We are all perhaps painfully aware of the controversies concerning evolution, creation, and intelligent design that are now being played out not only in our high schools and universities but even in our courts. Why do scientists in general not accept such branches of science as intelligent design and creation science that are often supported by Christians, but only by a few others? Are Christians just somehow making a mistake and these really should be considered out of bounds for science? Or are these investigations really sound science in some sense, but most scientists are bringing in a prejudice through the back door to reject these alternative proposals? Shouldn’t we all be able to agree about what is scientific and what is not?

Why is it so difficult to find a resolution to the ongoing debates? The simple answer would be to say that worldviews are at work behind the scenes (and they are), and neither side is willing to give ground to the other. While this may be true, it is a rather superficial assessment of the situation, if only for the reason that most parties in the discussion purport to be doing “good science,” simply by making “objective” observations and bringing them to the attention of others. Yet nevertheless they are not able to convince each other. There is also the fact that Christians are divided about the issues; there is not one “Christian” view and one “non-Christian” view, but a panoply of different views with none that is obviously correct.

These issues turn out to raise rather complicated questions to address, questions which are tightly intertwined with implicit modernist assumptions about science and the present post-modern shift that is underway. Directly applicable is the twentieth century attempt to solve a broad philosophical problem concerning science: is it possible to clearly define science so that we all agree what is “in” and what is “out”? In other words, can we limit what we mean by the “science part” of the activity, so that we can ignore the “religion part” and therefore isolate just the part we agree upon to be called science? Are there clear reasons why scientists should not accept intelligent design or creation science in principle as part of what counts as science? As it turns out, in the words of Larry Laudan, “it is probably fair to say that there is no demarcation
line between science and non-science, or between science and pseudo-science, which would win assent from a majority of philosophers.”¹ This is known as the *demarcation problem*.

If Laudan is right, and there is no clear way to draw a line of demarcation between science and non-science, this immediately raises other questions. How is it that science continues to march on, offering us success after success, if we can’t even define it? How do we keep science on track? How do we keep out the crackpots? And in the present-day climate, another obvious question is, what should be taught in schools? After discussing the demarcation problem, I will address these issues indirectly by suggesting an attitudinal change, by adopting an approach which I call *Mere Science*.

**The Demarcation Problem**

As Larry Laudan relates it in a 1983 paper, “The Demise of the Demarcation Problem,” the demarcation problem goes all the way back to the Greeks. In particular, Aristotle seems to have offered two criteria for what science is, that which involves certainty of principles (“know how”) and also that these principles follow from a comprehension of first principles or causes (“know why”).² This latter criterion is to set science off from crafts, where only the knowledge of how to carry out the tasks was needed. However, according to Laudan, in the seventeenth century, at the rise of modern science, this criterion was dropped as being problematic. Indeed, early scientists such as Galileo and Newton did much of their science without regard to relating their system of scientific beliefs to first principles. For example, while Newton admitted in his *Principia* that he would like to understand why gravitation was the way it was, “he was emphatic that, even without a knowledge of the causes of gravity, one can engage in a sophisticated and scientific account of the gravitational behavior of the heavenly bodies.”³ So although certainty was still important, the necessity to “know why” was dropped as a criterion for science.

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² Laudan, 339.

³ Laudan, 340.
By the nineteenth century the demand for certainty was also removed, in favor of an epistemology that was fallibilistic. In other words, most thinkers by the mid-nineteenth century had accepted that all scientific theories were tentative, and that revision and refinement was to be expected. The implication of this view “is that there is no difference between knowledge and opinion: within a fallibilist framework, scientific belief turns out to be just a species of genus opinion” as Laudan puts it.⁴ This brought about a shift of focus from infallible truth to seeking to define science in terms of method, a focus which remained throughout the better part of the twentieth century. The hope then was to show that all that is to be called by the name of science can be said to share the same set of methods, and also that these methods would have credibility in establishing knowledge. However, in the latter half of the nineteenth century it became quite evident that there was no such shared methodology. Indeed, many of the proposed methods suffered from such ambiguity that it would be next to impossible to tell when they are actually followed, and in 1908 Pierre Duhem showed quite convincingly that what was purported as the “scientific method” by philosophers bore little relation to what working scientists actually did.⁵

In the twentieth century, the focus turned toward logical criteria for method. For example, the logical positivists focused on the idea that scientific statements are those which can exhaustively be verified. However, this is too strong a condition, because scientists could never do all the potential experiments that would test a particular statement, and there is no reason in principle to limit the verification procedure to a subset of such experiments. In response, Karl Popper suggested that a better criterion for judging whether a statement is scientific would be that it be falsifiable, because if verification takes too much evidence, falsification can be done in principle with one negative experiment. However, if this were the only criterion, lots of of crackpot theories would have to be scientific, just because they have been falsified. On this point, Laudan gives us a whole list of potential candidates, including flat earthers, the-world-is-about-to-enders, magicians, astrologers, and water diviners.⁶ Laudan’s main criticism here is that such logical criteria do not refer to important things we all know intuitively should be associated with sci-

⁴ Laudan, 340.
⁵ Laudan, 341.
⁶ Laudan, 346.
ence, such as that a statement has evidential support or is worthy of belief. After considering several potential directions one could go beyond those above, Laudan reaches the conclusion that, "the evident epistemic heterogeneity of the activities and beliefs customarily regarded as scientific should alert us to the probable futility of seeking an epistemic version of a demarcation criterion."\(^7\) Indeed, he says, that the demarcation problem is actually spurious, because it assumes a set of epistemic invariants concerning science when in fact there are not likely to be any. His conclusion, although not striking, is informative. It is important, he says, to be able to distinguish between reliable and unreliable knowledge, but there is no reason to make this distinction turn on the notion of "scientific."\(^8\)

