2), and (4); (1) because it introduces iterative techniques which are widely used in applied mathematics, (2) because it is a realistic solution method to a basic mathematical problem, and (4) because our students may use it doing scientific research or other work.

I have argued that a philosophic framework of value--and in particular a Christian worldview--can guide the choice of projects, contribute to some methodological issues related to dubitability, and help affirm human value in a highly quantified society. I have asserted that some, but not all, mathematical work can be seen as valuable to the kingdom of God. The challenge, then, is to develop a more conscious scheme for valuing mathematical work, based not upon what is interesting to the mathematician but what is relevant to our world.

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Notes

1. This assertion is made by James Sire, *The Universe Next Door* (Intervarsity Press, 1976) p. 26.

2. I am avoiding the question of whether mathematics and logic are *contingent*, as described in Arthur Holmes, Wanted: Christian Perspectives in the Philosophy of Mathematics, in *Proceedings of a First Conference on Christian Perspectives in the Foundations of Mathematics,* Brabenec, R. L., ed. (Wheaton College, 1977) pp. 39-48. Poythress argues that they are part of God's nature, not his creation, so that he did not choose mathematics from among many possibilities. In contrast, *voluntary nominalism* holds that God had sovereign choice in creation, including mathematics.

3. Charles Hampton, Epistemology to Ontology, in *Proceedings of a First Conference on a Christian Perspective in the Foundations of Mathematics,* R. Brabenec, ed.(Wheaton College, 1977) p. 86.

4. Philip J. Davis and Rueben Hersh, *Descartes' Dream* (San Diego: Harcourt Brace Jovanovich, 1986), p. 85.