SOUTHEASTERN

THEOLOGICAL REVIEW

Vol. 3, No. 1 Summer 2012

Southeastern Theological Review
Is published biannually for the faculty of Southeastern Baptist Theological Seminary.

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Annual Subscription Rates: **$30 (regular) $15 (student)**
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Direct correspondence to: **Attn: Editor, *Southeastern Theological Review***
Southeastern Baptist Theological Seminary
PO BOX 1889, Wake Forest, NC 27588-1889 (USA)

ISSN 2156-9401

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Inference, Method, and History

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Introduction

The publication of Mike Licona's book *The Resurrection of Jesus: A New Historiographical Approach* provides a welcome opportunity for reflection on the goals and methods of historical inquiry and the implications of various methodological commitments for the study of the historical Jesus and the resurrection. Indeed, the sheer number of interesting and important topics that Licona has drawn together makes it impossible to discuss more than a small fraction of the book in a single article. I will therefore bypass with little or no comment many sections that are as fascinating as they are valuable, such as the magisterial and convincing discussion of Paul's conception of the resurrection body that spans pp. 400-437, and focus on a cluster of issues involving inference, method, and the New Testament evidence.

History and Truth

Early in the book, Licona endorses the definition of "history" as "past events that are the object of study" (p. 30), and he makes it clear that the goal of history, as far as he is concerned, is *truth*— getting it right about those past events. Neither the definition nor the goal is uncontested, and Licona takes the reader through a substantial selection of widely diverging opinions on bias and the historian's horizon, the role (and paucity) of consensus among historians, the prospects for the possibility of historical objectivity, and the burden of proof.

The cacophany of conflicting voices is deafening; and were it not for their influence, some of those voices might safely be ignored. In an irenic moment, Licona acquiesces in the idea that the postmodern critique has been valuable for the discipline of history (p. 87). This is faint praise, but I would begrudge them even this much. Scholars of the stature of J. B. Lightfoot do not need the nattering of would-be literary critics infected with bad epistemology to teach them to be judicious. We may all lament the loss of a large part of a generation who, had they received sound training, might have produced work of genuine intellectual merit. But they did not, and except as

textbook examples of ἐνέργειαν πλάνης[1] they deserve all the neglect we can give them.

Even among the saner participants in the discussion, however, there are significant points of disagreement among the scholars Licona cites. Who, if anyone, bears the burden of proof in historical discussions? Should historians approach ancient texts with an attitude of acceptance, skepticism, or neutrality? How should a reasonable historian address reported miracles? Is there a role for the application of mathematical probability in historical reasoning? And what level of confidence should a reasonable, well-informed historian have that the Gospels give us a substantially trustworthy account of the events surrounding Jesus' death and resurrection?

The Burden of Proof and Methodological Neutrality

After canvassing a wide range of opinions on the topic of the proper approach to the study of the historical Jesus and the resurrection, Licona settles on a viewpoint which he christens "methodological neutrality." The core of this idea is that the one making the claim bears the burden of proof (p. 96). Someone who asserts that Jesus rose bodily from the dead bears the burden of proof for his claim; someone who asserts that the disciples were victims of hallucinations bears the burden of proof for his claim; and all positive assertions, from all quarters, are "assumed to be false until sufficient evidence is provided to the contrary" (p. 97).

There is something very attractive about the idea of methodological neutrality. We all want to avoid excessive optimism or pessimism as we come to the examination of any piece of evidence, whatever the issue. But I am not sure that this methodological position, thus described, is as useful as Licona seems to think. It is rare indeed that we come to any interesting inquiry in the entire absence of relevant information, and that information often conditions how we should accept assertions from different quarters. In some cases, preliminary agnosticism is quite unreasonable—I should wish, for example, that everyone who possesses a modest amount of information would approach Benny Hinn's antics and assertions with a healthy dose of skepticism. And sometimes the mere fact that someone of ordinary credibility has made a claim suffices to discharge whatever burden of proof there might be. If my wife tells me that there are apples in the refrigerator, I will not approach the matter with the assumption that her claim is false until I check for myself. If a perfect stranger tells me that there has just been a serious accident on the nearby interstate, then in the absence of further evidence, I will probably accept his assertion. If Josephus informs me that Herod the Great had his fa-

[1] [Editor: "A deluding influence" from the Greek; see 2 Thess. 2:11].

vorite wife murdered, I will accept his testimony. Equal opportunity skepticism, if employed without a view to what we already know, is unreasonable.