Laudan also considers the important question of why we would want a distinction between science and non-science in the first place. One reason traces itself back to the early association of science with knowledge in general, in the sense that the very term derives from the Latin word *scientia* which means "knowledge." So perhaps we could view it as a misguided attempt to pin down criteria or a method by which we would be able to have certain knowledge as opposed to when we only have "belief." But in the present day, there is more at stake. Coming out of the modern period, the term "science" has emerged with cultural weightiness; for we all know that saying something is "unscientific" is usually taken to be derogatory. Thus if something can be put forward as a "scientific" investigation, it is a lot more likely to get funding, or be convincing in an argument, for example, than something that is not. One could even consider the postmodern argument that considerable power is at stake, for example with regard to funding, and with regard to which worldview controls what can be taught in schools. So it is easy to understand that there are many reasons for all sorts of people to want to have a demarcation, even if none is forthcoming.

These are not the only problems with demarcation. Del Ratzsch, in his book *Science and Its Limits*, tells a story that parallels that given by Laudan but with a slightly different focus, that of the "humanness" of science.\(^9\) To summarize, the goal coming out of Enlightenment thought is

\(^7\) Laudan, 348.

\(^8\) Laudan, 349.

to pin down a way to get science to be automatic; to just to “turn the crank” so to speak, and out comes reliable knowledge. A reasonable place to start would be, in the spirit of the Enlightenment tradition, to try to limit what is admissible to only those things which are self evident, such as the rules of mathematics and logic, and to empirical data that is obviously true or in other words, evident to the senses.10 To begin an assessment, the first question we ask of this system is, where do theories come from? Experiments could only produce sets of data, and making deductions using the rules of logic alone cannot produce a theory out of a set of data. It's obvious that a creative leap is involved. For example, consider Newton's gravitational law, that the force between two bodies is proportional to each mass, and inversely proportional to the square of the distance between them. How could Newton arrive at that statement merely from deductions in logic and the data such as knowing where the planets were relative to the sun? He couldn't. Not only did he need to use some clever insights from previous scientists such as Kepler, he also presumably needed some trial and error, an ability to look beyond the data to see a general pattern, and then the insight of how to express that pattern in mathematics, which all involves creativity. Creativity is not a part of the strict attempt to define science as being limited to empirical data and careful inductive or deductive reasoning. Where does the creativity come from? In fact, it can come from many sources, including a person’s religious, or anti-religious, convictions.

OK, so we can't rule out creativity in forming our theories, but perhaps we could let that slide. Suppose we just allow creativity in, but we limit the science by applying our strict criteria to the testing of the theory, no matter where it comes from. This is the verification step Laudan discusses. But it goes wrong also. If we start with a theory, we can certainly make a prediction, devise an experiment to test that prediction, and go test it. But so far we have only tested one prediction. So we do this again. And again. And again. So far so good. The problem is that any reasonably complex theory will allow for an arbitrarily large number of predictions and we can't test them all. For example, suppose we have tested twenty predictions of our theory and they all passed. But how do we know that the twenty-first prediction will not fail the test. (The same goes for the three hundred and first prediction after the theory passes three hundred tests.) And furthermore, how do we know that there is not some other reasonable theory that would

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10 The rules set out here are in the spirit of logical positivism, although that philosophy in a formal sense is much more complex and precise. See, e.g. *The Structure of Scientific Theories*, Fred Suppe, ed.
give the same prediction for the first twenty cases but a wildly different prediction for the twenty first case? We cannot. This latter fact says that theories are always underdetermined. That means that given a finite set of data, in principle there could be many theories that would fit the data, so in order to choose, we would have to invoke some other criterion than merely a good fit to a finite amount of data. One example of such an auxiliary criterion is that of simplicity.\(^{11}\) Often scientists prefer a simpler theory over a more complex one if they are equally good in all other respects. But simpler theories are not always more correct theories, and anyway, this simplicity assumption is also outside the criteria we have set for limiting our science.

Perhaps we should pause to say that real science doesn't work this way. Usually if we can predict quite a number of things with a theory, we will think it is pretty good. In practice, we don't use such strict criteria as assumed above, and we often use judgment calls. Judgment calls are of course not self-evident or from experience, so they are outside our trial definition of science. In other words, a judgment call can be affected by or even determined by a worldview. But that is a side point for now. Let's continue on to see if we can do better with our definition. Now that we have to give up the possibility of unambiguously verifying a theory, perhaps we can at least take the attitude that successful theories are presumed tentatively true until they are falsified.\(^{12}\) After all, in principle it should be a lot easier to falsify a theory. You don't have to verify that countless predictions actually work; all you have to do is find a data point enough out of agreement with the theory to be convincing and you realize that the theory cannot be true. This would also imply that we can limit the theories we call scientific to only those which are falsifiable, or in other words, theories that can be tested so that data could possibly contradict them.

This proposal also runs into a number of pitfalls. Perhaps the easiest to see is that all but the simplest of experiments depend on some sort of equipment that typically depends in turn on theories about science other than the one that is being tested. For example, if you are testing a prediction in the theory of gravity by doing a precision measurement of a falling object using a sophisticated laser apparatus attached to a computer, your experiment obviously depends on a

\(^{11}\) The criterion of simplicity is known as Ockham’s razor after William of Ockham (c. 1287-1347).

