Zygon®: Journal of RELIGION & SCIENCE

Brooke, John Hedley. 2024. "On the Experience of Being Replicated." *Zygon: Journal of Religion and Science* 59 (2): 564–74. DOI: https://doi.org/10.16995/zygon.11501

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On the Experience of Being Replicated

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This article provides an informal account of the experience of being replicated, with a summary of the original author's earliest misgivings. It then identifies features of the third chapter of the book *Science and Religion* that may make it peculiarly resistant to replication. Then, the questions are addressed whether and how a replication itself should be replicated. Finally, the article provides reflection on the value of replication by responding to the earlier contributions led by Hans Van Eyghen and Rachel S. A. Pear in this thematic section.

Introduction

This is not, as its title might suggest, an article about identity theft. It is about a very different and less menacing challenge. It came as no small surprise to learn that a book I published more than thirty years ago, and more specifically a chapter I began writing in the early 1980s, was to be given prominence in a project on the feasibility of replication studies in the humanities (Brooke 1991, 82–116; 2014, 110–57). When I received the invitation to serve on the project's advisory board, it was impossible to suppress feelings of gratitude that my book was still enjoying such recognition, but these feelings were also laced with apprehension. What exactly was a replication study? Should I be concerned by the prospect of having my work forensically dissected? What value could there possibly be in reevaluating a text that, however influential, was now showing its age?

I therefore begin my informal account of the experience of being replicated with a summary of my earliest misgivings. I then identify features of my third chapter—a commentary on possible parallels between scientific and religious reform—that I believe made it peculiarly resistant to replication. Then to the mischievous question: whether, and how, a replication should itself be replicated! I finally reflect on the value of replication by responding to the judicious contributions of Hans Van Eyghen and Rachel S. A. Pear. As this is an informal contribution, I do not engage with the deepest theoretical questions discussed by Rik Peels and his colleagues at Vrije Universiteit Amsterdam.

Initial Reservations

My original misgivings arose from what I saw as a potentially high degree of disanalogy between replication practices that may constitute an idealized scientific methodology but seem largely foreign to the culture of many humanities disciplines. The elimination of the subject, a conventional ideal in the protocol of the experimental sciences, struck me as impractical and largely foreign to a discipline such as history, where evidence of the cultural formation and predilections of authors in the selection and interpretation of their data is not only tolerated but almost expected. Moreover, if in the replication of a piece of historical writing the main aim is the validation or a deeper questioning of the original research, what are the criteria for validation when, in both context and content, scholarship has advanced beyond what was available at the time of writing—the 1980s for my *Science and Religion*? For example, in the salient chapter, I was both sympathetic to and critical of Robert Merton's famous thesis concerning Puritan values and the stimulus they allegedly gave to the practical sciences in seventeenth-century England. But suppose I had argued trenchantly in its favor. How would the replicator deal with the embarrassing fact that Merton later came close to disowning his thesis and that, by 2000, its numerous flaws had been fully exposed and succinctly expressed (Davis and Winship 2002, 125–28)?

I had other reservations. How is it possible to replicate a methodology that may not be explicit in the original text? In retrospect, I have tried to reconstruct the route I took to the methodology that was tacitly used in the research for, and composition of, my book. In this rational reconstruction, I could identify seven distinct stages in the refinement of my own approach to my sources, both primary and secondary. The details need not detain us, but my skeptical thought was how could someone who had not traveled that same route validate a tacit methodology that had its own history in the life of the author? Is the replicator obliged to study, where possible, the author's biography or autobiography? Underlying my skepticism was a creed I had often heard my historian colleagues express, namely, that the writing of good history is more a craft skill than a scientific exercise.

Even in the sciences, as Harry Collins (1985) points out, the practice and evaluation of a replication are not straightforward tasks. It matters who repeats an experiment or study, who counts as a legitimate peer, and how their relationship to the original experimenter is evaluated (cf. Penders, Holbrook, and de Rijcke 2019). There are questions to be asked about the perceived competence of the replicator as well as that of the original experimenter. The same must be true in the humanities. One of the several distinguished historians of science to teach at Vrije Universiteit Amsterdam, Martin Rudwick (1985), drew attention to what he calls a gradient of perceived competence among participants in scientific controversies and its influence in shaping outcomes, in shaping what counts as scientific knowledge. The perceived stature of the person undertaking the replication, whether in the sciences or the humanities, has to be considered, and this is an inescapably social matter.