The difficulty, of course, is that everyone seems to come to the study of the resurrection with a significant set of assumptions in place. It is difficult to see how it could be otherwise. Theist? Atheist? Agnostic but open to the possibility of something beyond naturalism? One's worldview will inevitably affect the assessment of the evidence. And it should. Does this leave us at an impasse?

Not necessarily, for three reasons. First, relevance is a two-way street, and the evidence should also impact one's worldview—a point to which I will return when considering the relevance of Bayesian methods to historical studies. Second, not all assumptions are equally reasonable. However much it may offend disciplinary pride, this is one place where philosophers and historians need one another. Dogmatic naturalism requires, for a full exposure of its bankruptcy, a philosophically informed critique. And philosophy, uninformed by history, is unable to advance a step in any direction in the evaluation of the empirical claims that lie at the heart of Christianity. Third, in our age of increasing specialization, we cannot take it for granted that everyone who comes to the discussion is well informed even about the facts that are considered uncontroversial by experts in their respective areas of specialization. The problem is not simply one of limited information; there is a depressing amount of positive disinformation disseminated by parties whose talent for propaganda exceeds their love of truth. False facts, as Darwin noted long ago, are highly injurious, for they often endure long.

Hume on Miracles

Hume's famous attack on the credibility of miracle reports has exerted a powerful effect on biblical studies from at least the time of Strauss's *Leben Jesu* (1835) onward. Licona deals with the challenge in a straightforward manner, and though I think the response could be strengthened by consideration of the rejoinders offered by Hume's contemporaries like William Adams, John Leland, George Campbell, and John Douglas and sharpened by reflection on some of the contemporary analyses of Hume, I find myself in substantial agreement with Licona's response to Hume. But I cannot say as much for some of those he quotes. Consider J. D. G. Dunn's comment, which Licona quotes (on p. 138), and which I will give a little more fully than he does:

> As David Hume had earlier pointed out, it is more probable that the account of a miracle is an untrue account than that the miracle recounted

> actually took place. That was precisely why the claim to miracle became more problem than proof.[2]

And Dunn adds in a footnote:

> One need only consider the typical reaction by most today, including most Christians, to claims of miraculous healings by 'televangelists' or miraculous phenomena linked to statues of the Virgin Mary or of Hindu gods, to see the force of Hume's argument.[3]

This is partly right and partly wrong. The right part is that in the absence of more particular evidence, claims of miraculous intervention should be held to a higher standard than claims about ordinary events; this follows, not because they are miraculous, but because—at least for most of us—they are rarer than ordinary events. But what is wrong is that Dunn leaves out of sight the question of the specific nature of the testimonial evidence. It may well be the case that the testimony of some randomly selected individual who has, so far as we know, nothing to lose by making up a tall tale will fabricate a miracle story. But not all testimony fits this description. Human nature, like physical nature, has its laws and its limitations. Even Voltaire granted that, although the majority of our beliefs are at most only probable, things admitted as true by those most clearly interested to deny them may form an exception.[4]