\(^{12}\) This is Popper’s criterion as described previously.
sophisticated use of the principles of electricity among other things. Although the theories on which the equipment depends may be considered as tentatively true because they have not yet been falsified, if the experiment fails, how do we know which theory is at fault? By consequence, if the experiment does fail, we cannot strictly speaking say that the law of gravity was violated, but we can only assign some level of likelihood to that possibility. Of course the latter is another judgment call. Another problem is to decide how far out of agreement a data point has to be before we are convinced the theory is invalid. In practice, if there is no other theory to replace the one being tested, such anomalous data can simply be ignored in expectation that an error will be found in the experiment. In other words, data that does not fit the predictions of a theory are not always convincing, depending on what else is involved. Here is another judgment call.

But as we have already said, real science is not done this way. Real scientists have complicated conceptual frameworks about science as derived from their own worldviews, and what actually goes on is much richer. Judgment calls with metaphysical content are really made all the time, to help affirm a particular theory and to rule out another. This fact alone is probably as problematic for the falsification criterion as anything. But real science also does make progress. Many theories are held by many scientists to be pretty much beyond doubt, because in their judgment the evidence just seems to be so good. Considerations like these led Thomas Kuhn to consider the problem in a completely different light. Rather than try to define science, he decided to look at scientists. Thus he embarked on a historical study to see what scientists actually do, and how progress is made. He was particularly interested in the times when drastic shifts were made in the history of science, such as the Copernican revolution or the breakthrough discovery of quantum theory. What he claims to have found was that scientists don't typically focus on verifying theories that are already assumed correct, or on falsifying them either, for the same reason. This led him to suppose that actually the scientists worked within what he called a

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13 We have drastically simplified the story leading up to Kuhn of course, omitting direct reference to the contributions of such important philosophers as Rudolf Carnap, Phillip Frank, W. V. O. Quine, Carl Hempel, Paul Feyera-hend, Imri Lakatos, and N. R. Hanson among others. For a more complete account, see Suppe, or one of the standard anthologies on this subject such as Philosophy of Science: The Central Issues, Curd, Martin and J.A. Cover, or The Philosophy of Science, Boyd, Richard, Philip Gasper, and J.D. Trout, eds.

paradigm of assumptions, which were their basic backdrop for doing science. The paradigm typically comes from some particularly dominant aspect of the science of the day, and is extended as a kind of framework within which to judge other science. For example, not so long after Newton made his monumental contributions, the general assessment was that his laws of motion and his gravitational law described the motion of objects in a mechanical way. Extending this idea to the whole universe gives us a mechanistic view.

According to Kuhn, scientists spend most of their activity trying to explain as much as they can within the paradigm. A lot of this has merely to do with articulating the paradigm, i.e. explaining how all the various phenomena fit within it. Checking phenomena for which definite predictions can be made within the paradigm is also important. Occasionally though, they run across something unexpected that does not appear to fit the paradigm, which Kuhn calls an anomaly. Sometimes they are able to incorporate an anomaly into the paradigm, perhaps with only a minor adjustment, or perhaps by realizing something that had been overlooked before. Otherwise they may simply ignore the anomaly as being unimportant as suggested above. But occasionally, according to Kuhn, they may not be able to ignore it or explain it. In these cases, says Kuhn, the anomaly may lead to a crisis state, which may ultimately lead to a scientific revolution. However, Kuhn points out that a paradigm in trouble does not lead to a scientific revolution unless there is a new paradigm available to replace it. If no such alternative view exists, scientists will just live with the anomaly, perhaps while continuing to bang away at trying to solve it, or they may simply leave it for future generations.

Thus for Kuhn, science is essentially to be understood sociologically, with every aspect of the social interaction of the scientific community to play a role. Some have read Kuhn as saying that science is only a sociological construct, a point of view that is used to good advantage within the postmodern world. This would be going too far. But for our purposes, we should note Kuhn's work as very important for emphasizing the richness of the scientific enterprise, and the essential role that social interaction plays as well as the role of a culture's underlying default philosophy or religion. All of these conspire together to portray a vibrant and rich scientific en-

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15 I follow Ratzsch here

16 In some sense, Kuhn spent the rest of his life backtracking from this extreme view.
deavor. However, the result is unambiguous. Science depends on worldviews. There is no strict demarcation between science and religion. The story of the philosophy of science does not stop here of course, but for our purposes, all the key ideas are on the table.

The main conclusion we draw from this section is that we should take seriously the fact that it is unlikely that we will ever find clear demarcation criteria for separating science from non-science.

**Methodological Naturalism**

One might wonder, if there are no necessary and sufficient conditions of demarcation between science and non-science, perhaps either necessary or sufficient conditions could be useful in deciding what is scientific. Laudan rightly argues that neither one is adequate because both leave too large a door open for obvious non-science to creep into the scientific arena. For example, if we have only necessary conditions, these would not tell us that something is definitely a science, and if we have only sufficient conditions, these do not tell us when something is not a science. It would hardly do to be left with only being able to affirm that physics “might be” a science, or that astrology “cannot be ruled out” as science. Just about every criterion one can come up with, such as well-testedness for example, would pertain to all sorts of things that are either science or non-science. For example, Maxwell’s equations are well tested as the equations related to electromagnetism and easily pass muster as science, but you could also have a well-tested method for teaching how to swing a golf club or a well-tested cake recipe, neither of which would typically be considered as science.

There is one often suggested criterion as a necessary condition for science that seems to stand out above others as being important to consider. That is the notion that the very nature of science demands that one should assume the methods of science to be naturalistic. This, it is said, is necessary because after all, science studies nature. However, aside from suffering from the problems Laudan has raised, the idea that we should limit our science to methodological naturalism (MN) raises problems of its own. First we have to ask, what could the term “methodological naturalism” actually mean? Most people might say something like this. It is in the
very nature of science that we study things that are in principle repeatable, these being the regularities in nature. Since we can only study scientifically those phenomena which exhibit regularity, we can safely assume that these can be modeled with natural laws, and we should approach them with this assumption. This attitude I will refer to as weak methodological naturalism (WMN). But this is really saying no more than that science is only good at studying regularities (including probabilities, catastrophes, and their leftover evidence, etc.) and this is really innocuous. It adds nothing to science, and if taken seriously, there are many things that are not addressed by the criterion, the most obvious being the historical sciences. For when it comes to cosmology or to evolutionary theory, we cannot repeat what happened, and we are always left with an inference. So we would have to conclude that on the assumption of WMN, these fields are simply not science.