If I was originally skeptical, it was for the reasons I have just adduced. Having participated in the project, primarily as an observer but occasionally as an advisor, some of those reasons now seem less incisive. Their applicability would seem to vary with the subject and type of replication undertaken, making generalization difficult. In my comments on the contrasting modes of replication pursued by Van Eyghen and Pear, I shall give examples of what I have found valuable in their work. Accordingly, rather than multiply grounds for skepticism, I turn now to their respective presentations, complimenting them both on the courtesy and fairness with which they have treated my work. It is a compliment they deserve, not least because there were features of my third chapter that might almost have been designed to deter any would-be replicator!

Chapter 3 and Its Resistance to Replication

Among the propositions in chapter 3 of my *Science and Religion* that might almost have been inserted as a defense against future replication studies, one was a feature of the book as a whole, another a feature of my treatment of Merton's thesis, with which Van Eyghen engages. I was discussing Merton because his

celebrated study of *Science, Technology, and Society in Seventeenth-Century England* (Merton 1938, 1970) was germane to my question of whether there were significant parallels between scientific and religious reform. His thesis was that a god-centered involvement of Puritans in the world would encourage the growth of science. There could be real connections between the spiritual injunction to glorify God and a quest for knowledge that would not only demonstrate the Creator's power but also alleviate suffering. It was an argument that bore a certain similarity to that deployed by another of Vrije Universiteit Amsterdam's distinguished historians of science, Reijer Hooykaas (1972), in his work on Calvinism and science.

In my response to Merton, I was compelled to introduce a complication. However plausible the parallel between scientific and religious reform, testing it was extremely difficult (Brooke 1991, 112–16; 2014, 151–57). The strategies in place at the time, which often involved head-counting in incipient scientific societies to see if there was a disproportionately high number of Puritans in their membership, were inconclusive and the numbers open to different interpretations. My frustration was compounded by a lack of consensus on how to define "Puritanism," how best to differentiate it from other expressions of Protestant thought, and how to cope with its changing complexion as political circumstances in mid-seventeenth-century England changed. Sometimes, the terms Puritan and Protestant were used interchangeably, occasionally even by Merton himself. This meant there was a deterrent to easy replication embedded in my text. How would a replicator thirty years later be able to navigate the same frustrations?

The feature of the book as a whole that might repel replication is its openness to complexity when researching the relations between scientific and religious thinking. I had adopted this as a historiographical principle, in contrast to the restrictive master narratives of conflict or harmony I wished to critique. Somewhat to my embarrassment, this has been labelled "Brooke's complexity thesis," whereas I see it as a heuristic prescription for enlarging openness to, and grasp of, the richness and diversity of the arguments and positions defended in the past and (as a corollary) in the present. It is not so much a thesis with explanatory pretensions as a methodological principle that facilitates the critique of master narratives that have dominated polemical literature (Harrison 2017, 223–26). It is also a principle that assists the retrieval of lost understandings and the generation of new historical perspectives (Hesketh 2017, 191–92). But it does so at a price. It could make the validation of my historical analyses more challenging because the validator will have to contend with the same levels of complexity and inconclusiveness as those identified in my original text. Were my replicators irritated by my relish for complications? Did what I experienced as difficulties in testing the idea of parallels between scientific and religious reform translate into difficulties they might have experienced in conducting their replication?

Replicating the Replication?