This concession goes to the heart of the problem with Troeltsch's "principle of analogy." Granting for the sake of the argument that visible miracles are unknown today,[5] it follows that a reported miracle in an ancient text is an event that bears no analogy to our time. But the principle cuts both ways. It is equally unknown in our day for a group of people in a hostile environment voluntarily to endure a lifetime of labors, dangers, and sufferings, and to submit to new rules of conduct, in attestation of a claim they must know full well to be false, without conceiving themselves to be deriving any earthly benefit from the pretense. The evidence of Christianity presents the Troeltschian with a dilemma: something disanalogous to the present has happened no matter which way he turns. The question, as the Oxford logician Richard Whately observed in a similar context, is not whether there are difficulties in accepting the resurrection, but whether the difficulties on the side of denying it are even greater.[6]

In *The Trial of the Witnesses of the Resurrection*, first published in 1728, Thomas Sherlock anticipates Hume by giving his protagonist the opportunity

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[2] James D. G. Dunn, *Jesus Remembered* (Grand Rapids: Eerdmans, 2003) , 103-104.

[3] Dunn, *Jesus Remembered*, 104 note 10.

[4] Voltaire, *Oeuvres completes de Voltaire* (volume 5 ; Paris, 1869), 609.

[5] But see: Craig Keener, *Miracles: The Credibility of the New Testament Accounts* (2 volumes; Grand Rapids: Baker Academic, 2011).

[6] Richard Whately, *Elements of Logic* (9th edition; London: Longmans, Green, Reader, & Dyer, 1870), 144-45.

to address the charge that the resurrection is, by its very nature, beyond the reach of evidence. The response is intriguing:

> Suppose a man should tell you that he was come from the dead, you would be apt to suspect his evidence. But what would you suspect? That he was not alive when you heard him, saw him, felt him, and conversed with him? You could not suspect this, without giving up all your senses, and acting in this case as you act in no other. Here then you would question whether the man had ever been dead. But would you say, that it is incapable of being made plain by human testimony that this or that man died a year ago? It cannot be said. Evidence in this case is admitted in all courts perpetually.
>
> Consider it the other way. Suppose you saw a man publicly executed, his body afterwards wounded by the executioner, and carried and laid in the grave; that after this you should be told that the man was come to life again; what would you suspect in this case? Not that the man had never been dead, for that you saw yourself; but you would suspect whether he was now alive. But would you say this case excluded all human testimony, and that men could not possible discern whether one with whom they conversed familiarly was alive or no? On what ground could you say this? A man rising from the grave is an object of sense, and can give the same evidence of his being alive, as any other man in the world can give. So that a resurrection considered only as a fact to be proved by evidence, is a plain case; it requires no greater ability in the witnesses, than that they be able to distinguish between a man dead and a man alive, a point in which I believe every man living thinks himself a judge.
>
> I do allow that this case, and others of like nature, require more evidence to give them credit than ordinary cases do. You may therefore require more evidence in these than in other cases; but it is absurd to say that such cases admit no evidence, when the things in question are quite manifestly objects of sense.[7]

I submit that Sherlock is right. A resurrection from the dead is an event out of the ordinary course of nature, and *in the absence of more specific information*, we should all be somewhat doubtful about it—more doubtful than about the assertion that the speaker is mistaken or lying. That is the grain of truth at the heart of Hume's rhetorical pearl. But it is quite possible for the evidence of our senses to overcome even a very substantial antecedent burden of proof. Some atheists are wont to display the strength of their conviction by suggesting that anyone who thinks he has viewed a miracle should turn himself in for psychiatric treatment. A good dose of Sherlock should clear that up.

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[7] Thomas Sherlock, *The Trial of the Witnesses of the Resurrection* (Boston: John Eliot, 1809 [1728]), 64-5.

Here is one point where I think an appreciation of Bayesian methods would strengthen Licona's case. But since he is skeptical about those methods, the subject requires some exploration in its own right.

Bayesian Probability and Historical Reasoning

Historians are generally wary of the introduction of probabilistic methods into their discipline, and the ham-handed forays of well-intentioned mathematicians and philosophers into their discipline gives them some reason for apprehension. Licona quotes several skeptical statements on the applicability of probability to history, none of them positive, and I am conscious that this consensus places me at a disadvantage as I attempt to make the case, not for every such application that has ever been made (who would want to do that?), but for at least the relevance of probabilistic methods to historical study.