Some, however, use the term in a much stronger way, so as to attempt to make it do the work of demarcation. This strong methodological naturalism (SMN) would say something like this. Because science is about natural phenomena, we must approach all problems of science with the assumption that the solution is naturalistic. The main problem with this assumption is that it is bringing metaphysics in the front door using a sledge hammer, rather than merely sneaking it in the back door. More importantly, it pays no attention to other properties we would like to have associated with science such as veracity. We should expect that there will always be a naturalist story, since humans are pretty creative, but for any particular set of phenomena, the naturalist story may simply be wrong. We should realize that the assumption of SMN is really a metaphysical commitment about the way things must be, rather than a scientific demarcation criterion about how science is, and it does not help any discussion about the way things really are.

In his paper, “Methodological Naturalism,”17 Alvin Plantinga provided some other important arguments on this topic, of which I will mention only two. The first is a discussion about Pierre Duhem’s attempt to keep science free from metaphysics essentially by defining it that way. The point Duhem makes is that if he includes metaphysical assumptions in his theories, they may not be accepted by others. So since it is important for the theories to be acceptable to others, Duhem tells us,

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“I have denied metaphysical doctrines the right to testify for or against any physical theory.... Whatever I have said of the method by which physics proceeds, or the nature and scope that we must attribute to the theories it constructs, does not in any way prejudice either the metaphysical doctrines or religious beliefs of anyone who accepts my words. The believer and the nonbeliever may both work in common accord for the progress of physical science such as I have tried to define it.”

Plantinga calls such science “Duhemian science.” According to Plantinga, this argument for methodological naturalism is rather attractive because it is simple and commonsense, and it would allow people with all sorts of metaphysical commitments to work together in a common enterprise. Therefore this “Duhemian science” is public science, common to all, only employing assumptions that are universally held. Plantinga proceeds to argue that the science that emerges is very limited, only including such things as can be verified incontrovertibly, and therefore universally agreed upon. While the science that we all agree on anyway, such as the physics or chemistry that we can readily repeat in the laboratory and included in Duhemian science, this would do nothing to shed light on any controversial science.

But is it worth it to pursue Duhemian science as a metaphysically neutral scientific core? As a practical matter, who would want to engage in such a limited practice? Is universal assent important enough to so limit science? Perhaps this attitude raises universal acceptance to the status of an idol. But more importantly, there is still a further problem with Duhemian science Plantinga has not addressed: even if we have the notion that we can in principle rule out all metaphysics or religion concerning our conclusions, in practice this would be impossible. Here comes into play the second point Plantinga makes that I wish to discuss, what he calls “Augustinian science.” This notion is taken from Augustine’s idea that “human history is dominated by a battle, a contest between the Civitas Dei [City of God] and the City of Man” and of our Christian obligation to discern appropriately the extent of the contest and its role in the academic community. In other words, there is an antithesis between the Kingdom of God and the

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18 Duhem, as quoted in Plantinga 220 (see note 61, Facets, 220).

19 Plantinga points out that this criterion does not strictly speaking rule out metaphysical or religious assumptions as long as they are commonly held.

20 Plantinga, 213.
Kingdom of darkness of which we are all a part. With this antithesis at work in each of our hearts, as Romans 7 describes it, even our best intentions could not bring about common agreement on what should constitute common science. Philosophically this is the demarcation problem working overtime; practically it would demand that all practicing scientists, and all interested lay parties, be as astute as the wisest of philosophers, and also be able to check their own subliminal motivations, possibly motivated by sin, at the door. This simply cannot be done.

Concluding that “there is little to be said for methodological naturalism” Plantinga suggests that a better way to proceed would be to go ahead and engage in “Augustinian science.” What he actually means by this is that we are all to bring everything we know and believe into what we do. This indeed could be a promising starting point for working together in science. Perhaps a metaphor might help illustrate the point. Consider science as a mighty river. Into the river feed many different tributaries, each representing science being done from a different worldview. The particular tributaries may look somewhat different as sciences, but when they get into the mighty riverbed, they all mingle and contribute to the cultural science, even though they don’t entirely lose their identity. However, there are other things that keep them on track in terms of science. Most powerfully, the constraints of creation, the way things really are, constrain the science outcome claims. Other factors also provide constraints, such as the common methods of working that are actually in place in the “guilds” of the various sciences as well as some common working assumptions. On down river however, we reach the delta of interpretations of the science that is being done in relative commonality. This delta has as many streams as there were tributaries, so disagreements will arise down there. The metaphor suggests a way of proceeding beyond the impasse of culture wars in science to a more fruitful dialog. This way of proceeding I call Mere Science.

Mere Science

If science is really a lively multi-faceted collective effort with many different insights from many different worldviews as well as many different personal convictions, and if the de-

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21 This metaphor is taken from Tim Morris and Don Petcher, *Science and Grace*, Wheaton, IL: Crossway, .
marcation problem is not solvable, then we should expect there to be controversy in science. So what can we do? One fruitful way of taking the demarcation problem seriously is to point out what we know up front, that science cannot be defined in any neutral manner, and that world-views will enter in. Rather than only focus on what might be agreed upon, why not also celebrate the diversity? Our goal then is to open up the controversy—to help those involved to understand more deeply what is really going on—so that communication is enhanced for us to agree to disagree when necessary, and to understand why. In other words, we should accept the diversity in science and learn from it.