I have subsumed my next few words under this cheeky heading because it is impossible to read reviews of one's own work without forming an impression of their fairness and accuracy. And who is to adjudicate the cogency of the replication? Preparing for the workshop held at Vrije Universiteit Amsterdam in June 2023, I had the opportunity to read a preliminary draft of Hans Van Eyghen's article, which I found reassuring. On minor points I had a few concerns I was able to transmit to him. But my overriding impression was of fairness and sensitivity when summarizing my line on Merton, as in the extract below:

> Brooke's interest does not lie in evaluating Merton's portrayal of the Puritan sentiment that allegedly fostered scientific legitimacy. Instead, his focus is on whether that sentiment led to greater acceptance of science. Brooke does add that science may have been valued by Puritans because it affords a useful diversion from sensuality—from bags, bottles, and mistresses, as Robert Boyle would put it (Brooke [1991] 2014, 148). Brooke ([1991] 2014, 149) does slightly rephrase Merton's original thesis to state that "Puritan values helped to create an audience receptive to programs for the improvement of man's estate." Brooke therefore stresses the increased legitimacy of practical science rather than all of science—an element also at work in Merton's original defense.

I would happily settle for that. Nevertheless, as a critic, I should perhaps give an example of a remark that puzzled me. In the course of his exposition, Van Eyghen mentions at least three different ways in which a Puritan religiosity might have been a catalyst for the applied sciences, such as medicine and agriculture. A stimulus might have come from the belief that the application of science to mitigate human suffering should count as a good work, not (it should be emphasized) as a means of earning salvation but as a duty of faith. A second might have been through the hope of finding greater assurance of one's own salvation through the experience of God's blessing in one's worldly endeavors. A third might have come from a different hope, that the application of science might go some way to restore the dominion over nature Adam had lost at the Fall. What surprised me was the remark that "Brooke's emphasis on undoing sin or setting the image of God right by science is . . . somewhat strange." It would have been if I had used those words. But even in Francis Bacon's potentially secular vision of a science-based utopia it was not claimed that science could undo sin. That was only possible through divine initiative and mediation. I certainly doubt that a Puritan cleric such as William Perkins, who Van Eyghen is discussing at that point, would have warmed to Bacon's program. I was, however, following the lead of Charles Webster (1975, 21–31), who in his book *The Great Instauration*, recognized a millenarian streak in exemplary Puritans who were attracted to Bacon's reformist agenda. There are of course

ways in which the doctrine of the Fall could have been obstructive to imaginative scientific theorizing. However, as Peter Harrison showed in *The Fall of Man and the Foundations of Science*, the pursuit of science for altruistic purposes could also have been married to a Puritan eschatology. With its biblical justification in the prophetic message of Daniel 12:4, that "many shall pass to and fro, and knowledge shall be increased," it could have been seen as hastening the millennium and Christ's return (Harrison 2007, 186–88).

There is a hint of self-defense in that last paragraph, but is there not also a pointer to the value of a replication study if it persuades an author to revisit a former self? In my case, I was impelled to reread Harrison on the Fall and his debt to Webster. At this juncture, having now reintroduced the question of value, I shall look more closely at some examples from the conclusions of both Van Eyghen and Pear of what, as the author in the hot seat, I found valuable in their studies.

The Value of Replication

Can there really be value in a replication study, particularly when the text under review, in some respects at least, will almost certainly have been superseded? It has occurred to me latterly that a project designed to investigate the reasons certain books win acclaim when others in the same domain are less successful could have a general value. Here, I simply select two features of Van Eyghen's investigation that were valuable to me, in that they drew attention to two kinds of omission in my text. In one case, the omission was of a geosocial group, the New England Puritans. In the other, the omission was of a possible correlation between the growing acceptance of science in late seventeenth-century England and a growing indifference to religion, a growing secularity, among natural philosophers.

Van Eyghen finds among New England Puritans a range of positions on divine activity in nature, the consideration of some of which, he implies, would have enhanced my discussion. Among them is a willingness to say that God can make use of natural forces as secondary causes to achieve "God's purposes"—purposes that might include meting out punishment for moral transgression. The rationalization of earthquakes had a high profile in that context. The key point is that it was not a case of either a naturalistic or a supernatural explanation. It was a case of both/and. Theological strategies for admitting the simultaneous coexistence of two levels of causality were important for the natural sciences and it is good to be reminded of this. Although I did discuss this issue in chapter 4, with reference to the providentialism of Robert Boyle in the context of his "mechanical philosophy," I probably did say too little about it when dwelling among the Puritans in chapter 3. It is worth adding that the changing status of scientific naturalism within Christian thought from the seventeenth century onwards is the subject of an outstanding recent book by Peter Jordan (2022) *Naturalism in the Christian Imagination*.