Historians and New Testament scholars should be warned that in discussing this issue they are wandering into a war zone where two entrenched schools of thought—the Bayesians and the Frequentists—are constantly lobbing rhetorical grenades at one another. They should therefore take sweeping dismissive statements on both sides with an appropriate ration of salt.

Licona rightly points out that the problem of acquiring prior probabilities is a major issue for Bayesian approaches to uncertain inference, and he repeatedly describes them as "inscrutable." However, the situation is not so bleak as Licona seems to think. Four points deserve consideration here. First, under certain circumstances, symmetries in our data underwrite symmetrical epistemic attitudes toward hypotheses. If you know that one red, one green, and one blue marble have been drawn (with replacement each time) from a bag of marbles, and you know nothing else, then it would seem reasonable that you should take the same epistemic attitude toward the prediction of a red marble on the next draw as toward the prediction of a blue or a green one. The difficulties arise in finding and exploiting such symmetries in much more complex sets of data. But I am not persuaded that the Bayesian project is, as Bartholomew categorically states, "essentially subjective" (p. 117). There are more forms of Bayesianism than the subjectivism of de Finetti and Jeffrey.[8]

Second, there is no obvious reason why Bayesian conditionalization on former probability distributions must be seen as the only reasonable method of obtaining initial probabilities. It is difficult, as Licona notes (p. 116), to arrive at a reasonable probability for the existence of God in the absence of *all* evidence whatsoever. But if there is a reasonable stance to take on the probability of God's existence on the basis of some body of evidence—say, as an explanation for the origin of the universe, the origin of life, the origin of

[8] See Timothy McGrew, "Toward a Rational Reconstruction of Design Inferences," *Philosophia Christi* 7 (2005): 253-98 (288).

embodied consciousness, and the existence of objective moral values—then there is no obvious reason that one may not *start there* in considering the impact of further evidence. Bayesian methods can be put to work whenever the relevant probabilities are defined; they do not require that those probabilities have been arrived at themselves by conditionalization, and so on back to some Ur-distribution in which all propositions take well-defined values relative to tautological background information. If something other than bare priors and conditionalization is needed to set the probabilistic machinery in motion, so be it.

And this consideration touches on Licona's worry that a prior probability for the resurrection may be inscrutable. As William Paley pointed out over two centuries ago, the probability of a visible miracle may be reasonably estimated (at least for a lower bound) by the joint probability of two claims: that there is a God who has intended a future state of existence for his creation, and that he should desire to acquaint them with it in some fashion that could not reasonably be dismissed as the operation of nature or the result of mere human sagacity. For there is no other way for God to stamp his endorsement on a communication than for him to sign it with the one act that distinguishes him from all of his creation, the act of sovereignty.

Some recent writers have criticized this view of the miraculous. On pp. 143-44, Licona quotes N. T. Wright in a statement that combines some sound insights with an unfortunate lapse:

> The natural/supernatural distinction itself, and the near equation of "supernatural" with "superstition," are scarecrows that Enlightenment thought has erected in its fields to frighten away anyone following the historical argument wherever it leads. It is high time the birds learned to take no notice.[9]

I applaud Wright's insistence on following the argument wherever it leads, and his comment about the term "superstition" strikes the center of the mark. But the distinction between the natural and the supernatural cannot be so easily dismissed; it is the foundation of Nicodemus's recognition that no man could do the works that Jesus did unless God was with him (John 3:2). I hesitate to suggest that Wright has been influenced by postmodernism here, but the wholesale castigation of the Enlightenment has a depressingly familiar sound. It is not a safe rule of inference to deny something merely because it is the sort of thing that an Enlightenment thinker would say.