OK, we all do Augustinian science. That implies an acknowledgment that we cannot possibly separate religious or metaphysical assumptions from our science entirely. At the same time, the demarcation implies that we shouldn’t worry so much about what is “in” and what is “out” in terms of science, and we should admit openly that faith/metaphysical convictions play a role in any one individual’s science. We should all be free to go about our business with everything we think we know, regardless of where it came from, and carry on with the scientific enterprise. Taking the insolubility of the demarcation seriously means that we accept that there will be things we do not hold in common. There is something tremendously freeing about this; we no longer need be driven in our science to resolve all differences as in Modernist science, but rather we can focus on how cultural science has made tremendous progress despite the differences. Let us therefore raise the question, what is actually in common among practicing scientists with different worldviews that has allowed science to have made so much progress and indeed to continue to make progress?

This focus on the commonness and the diversity brings us to the notion of Mere Science. By Mere Science I mean merely the enterprise of doing science in which everyone engages in his or her own brand of Augustinian science, while acknowledging the demarcation problem, the existence of underlying presuppositions, and those things held in common as well as those things that are not held in common. Thus Mere Science is not an attempt to define what science is. Rather it should be considered as the sociological enterprise of science accompanied by some

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22 Obviously some will be using the attempt to demarcate science as a power play in favor of those who speak for science. This should be identified and pointed out of course.
ground rules to facilitate maximum communication and progress. The ground rules indicate that we expect differences and ought to work hard to disclose them on as deep a level as needed. So we do not put forward a definition of science; focusing entirely on an essentialist definition can actually distort and impede the enterprise itself, fostering division and neglect of possibly valuable insights. Rather Mere Science is intended to be a prescription for aiding in the doing of a rich, full, “thick” science, encouraging and clarifying the whole enterprise.

Are there really things held in common in the diverse cultural activity of science? Yes of course. Otherwise scientists of all different philosophical and religious persuasions could not have actually worked together for many decades to bring science to where it is today. To get at the commonalities, let’s start by considering a fairly large number of scientists, in a room together, charged with the task of finding out what is agreed upon that makes the scientific enterprise work. They would not necessarily start by their agreement on conclusions of science, because in these they would quickly find controversy. They would of course find agreement on a broad array of conclusions though, including the scientific results about which there is little controversy and which can be easily repeated. These would include the large bulk of what constitute the sciences of physics and chemistry for example, probably identical with the things that would be considered Duhemian, which by definition are universally agreed upon. But they would presumably not agree on many things, particularly on those that are closer to their differing philosophical and religious commitments. (It is no good at this point saying that there would be agreement on the controversial issues, such as evolution or intelligent design, “if only everyone would be truly scientific” or something of that ilk. To say this is to deny the insolubility of the demarcation problem that we have already affirmed.) This being the case, they would have to look a little deeper to decide what makes the enterprise work on the whole, yet still result in controversy in part. In order to get a clearer picture, I would like to suggest that Mere Science entails at least three important things all those of us doing science have in common: 1) the common “constraints of creation,” 2) a common “confession of faith,” and 3) a common socio-cultural environment.

23 For example, some would believe that biological evolution had occurred and others would not. This must be considered in the “delta” of the river of science.
The first and most important thing that we all have in common is what I call the “constraints of creation.” By this I mean that, because of God’s covenant faithfulness in upholding His creation, when we probe creation, it responds back, and in a uniform way. This creational response is not relative to the particular views of the observers, but is orchestrated by God's own action in the world in sustaining the laws he has established. So it is not something we believe but something outside of us. Thus if two people perform a similar experiment, similar results will be found. No matter what our metaphysical assumptions are, we will both agree that jumping off a bench will result in falling to the ground. Or to take another example, if we take a working voltmeter and measure the voltage difference between two points of a circuit, we will all agree (assuming our eyesight is reliable, etc.) that the voltage is, say, 5 volts. This is what makes bench science possible and fairly universal. A lack of agreement on points such as this would usually signal to the scientist faulty equipment or an incompetency of some sort. The constraints of creation will guarantee that among competent scientists, all repeatable science will be rather uncontroversial.

The second thing that most practicing scientists have in common is what I will call a common “confession of faith.” This perhaps seems a strange analogy, but consider a church confession such as the Presbyterian Westminster Confession, the Baptist Philadelphia Confession, or the Belgic Confession of the Dutch Reformed Church. One way to think of such a confession is that it contains the assertions that the “elders” of the church have put together to summarize what the church believes, predicated on a long history of tradition. The “elders” of our science community can likewise easily uncover many things that we all accept as necessary for doing science. A partial list would include that we all believe in regularities in nature that we can uncover, that our senses provide a reliable mediation to us for representing this nature, and that our rules of mathematics and logic form reliable tools for deducing conclusions from particular start-

24 This terminology is obviously chosen to emphasize the Christian perspective on nature, that it is a creation by God. When speaking to unbelieving scientists, the term “constraints of nature” would just as easily serve.
ing points. We would also agree on such things as certain principles or methods of inquiry such as that certain issues of competency must be followed and that integrity is needed (we can't be dishonest). These sorts of things that allow us to sit together in a lab, or at a blackboard, and ponder the meaning of our data or calculations together no matter what our background metaphysical assumptions are, could certainly be agreed upon. However, we should note that it is not necessary that we actually make a list that we agree upon. Indeed if we would do so, there may be some disagreement on the fringes of what such a list should include. But the point is that such principles have already emerged out of the tradition of science and they are pretty uniformly assumed.