I was helped to see my second omission by the fact that Van Eyghen found something in one of my secondary sources that he considered too important to neglect. The source was an essay by Lotte Mulligan (1973) on "Civil War Politics, Religion and the Royal Society." In this, she traces the religious background of more than 150 early members of London's premier scientific society. I had been attracted to her conclusion because it added weight to critiques of Merton. What had she found? "The typical background of a science enthusiast in the 1660s was not middle-class, mercantile, puritan, politically radical, unacademic or utilitarian. Rather, our typical Fellow was a royalist, Anglican, university-educated gentleman" (Mulligan 1973, 108).

The Puritan minority was as small as one in twenty of her sample. Mulligan did concede that, within that group, there was a disposition towards the practical sciences. Might this in itself give limited support to Merton's position? This is the context in which Van Eyghen rightly detects my greater sympathy for the alternative to Merton's thesis favored by Barbara Shapiro—namely, a correlation between science and the moderate, latitudinarian wing of the Anglican Church in which a spectrum of non-fundamental doctrinal positions was tolerated. This latitudinarianism was epitomized by the mathematician (and eventual bishop of Chester) John Wilkins, England's most proactive Copernican, who, especially after the restoration of the monarchy in 1660, displayed a broad-minded tolerance of religious dissent and a distaste of Puritan dogma (Shapiro 1969, 1983). I had not seen any real tension between Mulligan and Shapiro, but, by contrast, Van Eyghen does. He describes Mulligan's conclusion as "starkly different" from mine. This is because she opines that acceptance of new ideas in the Royal Society might have had more to do with a waning interest in religious disputes and the waning influence of religious ideas than with any specific religious mentality. As a result of this replication study, I have been reminded that my book did not sufficiently address some of the possible links between science and religious indifference, or between science and secularization. There is surely value in that, at least for me.

With reference to Rachel S. A. Pear's polished conceptual replication, it was gratifying to learn that her study of Jewish historians and their treatment of Jewish responses to Copernican astronomy was judged to be consonant with the historiographical principles for which I had argued. For example, she found a similar pushing back against denominational, deterministic perspectives in which scientific preferences had been too neatly and narrowly ascribed to specific religious allegiances: "The impact of Brooke and his like-minded contemporaries was so great that what they fought hard for thirty years ago is now somewhat taken for granted." I was particularly intrigued to read her work because I knew of historians of "Judaism and science" who had taken my historiography seriously, resulting in one case in a vivid description of how it had changed the framing of his doctoral project and even the young scholar

himself (Efron 2010, 247–50, 255–58). I was also intrigued by Pear's discovery that, where there was openness among Jewish commentators to scientific reform, and specifically to the new astronomy, no religious reform comparable to that released by the Protestant Reformation had been a prerequisite. There are interesting issues here, not least in the scope for new research on the Catholic Reformation and its representation in scientific education, notably in the sphere of mathematics, in which Jesuit educators were prominent and where popular religious imagery could still be involved (Castel-Branco 2021).

I particularly value the opportunities Pear's study presents for serious comparative work. I should introduce my first example with reference to an epistemological distinction, fundamental to an understanding of the reception of Copernican astronomy. In broad terms, this was the instrumentalist/realist distinction. Are the mathematical models of the astronomer representations of a physical reality (does the Earth really orbit the sun, as Galileo believed?) or are they typically instruments for the prediction of planetary motions? Within the Ptolemaic tradition, mathematical models were essentially for predictive purposes; cosmological representation belonged more to the domain of Aristotelian philosophy. The distinction can, and should, complicate the determination of what a reformist position might be. It could be more radical to accept that mathematical astronomy can describe a real cosmological system, even while rejecting the Copernican hypothesis, than to accept a heliocentric model as the most mathematically elegant while excluding, in principle, that it could ever be presumed to describe a physical reality.