Third, there is more to Bayesian reasoning than a calculation of a posterior probability from priors and likelihoods. When prior probabilities are difficult to obtain, we may focus on the likelihoods, asking, in effect, "How strongly should we expect these data, supposing that the hypothesis were true; and how strongly should we expect them, supposing that it were false?" It is

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[9] N. T. Wright, *The Resurrection of the Son of God* (Minneapolis: Fortress Press, 2003), 707

true that we cannot, from the ratio of these two items alone, calculate a posterior probability. But if the ratio favors the hypothesis heavily, as it sometimes does, that fact may serve to shift the burden of proof. The mounting weight of favorable evidence can lay a burden on the doubter to explain just why and how long we should remain agnostic. This is the approach taken in some recent work on ramified natural theology.[10]

Fourth, a probabilistic analysis affords the most perspicuous way of illustrating the flaws in Hume's reasoning. Recent work on the probabilistic analysis of testimony and Hume's argument has clarified the assumptions behind the use of testimony and illuminated the flaws in Hume's reasoning in a way that even the best of the earlier work could not.[11] That is not to say that none of the points can be stated informally; Licona sometimes does so himself (see p. 141, note 24, for example). But the mathematical treatment brings a cutting edge to the analysis that permits the decisive resolution of problems that have baffled even good thinkers when they are merely stated verbally.

To take just one example, J. L. Mackie argues that

> the intrinsic improbability of a genuine miracle, as defined above, is very great, and one or other of the alternative explanations in our fork will always be much more likely—that is, either that the alleged event is not miraculous, or that it did not occur, that the testimony is faulty in some way.
>
> This entails that it is pretty well impossible that reported miracles should provide a worthwhile argument for theism addressed to those who are initially inclined to atheism or even to agnosticism. . . . Not only are such reports unable to carry any rational conviction on their own, but also they are unable even to contribute independently to the kind of accumulation or battery of arguments referred to in the Introduction.[12]

Pace tanti viri,[13] Mackie is mistaken here; the formal analyses by Rodney Holder and John Earman, following the pioneering work of Charles Babbage, have decisively refuted this claim. The accumulation of a sufficient number of independent testimonies, each with a likelihood ratio that exceeds unity by at

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[10] See Timothy McGrew and Lydia McGrew, "The Argument from Miracles," in William Lane Craig and J.P. Moreland (eds.), *The Blackwell Companion to Natural Theology* (Oxford: Wiley-Blackwell, 2009), 593-662.

[11] Rodney Holder, "Hume on Miracles: Bayesian Interpretation, Multiple Testimony, and the Existence of God," *British Journal for the Philosophy of Science* 49 (1998): 49-65.

John Earman, *Hume's Abject Failure* (Oxford: Oxford University Press, 2000); Timothy McGrew and Lydia McGrew, "The Reliability of Witnesses and Testimony to the Miraculous," in Jake Chandler and Victoria Harrison (eds.), *Probability in the Philosophy of Religion* (New York: Oxford University Press, 2012), 46-63; Timothy McGrew, "Miracles," *The Stanford Encyclopedia of Philosophy* (Fall 2011 Edition), Edward N. Zalta, ed., URL = http://plato.stanford.edu/archives/fall2011/entries/miracles/.

[12] J. L. Mackie, *The Miracle of Theism* (Oxford: Oxford University Press, 1982), 27.

[13] [Editor: "With due respect to him" from the Latin].

least some given finite amount, however small, will swamp *any* finite antecedent presumption against an event. It follows that it is simply incorrect to say, as a number of well-respected Christian scholars[14] have said, that one *must* believe in the existence of God before reported miracles can play *any* role in one's evidence for the existence of God.

Historical Bedrock and the Historical Reliability of the Gospels

The heart of Licona's project is the examination of the resurrection in the light of certain pieces of data that he calls "the historical bedrock." The concept is an important one for his project, and he gives us several different descriptions of it. The bedrock consists of "historical facts that are regarded as virtually indisputable" (p. 278); they are "so strongly evidenced that they are virtually indisputable," and "the historian can fairly regard them as historical facts" (p. 56); and "the majority of scholars regard them as historical facts."