Finally, the third common element of Mere Science is a common socio-cultural environment. Although there are the constraints of creation, and we do have many common confessional commitments, we cannot deny the social role of our emergent science. We do not here mean that we all come from a common cultural background in general, such as the western culture say, but rather that all scientists operate within one international scientific culture that has emerged in our working together. Thus English, Indian, and Japanese scientists can all work in concert, bringing their different cultural insights and tendencies with them to enrich the enterprise, but all working together within the confines of the common confession to allow science to emerge. The importance of this social aspect must not be under-emphasized. While the constraints of creation may keep science largely on track without demarcation, it is the collective judgment of all involved that keeps out wild speculation, and ultimately brings about consensus. But we should also not forget the other side—that Mere Science is socially emergent sci-

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25 The idea that certain faith commitments are necessary for science is not new. An early mention of this is found in Abraham Kuyper's Lectures on Calvinism, wherein a list can be found that parallels the list given here: “Every science in a certain sense starts from faith, and, on the contrary, faith, which does not lead to science, is mistaken faith or superstition, but real, genuine faith it is not. Every science presupposes faith in self, in our self-consciousness; presupposes faith in the accurate working of our senses; presupposes faith in the correctness of the laws of thought; presupposes faith in something universal hidden behind the special phenomena; presupposes faith in life; and especially presupposes faith in the principles, from which we proceed; which signifies that all these indispensable axioms, needed in a productive scientific investigation, do not come to us by proof, but are established in our judgment by our inner conception and given with our self-consciousness.” Abraham Kuyper, Lectures on Calvinism, Grand Rapids, MI: Eerdmans, 131. Other more recent lists can be found in many places including Ratzsch.

26 These last couple points are motivated from a discussion in , Howard Van Till, and Davis Young, Science Held Hostage,
ence—there is no infallible judge standing by to tell us what is ultimately true and what is not. Rather, some things not accepted at one time may be accepted at a later time and vice versa. So in that regard, Mere Science can be viewed as an emergent science, consisting of the overlap of the conclusions of the many groups who practice their own versions of Augustinian science, but with a fuzzy boundary. Some science is more credible to the masses and some less so.

While we are considering the social aspects of science, we also cannot forget social politics. Within science, we may have people pushing agendas for reasons, such as political or theological, that do not clearly belong to the domain of scientific enterprise. In fact, without demarcation, it is inevitable that we will have a certain amount of social politics within science. So we just have to learn to live with it, while attempting to be honest in our own contributions when outside agendas may be playing a role.

Before going on, we should also ask the question, what is not common among people with different metaphysical or religious commitments. Here I remind the readers of two important things. First, in view of the demarcation problem, there is no common agreement on what counts for science. We just have to make our peace with the socially emergent fuzzy boundary of what is commonly accepted as science. Second, while I have said that there is a common set of beliefs important to science that I termed a common confession, there are no common underlying “religious beliefs” that lead to the common confession. This common confession that has been socially adopted as science has emerged, is but a mere cross-section of beliefs that can be held in common by people with a variety of different religions, worldviews, or metaphysical convictions, and each may have a different reason for prescribing to the confession. To illustrate, let’s take the example of the confessional item that there are regularities in nature to be investigated. There is no reason why Christians, Jews, Moslems, pantheists, and materialists could not all agree on this. But a materialist may have vastly different reasons for holding this common belief than a Christian, Jew, or Moslem. The materialist may, for example, have a mental picture of the universe as a kind of huge machine that operates according to mechanistic laws, whereas the Christian may believe that the laws are reflections of the continuing sustenance of the God who is faithful to His covenant promises. While these are two vastly different beliefs or items

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27 See Morris and Petcher, *Science and Grace*, chs. 4 and 5 for an elaboration of these ideas.
of faith under the surface, only the commonly held belief (part of the “confession”) that regularities are to be expected is needed for a common scientific venture to be possible. The two may argue about the deeper issues over coffee but their views on this point will have little effect on their collaboration in the laboratory.

Before I give some examples of Mere Science at work, perhaps it would be helpful to raise possible objections to this formulation of science. The first objection that might be raised is that if science cannot be defined, how can we make sure that we are able to rid the field of crackpots who masquerade as scientists, and get quite a large following, but nevertheless are actually espousing nonsense? In a sense, this is a real problem, but in practice we will do this just the way we do now. Since what we are really doing is coming to terms with the lack of demarcation criteria, the situation on this score is really unchanged. But also Mere Science ought to minimize this problem by opening up discussion on deeper levels of conviction. If we take the insolubility of the demarcation problem seriously, we cannot rule out crackpots except within the social emergence of the cultural science. Sure crackpots have their following for awhile, or perhaps even a long while if their claims do not readily run up against creational constraints that are clear to everyone, but there is little to be done about that except perhaps “evangelism.” But, someone might object, doesn’t this mean that we are giving up the notion that science is leading us to truth? To this I answer, not at all. The constraints of creation are real, and depend on God’s sustenance, not our opinions. However, our knowledge from God is revealed to us through the various channels that He has ordained, so there are always faith commitments behind the scenes which shape how our knowledge appears to us. Truth? Yes. Universally apprehended? No. Only to the extent that it is thus far revealed. Furthermore, this truth is revealed over time, and at any given time of history we should expect to see “but a poor reflection as in a mirror” and to “know in part” (1 Cor. 13:12).