I wondered whether this instrumentalist/realist distinction was also pinpointed by Jewish historians as a complication—a question I put to Pear. From an essay by Noah Efron and Menachem Fisch, to which she guided me, I learned that this was not such an issue among early Jewish commentators on heliocentrism. For example, rabbi David Gans was willing to say that Tycho Brahe had "proven clearly" that the planets Mercury, Venus, Mars, Jupiter, and Saturn had orbits centered not on the Earth but on the sun (Efron and Fisch 2001, 74). He also praised the sharpness of Copernicus, who wanted to prove that the Earth is not stationary but perpetually revolving. But when it came to deciding between the Ptolemaic, Copernican, and Tychonic systems, Gans would not conclude that any one was superior to the others. There is a contrast with Western astronomers, yet there are so many nuances in Gans's position that it provides extra confirmation that speaking of a reformation in science is no straightforward matter (Efron and Fisch 2001, 74–75).

My second example concerns a citation Pear gives from a book by David Nieto, an Italian rabbi-physician who moved to England in 1701 to serve the Jewish, Spanish, and Portuguese community. His book *Mateh DaN* (1714) raises the question of whether the motion of the Earth can be accepted, given that Joshua's famous command was to the sun (not the Earth) to stand still. One of

the speakers in Nieto's dialogue says decisively that the Copernican model must be rejected for that reason. His partner in the dialogue asks how that objection is answered, eliciting the reply that "they claim that the prophet used this language so that the ordinary person could understand it, for [ordinary people] believe that the Sun moves and that the Earth is motionless." The response is abrupt and decisive: "That answer has no value" (quoted in Brown 2013, 109–10).

Here, an answer is dismissed that many Protestant reformers, including John Calvin, and many Protestant natural philosophers, including Isaac Newton, found attractive (Snobelen 2008a, 491–530; 2008b, 691–732). It is often called the accommodation theory because the language of scripture is understood to be "accommodated" to the needs of ordinary people. Seeing that stratagem so cursorily dismissed in Nieto's dialogue prompts me to ask a further question: How widespread was this accommodation principle among Jewish commentators on science and the Hebrew Bible? For the most radical Jewish commentator, Baruch Spinoza, scripture simply cannot be accommodated to the new sciences (Rudavsky 2008, 558), but what of those with "softer" views? This is just one example of the fertility I see in Pear's replication study precisely because of its stimulus to such further comparative work.

A Final Question

I began this essay on a rather negative note as I reported my initial misgivings. I end with a question that might also appear subversive, though I think it is worth asking. It applies more to a direct than a conceptual replication. Having said that Van Eyghen's study reminded me of omissions in my book, there are reasons I had to say "reminded." Primarily, this was because there were reviews at the time of publication in which gaps and deficiencies were identified. The most rigorous of these was an essay review by the historian Scott Mandelbrote published in *Annals of Science*. It was the kind of review one dreams of, in which he wrote of my book that "it must now become the standard against which to measure all future ventures into this field" (Mandelbrote 1993, 373). Nevertheless, it did not escape criticism, and two or three of his reservations stuck in my mind. One was my high dependency on secondary sources, even for quotations. Interestingly, one of those instances was my discussion of the Merton thesis in chapter 3, where, as my accompanying bibliography shows, I was striving to navigate a veritable library of recent secondary literature (Brooke 1991, 361–66; 2014, 490–97).

A particular lacuna Mandelbrote regretted was that I had not said more about putative connections between science and secularization. I hope that I have at least partially remedied that in subsequent publication (Brooke 2010). The resemblance between Mandelbrote's observations and some of those made by Van Eyghen does, however, lead to a question. What ultimately can be gained from the direct replication of a historical text that could not be gained simply

by reading a cross section of reviews written around the time of publication? Responding to that question at the Vrije Universiteit Amsterdam workshop on replication in the humanities, Van Eyghen adroitly pointed out that once a reputable replication is in place, it could save those interested in the quality of a book the considerable labor involved in locating and reviewing the reviews. It is difficult to quarrel with that, though it is a reply that raises different questions. From where will the necessary replicators be found? What proportion of professional historians, for example, would prefer to replicate the work of their colleagues rather than pursue their own research projects?

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