I must confess here my profound unease with any method of proceeding that leaves the data hostage to the current consensus in biblical studies. In part, this unease arises from an historical induction. Few scholars now remember the jubilant confidence with which the results of German scholarship were received by the more progressive Victorians, so it may be of some value to recall the breezy summary of John Fiske:

> The times and places at which our three synoptic gospels were written have been, through the labours of the Tübingen critics, determined almost to a certainty. Of the three, "Mark" is unquestionably the latest; with the exception of about twenty verses, it is entirely made up from "Matthew" and "Luke," the diverse Petrine and Pauline tendencies of which it strives to neutralize in conformity to the conciliatory disposition of the Church at Rome, at the epoch at which this gospel was written, about A.D. 130.[15]

Alas for the assured results of "criticism"! Today few scholars even of the more liberal stripe would accept either the second century date or the thesis about the direction of dependency between Matthew and Mark. Yet on the whole, members of the guild are still prone to pass over the community's more embarrassing blunders (such as falling for Morton Smith's forgery of

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[14] Norman Geisler, *Christian Apologetics* (Grand Rapids: Baker, 1976), 95-6, 147; R.C.Sproul, John Gerstner, and Arthur Lindsley, *Classical Apologetics* (Grand Rapids: Zondervan, 1984), 146-7, 276; Willard, Dallas, "Language, Being, God, and the Three Stages of Theistic Evidence," in J. P. Moreland and Kai Nielsen (eds.), *Does God Exist?* (Nashville: Thomas Nelson, 1990), 197-217, 213-15.

[15] John Fiske, *The Unseen World and Other* Essays (Boston: Houghton, Mifflin and Co., 1904), 108.

Secret Mark)[16] and to exempt certainly currently fashionable ideas (such as Q, with or without redactional layers) from dispassionate scrutiny. It is very hard for someone immersed in the field to resist the suggestion that this time, it's different.

I do not wish for a moment to suggest that it is impossible to arrive at historical truth on the basis of evidence; I believe, in fact, that the goal is often achievable in New Testament studies. My quarrel is not with the evidence but with certain elements in the guild. And the point that I wish to stress is that the consensus of the scholarly community is at best a contingent marker for the weight of the evidence. It should never be substituted for the evidence itself.

Licona is well aware of the spotty track record of an appeal to the majority, and he says explicitly that "the majority of scholars have been mistaken on numerous occasions in the past" (p. 57). However, the individual scholar decides which facts belong in the bedrock not only by looking at the current consensus but also by judging that the consensus itself is well founded: these are the facts that *ought* to be taken for granted in any historical reconstruction. And here we encounter a problem: what should the responsible scholar do when, in his best judgment, there are facts that the community *ought* to take for granted but does not? Should he include those (with appropriate argumentation on their behalf) among the facts he seeks to explain? Or should he take the more minimal approach, arguing only from the facts that are both well supported and (nearly) universally accepted by the current scholarly community?

Licona chooses the latter path. The advantage of working only with such "minimal facts" is obvious: it reduces one's exposure to scholarly disagreement, as little or nothing in one's premises will arouse the skepticism of one's peers. But the tradeoff for this advantage is that one's basis is not so rich as it might have been and perhaps should be. This drawback of a minimalist method shows up in Licona's unfortunate concession that "whether the resurrection narratives in the canonical Gospels reflect independent apostolic tradition" is merely "*possible*" (p. 208). I agree with him that the letters of Paul contain valuable material that may fairly be regarded as almost certainly reflecting apostolic tradition. And there is certainly room for a book, like this one, that explores the question of how much one can legitimately infer regarding the resurrection without making use of the Gospels. But that case can be, I think, materially strengthened by the use of the resurrection narratives, which are after all our most detailed sources for the event.