Some may raise the objection that “things are going fine. Why advocate a change of mindset?” To this I would argue that things are going fine on many fronts, and in a sense Mere Science is a commendation of what is going fine. We don’t need to change the practice terribly much. But things are not going fine in a number of areas, at least from the point of view of Christians, particularly when there are strong differences of opinion. The obvious issue to raise
here is that of Darwinian evolution versus any other view that opposes that. Much of the dialog concerning intelligent design, for example, seems to be best described as two sides at loggerheads. Is this the best way to glorify Christ? Perhaps a more humble attitude is appropriate in order both to get at the real issues, and also to open the door for the gospel. For most scientists at large tend to think, while not actually dwelling on the matter, that science is fairly easily demarcated from religion. A more productive approach than arguing would be to ask on particular points why another scientist believes the interpretation he does, as opposed to another that might be on the table. This would serve to open up the issue of presuppositions behind the scenes, and to get the message out that these can be important would promote further dialog and open up opportunities for the gospel.

Another more subtle issue is that scientists at present enjoy an unprecedented amount of authority and respect because of the many scientific accomplishments of late, and this indeed represents a certain amount of power in the culture. So one objection that might be raised is actually born out of the desire to maintain this power. Scientists might be afraid of losing that privileged position if they acknowledge the complexities of the scientific enterprise, a fear that plays into the hands of postmodern critics. And yes, many scientists might actually be wielding their science as a power ploy. But this is part of what Mere Science attempts to speak against. If science does not speak with one voice on all issues, that should be faced and not hidden behind a façade of Modernist expectations of objectivity. We as Christians should not be too concerned about this for another reason. We know that the story of redemption is being orchestrated by God alone to bring us to his intended purposes, and so whatever happens in this respect should be put into His hands. In other words, let God be God! and in the mean time, be about your faithful scientific business.

**Mere Science at work**

Now how does all this look in the doing of science in various fields? First, let’s return to the example of physics. Actually most physics since the time of Newton, as it has developed through Maxwell, Einstein, Bohr, Heisenberg, and so on, is pretty uncontroversial. If we dis-
count the very, very small, and the very, very large, the laws of physics are known to a very high degree of precision, with little controversy about these laws. For example, quantum electrodynamics, which is the quantum theory of the electromagnetic interaction has been tested to a very high degree of accuracy with some measured quantities in agreement with the theory to over 10 decimal places. At least on the level that we can measure, there must be something right about this theory. So there is little debate about whether the laws of physics are correct as far as they go as long as we steer clear of more controversial of the very, very large, e.g. theories concerning the experimental discoveries of the acceleration of the far reaches of the universe, and the very, very small, e.g. superstring or membrane theory.

But if we ask a question about the past, such as concerning the Big Bang theory of the origin of the universe we add a level of controversy. This theory adds an element to the presently accepted laws, in that it involves an extrapolation back in time. In other words, if we assume that Einstein's well tested theory of general relativity is correct, and then we take the data we presently observe as input for extrapolating back in time, we will come to a very tiny universe at some point for which we do not have an uncontroversial theory. But assuming that everything evolved according to Einstein's equations, after this very early time our theory is in good agreement with the data that we see today. Of course, at present we do not know for sure how to correct Einstein's theory for very tiny distances (and from other considerations, we are quite sure it does not work for these tiny distances), so we cannot assert what happened “in the beginning” even within these assumptions. However, provided we are willing to assume that Einstein's equations describe what happened into the past until the point where the universe was so small that the theory breaks down, that is enough to conclude that there was a Big Bang. We may of course question the assumption about extrapolating into the past. To deny this assumption we could raise at least two issues. One is whether the universe could have been made to look old, but it is actually young. This would involve such things as assuming that God made light in path

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28 Some even argue such as Stephen Hawking did in *A Brief History of Time*, that we might be able to change the theory so that there was no “beginning.” This would not, however, change what must have happened from a later time on (as long as we assume the validity of Einstein's equations), nor would it be able to tell us what “really happened.” Extra philosophical/theological assumptions would be needed to conclude that.

29 That is essentially what I mean by a “Big Bang.”
on its way to earth so that it appears to have traveled for billions of years from (possibly non-existent?) stars. While logically possible, such an assumption is probably repugnant to most scientists, and may not be so palatable to many theologians.\footnote{This assertion is certainly a “science stopper,” and scientists would have as many reasons to object to it as there are underlying faiths. For example, a materialist realist might just take it as obvious that an effect must have a cause and that the cause must be found in the material universe. A Christian might argue that for God to make the universe to look old by placing light in the heavens as if there were bodies emanating this light, but for there not to be such bodies, would be strange indeed. How could “the heavens declare the glory of God” if they were lying to us about what is in the heavens?}

A second possibility is that Einstein's equations are simply incorrect when extrapolating back in time. However, because Einstein’s theory is so well attested to, this assumption would be very difficult to put to an empirical test even though it is in principle also a logical possibility.\footnote{For example, while there are some speculations about the possibility of the speed of light not being constant over the history of the cosmos (For a review, see Jean-Phillipe Uzan, “The fundamental constants and their variation, observational status and theoretical motivations", Reviews of Modern Physics 75 (2003) 403.), theories of this sort that fit the present data, and also substantially change the Big Bang picture, are hard to come by.}

In this example of cosmology, we should therefore conclude that the Big Bang model is on pretty solid ground scientifically, even though there is at least one, and possibly two, loopholes. The second of these loopholes however reveals something else about Mere Science. If someone would come up with an interesting alternate possibility to replace Einstein's theory that in the minds of most scientists actually had a chance of being correct, then it could generate tremendous interest, especially if a test were in the realm of possibility. However, the mere postulating of the possibility of another theory would not be considered interesting; there should be some bona fide connection with the observable universe to make it so. To summarize, we see here the possibility of disagreement arising even in a realm of physics that is not usually judged to be very controversial. I must point out that the disagreement arises most probably due to metaphysical\footnote{Hawking's imaginary time conjecture that the universe may not have had a beginning, because there is no singularity in spacetime fits this category. See Stephen Hawking, A Brief History of Time.} or religious\footnote{Such as a young earth view, held primarily because of an insistence on a particular interpretation of Scripture.} commitments. The important thing is that we should lay bare our own background assumptions and encourage others to do the same.
Let's look at a couple other examples. First, note that the theory of quantum mechanics is very well tested and hence not controversial. However, the various interpretations of quantum mechanics are another matter. There is quite a bit of controversy even among physicists on this matter; so the joke goes: if you have five physicists in a room, you would likely have six interpretations of quantum theory represented. But this is not really a problem for science, because most discussions about the interpretations of quantum theory actually go on in the halls of philosophy departments nowadays. It is mostly a parlor conversation topic among physicists, except for those with a philosophical bent. Hence the controversy, if there is one, is driven largely by convictions held outside of the day to day practice of science.

A more controversial area is the notion of intelligent design. One notion of intelligent design is that we may find in God's creation some structures that are “irreducibly complex” in the sense that, if one or another vital part would be missing, the structures could not even function remotely like they do now. The idea is that there are mutually dependent parts in the structure that could not in principle have been built up by “chance” from previous steps of natural selection. While the thesis may be entirely true, in practice as long as a plausible “story” can be invented by a Darwinist to “explain” any example provided, the notion will remain controversial. In this case, it should not be too much work to dig down to find the actual underlying assumptions that are driving the decision to believe one side or the other, assuming both parties are willing. The point is that these underlying assumptions may not be clearly (or merely) scientific. But this is no reason to rule out one side or the other, at least from a scientific point of view. We just live with the controversy, and perhaps more importantly, we can use it to clarify what is going on behind the scenes.

Finally, let us consider the theory of evolution itself. This theory is also clearly in controversy, even though it is held by the large majority of working scientists today. However, here we have to remind ourselves that Mere Science is not able to tell us what is bona fide science and

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34 Michael Behe, *Darwin’s Black Box*,

35 For a response to Behe, see Ken Miller, *Finding Darwin’s God*,

36 By this remark I mean that the reasons for a given conviction are neither a part of Duhemian science, nor would they be accepted as scientific by a large majority of practicing scientists. May the reader be reminded again of the demarcation problem so that no universal judgment can be made.
what is not—that would be a solution to the demarcation problem. Its main thrust is to focus us on commonalities, differences, assumptions, beliefs, and so on when discussing science. In that sense, we have to get down to particulars when trying to assess the situation or when communicating any reservations we may have. But the thrust of Mere Science here would be something slightly different. Rather than allowing the kind of arguments to go on as they have been, it would encourage the combatants to quickly get down to the underlying assumptions that play a role in their judgments. In this way hopefully dialog would be opened up, deep differences would be revealed, and even an opportunity for explaining the Gospel of Grace may be uncovered.

We might also briefly revisit one philosophical example: methodological naturalism. Where does this fit with respect to Mere Science? The real question here is, what does it help? In fact it is quite unhelpful. At best, it only serves to limit discussion in unhelpful ways, and to admit some metaphysical commitments to the table without allowing others in. At worst, it may really be serving as a false diversion from the real issues. So we have to say that there is no room for such an assumption. Let us get on with our science and with a more fruitful line of discussion.

Finally, after considering these examples, we ask: what, if anything, would change in the practice of science if the notion of Mere Science were universally adopted? The answer is, probably not much. Perhaps the discussion within science would change somewhat to include some self-reflection, but the sorts of speakers at conferences and what is generally accepted as important would not change much. The constraints of creation, the common confession, and the common scientific culture will keep science on track. The attitude toward what is going on at the “fringe” of science should eventually soften, even if only that scientists have adopted a more humble posture concerning the fringe issues. This might open up the possibility of inviting “fringe” speakers to conferences from time to time. Suppose, for example, that the organizers of an American Society for Microbiology conference decide that it is worthwhile to invite an intelligent design proponent to give a lecture, just to provide for interesting dialog, all in good fun,
because the boundary of science is not so sharp after all. People would vote with their feet whether such a lecture is worthwhile to attend, and science would go on.

What good is Mere Science if it can't tell us what is good science and what is bad science? Or what counts as science and what does not? Mere Science is really only a way of looking at science, a way of understanding why it proceeds as it does, and a way of understanding why controversies persist. It is by definition not a way of demarcating science. To that end it may be a way to sharpen the focus of controversies, and at the same time soften the debates that surround science. But in order to do this, everyone would have to appreciate its principles. To clarify, let us turn these principles into recommendations of which there are several.

The first and foremost recommendation of Mere Science is to take a humble posture in your claims about science. Humility is always good, but given all the subtleties behind the scenes of the debate, philosophical and otherwise, we should recognize that Mere Science is humility science. In that vane, we should be careful about speaking beyond our knowledge or expertise. This leads us to the second point: don't merely cite experts you trust. Experts may disagree. Make an argument. If you can't make an argument, based on your own understanding, then make sure that whatever else you say, it is said in the spirit of humility; and qualify what you say by an admission of your own lack of understanding. Third, lay bare your presuppositions. Mere Science encourages us to lay bare our presuppositions and to encourage others to do likewise. Those things that do not follow pretty clearly from the common confession are ripe for leading to controversy, and so they should be identified in order to make progress. In this context we should remember that theories are underdetermined, and so there could be any number of theories that accommodate the obvious data but diverge where the data is not so clear. Remember that we cannot claim absolute truth for science; only God knows the truth absolutely. So whether our science claims are exactly true or not, we cannot determine. We may know how good our approximations are relative to the data, but we should leave room to let God be God in terms of truth claims.