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[16] See: Morton Smith, *The Secret Gospel: The Discovery and Interpretation of the Secret Gospel According to Mark* (New York: Harper & Row, 1973); *Clement of Alexandria and a Secret Gospel of Mark* (Cambridge, Mass.: Harvard University Press, 1973); see also Stephen C. Carlson, *The Gospel Hoax: Morton Smith's Invention of Secret Mark* (Waco, TX: Baylor University Press, 2005).

I suspect that Licona's fairly negative rating of the resurrection narratives in the Gospels reflects not so much his personal judgment regarding their historical value as his awareness of the magnitude of the task that confronts anyone who wants to answer, in detail, the wide array of arguments against the substantial historicity of the resurrection narratives. *That* project could fill many substantial volumes, and this one is already long enough. But if this was his reason for trying to see how much could be done without making use of those narratives, then I wish that he had indicated his intentions in some other, less deprecatory fashion.

Another reason for my unease regarding the current consensus arises from considerations of methodology. I have more to say on this subject than can reasonably be said in one short essay, so I will confine myself to illustrating two types of bad methodology with which much recent New Testament scholarship is infected. First, the argument from silence, which is used so widely in negative criticism, is deeply problematic. Instances in the Gospels are thick on the ground. Did Herod the Great murder some Jewish male children in Bethlehem as we read in Matthew 2? If so, why is the event not mentioned by any other evangelist or by Josephus? Did Jesus raise Lazarus from the dead as reported in John 11? If so, how could the other evangelists omit the event? And John himself omits many things found in the Synoptics—if they had really happened, how could he have failed to mention them? Such questions are asked rhetorically. The unspoken inference is usually one of two kinds: first, that if the events had actually occurred, we would find corroborating reports of them, and since we do not, the events did not occur; or second, that if the author really knew whereof he spoke, he would have mentioned such events, and since he does not mention them, he does not have firsthand knowledge.

Such arguments are generally extremely weak, and I think they would be made less frequently in biblical studies if scholars took more notice of the nonsense they would make of secular history. Licona mentions one case: Josephus does not mention Claudius's expulsion of the Jews from Rome in or around AD 49, an event mentioned in passing in Acts 18:2 and explained, albeit briefly and unsatisfactorily, by Suetonius (*Life of Claudius* 25.4). Such cases can be multiplied many times over; I will simply list a dozen striking illustrations here. The principal historians of ancient Greece, Herodotus and Thucydides, make no mention of Rome or the Romans, nor do any of their contemporaries whose writings have survived—a curious omission noted by Josephus in his work *Against Apion* 1.12. Thucydides' *History* makes no mention of Socrates, whom we would now be inclined to view as one of the most important and interesting characters in Athens in the twenty years covered in that work. The works of Thucydides themselves go unmentioned in the surviving works of Aristotle and Xenophon; we must, in fact, wait two and a half centuries, until Polybius, to find a historian who takes notice of Thucydides. In two long letters to the historian Tacitus, Pliny the Younger gives a

detailed account of the eruption of Mt. Vesuvius—yet strange to relate, the governor of Bithynia never mentions the destruction either of the wealthy town of Herculaneum or of the more heavily populated Pompeii. Hadrian's secretary Suetonius also discusses the eruption of Vesuvius; but he, too, neglects to tell of the destruction of these towns. They are first named about a century after Pliny by Dio Cassius (*Roman History* 66), who not only could not have been an eyewitness but in all probability never spoke to one. Yet modern archaeology places their destruction beyond doubt. Eusebius wrote an official biography of Constantine that makes no mention of the death of his son Crispus or his wife Fausta. Marco Polo traveled across China in the late 13th century and wrote a massive travelogue, but he never mentions the Great Wall of China. Grafton's *Chronicles*, comprising the reign of King John, make no mention of *Magna Carta*. The archives at Barcelona reportedly contain no firsthand report of Christopher Columbus's return from his circumnavigation of the globe. Francis Bacon and William Shakespeare were nearly exact contemporaries, each with a large literary output, yet neither mentions the other. Similarly, John Milton and Jeremy Taylor fail to take notice of each other. Ulysses Grant published two volumes of his memoirs of the Civil War,[17] yet he never mentions the Emancipation Proclamation.

Such examples suggest that the possible causes for an author's omitting something that we now find interesting are more varied and more common than the causes for including something. An honest author will include an account of an event or a mention of a monument because he wishes to convey the truth to the reader; a dishonest author may invent it because he wishes to lead his readers into falsehood. But an event may be omitted for any number of reasons. Perhaps the author was inadvertent. Perhaps it slipped his mind while he was writing. Perhaps he had mentioned it already in some other work now lost to us and was disinclined to repeat himself. Perhaps he felt no desire to go back over ground already covered well by others in extant works. Perhaps he judged its significance for his purposes to be less than we should judge them. Perhaps he suppressed it out of delicacy or out of a desire to give certain individuals then alive protective anonymity.

Second, the practice of erecting elaborate theories on slight literary parallels has an alarming grip on the New Testament studies community. Consider, for example, Andrew T. Lincoln's reiteration of Benjamin W. Bacon's thesis that the account of the resurrection of Lazarus in John 11 is a literary reworking of materials from Luke:

> [T]he present form of John's story, with its particular figures and their characterization, its other literary features and its clearly Johannine theological themes in the dialogue, appears to be a skilful composition on the

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[17] Ulysses Grant, *Personal Memoirs of U. S. Grant* (2 volumes; New York: Charles L. Webster & Co., 1885).

> part of the evangelist, in which the named characters Martha, Mary and Lazarus have been taken over from Luke's Gospel (Luke 10.38-42; 16.19-31). John's narrative could well be a very extensive literary elaboration based on the general tradition that Jesus raised the dead or on one particular tradition of the raising of a dead man, which he has set in Bethany and associated with a family said to be close to Jesus, and whose miraculous character he has heightened by having the dead man already in the tomb for four days.[18]

Speaking as an outsider to the guild of New Testament scholars, I submit that this fantastic hypothesis of literary dependence, ungrounded in any independent evidence of such cobbling construction on John's part, would provoke open ridicule in any other philological discipline. Sadly, it would not be difficult to create a long list of commentators who take seriously the notion that the story of Lazarus in John 11 is in some sense based on the parable of the rich man and Lazarus in Luke 16. It is true, and a welcome development, that Tal Ilan's name lists have helped to dispel some of the fog by demonstrating that Eleazar/Lazarus was quite a common name in Palestine in the first century.[19] But it should not have been necessary to wait for this sort of evidence, which we were not guaranteed of being able to recover in any event. It should have been enough to point out that with such methods one might undertake to "show" that a randomly selected chapter from *The House at Pooh Corner* is a reworking of Matthew—or *vice versa*.

For all these reasons, I look forward to a renaissance of solid historical exploration of the Gospels in which dubious methodology is replaced by sound canons of historical investigation and hyperbolic doubts about their historical worth are displaced by a full appreciation of their value. There are promising works that move in this direction already.[20] It would please me greatly if Licona should in the near future feel led to contribute to their number.

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[18] Andrew T. Lincoln, *The Gospel According to St. John* (BNTC; London: Continuum, 2005), 42.

[19] Richard Bauckham, "The Bethany Family in John 11-12: History or Fiction?" in Paul N. Anderson, Felix Just, and Tom Thatcher (eds.), *John, Jesus, and History, Volume 2: Aspects of Historicity in the Fourth Gospel* (Symposium; Atlanta, GA: Society of Biblical Literature, 2009), 185-201.

[20] Craig Blomberg, *The Historical Reliability of the Gospels* (second edition; Downers Grove, IL: IVP, 2007); *The Historical Reliability of John's Gospel: Issues and Commentary* (Downers Grove, IL: IVP Academic, 2011); Craig Keener, *The Historical Jesus of the Gospels* (Grand Rapids: Eerdmans, 2009).