

STATISTICS AS SCIENCE: LONERAGAN, MC SHANE, AND POPPER

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On this occasion of honouring the achievement of Philip McShane, I would like to recall his earliest and, in my judgment, most important work, *Randomness, Statistics and Emergence*.¹ In particular, I will recall how that work situated Loneragan's important breakthrough on statistical method in relation to the major currents of thought on the subject, many of which remain influential still today.²

From afar, the question of the scientific status of statistics seems beyond question. For better and worse, statistical analyses and tables of data suffuse almost every scientific publication (as well as quite a few that are non-scientific), but especially in the fields of health care, economics, sociology, and psychology. Readers of such publications often must wade through lengthy and tedious justifications of the statistical methods, assumptions, and protocols. Indeed, instruction in statistical methods and procedures are required of virtually all students of the social and natural sciences.

Yet, if one moves in for closer inspection, the exact scientific status of statistical methods becomes blurry, despite their pervasive usage. Few scientists could give a coherent account of why they employ statistical methods – beyond very

¹ Philip McShane, *Randomness, Statistics and Emergence* (Notre Dame, IN: U of Notre Dame Press, 1970). All page references to this article appear in the text in parentheses.

² McShane's book also contains keen insights of exceptional value for the newly resurgent interest in 'emergent properties.' However, I must limit my discussion in this essay just to his treatment of randomness, probability, and statistics.

pragmatic justifications such as, ‘The journal or the funding agency requires them,’ or ‘Such statistics are needed if we are to impact public policy.’ In fact statistical methods have been intertwined with pragmatic concerns since their very inception in Girolamo Cardano’s efforts to improve the fortunes of gamblers.

If we turn from these pragmatic justifications to philosophical discussions more centrally concerned with the epistemological issues raised by statistical investigations, we are confronted overwhelmingly with thinkers from within the empiricist tradition. Early on, writers in the empiricist tradition – most notably John Locke – attempted to invoke probabilistic notions in order to extricate themselves from a series of impasses – only to have David Hume raise a seemingly insurmountable barrier to their efforts. Much of the subsequent history of philosophical reflection on probability and statistics has been dominated by the effort to improve upon Locke’s approach and to overcome Hume’s critiques. Even those authors who worked diligently on questions of the foundations of probability but who cannot be strictly classified as empiricist were responding to sets of problems framed by this empiricist debate.

Two major twentieth century thinkers – Bernard Lonergan and Karl Popper – recognised that in order to properly treat the foundational issues raised by probability and statistics, it would be necessary to step outside of the confines of empiricism. In *Randomness, Statistics and Emergence*, McShane carefully and comprehensively situates the contributions of Lonergan’s thought with respect to the spectrum of epistemological and metaphysical issues that arose out of the empiricist tradition. Although McShane does mention Popper a few times, he does not bring Popper and Lonergan into direct dialogue on the issue of statistics as science. In this essay, therefore, I will explore how the positions that Lonergan staked out, and the ways that McShane developed those positions, can address the work of Karl Popper on the subject of statistics as scientific knowledge.

McShane on Randomness, Probability and Statistics

I first encountered Philip McShane's *Randomness, Statistics and Emergence* as a graduate student, seeking aid in understanding Lonergan's formidable achievement in the philosophy of statistical method. Returning to McShane's 1970 study for this *Festschrift*, I could not help but be impressed once again. Its style of expression is engaging. It leads the reader carefully, patiently, and gradually through many complex issues. This is not to say that the book does not make considerable demands upon the reader. It does – because the subject matter that it treats is intellectually demanding by its very nature. *Randomness, Statistics and Emergence* is comprehensive, not only in addressing the issues presented in Lonergan's *Insight*, but in its treatment of the other major writers on the problems of the foundations of statistics. McShane gives careful, fair, and clear presentations of each thinker's positions. He also makes Lonergan's treatment of statistical method even more accessible to the introductory reader than Lonergan himself did.³ All the while McShane organises his presentation in accord with the exigences of Lonergan's 'moving viewpoint.' McShane's book remains far and away the very best source for anyone wishing to enter into the details of the *Insight* chapters on empirical scientific method.

Early on McShane identifies the bewildering array of seemingly disconnected topics that appear almost at random in the writings of various thinkers on probability and statistics. He treats, for example: the problem of defining 'randomness,' the question of the proper axiomatic foundation for probability theory, the concept of a 'population' or 'aggregate' as fundamental, the elementary operations of classifying and counting, the vexing relationship between causal and statistical accounts, the 'difficulty of defining an initial state accurately enough' and why 'data are effected by numerous causes,' the problem of the complexity of laws governing concrete

³ An exception is Chapter 6, 'Chance,' where McShane engages in a lengthy, tangential discussion of space-time structure. While there is an indirect connection between this discussion and chance, clarity would have been served by making space-time the subject of another book.

situations, and especially the intractable debates over ‘induction and probable degree of confirmation’ (14-16). Not only does he provide an overview of this complex and seemingly disjointed field; he also gradually shows the great dialectical and hermeneutical value of Lonergan’s fundamental insights for the reading of the history of discussions on these topics. Let me offer but three examples:

McShane quotes a standard textbook definition of statistics as follows: statistics is ‘the branch of scientific method which deals with data obtained by counting or measuring the properties of populations [or aggregates]’ (14). He then notes that the textbook authors rightly identify population or aggregate as ‘the fundamental notion of statistical theory,’ but then astutely observes that they fail to explain how this fundamental notion is connected in any way with the basic statistical operations of classifying and counting. Somewhat later McShane surveys a series of attempts by different authors at defining randomness, and then notes (reminiscent of the spirit of Aristotle) that ‘the common factor in all the usages of the words ‘random’ and ‘randomness’’ is some form of ‘no reason why,’ [an] absence of reason’ (18-28). He then goes on to show how this shared denial of intelligibility is in fact the denial of a particular kind of intelligibility – namely, the kind of intelligibility that permits precise prediction, or, in the more refined terms Lonergan developed, a lack of classical, systematic intelligibility. McShane shows the surprising unity among these disparate efforts, for a ‘population or aggregate’ is more precisely defined as ‘a coincidental manifold not held together by any [classical] law.’ This more refined definition further implies ‘residues’ in the data which can be properly investigated by the elementary statistical procedures of classifying, counting, and calculating actual relative frequencies. Later McShane goes on to show how the seemingly ‘abstract’ theoretical treatment of ‘ideal probabilities’ by a still third cohort of authors is related to the concrete procedures of statisticians investigating concrete populations. Drawing upon his sophisticated appropriation of Lonergan’s ideas, McShane reveals how the disparate work of these various groups of authors ‘can all be contributions to the

clarification of some basic but polymorphic fact,' to adapt a phrase from Lonergan himself.⁴

A second example comes in McShane's careful discussion of convergence. Convergence was a topic of considerable difficulty as the calculus was passing through its adolescence. What is now called 'analytic convergence' was initially thought of by relying upon the imagination – i.e., relying upon unacknowledged imaginative elements rather than careful formulations of the purely intelligible elements.⁵ Put imaginatively, an analytic function converges to a limit when it 'gets closer and stays closer' to that limit. Now if one conceives of a probability as an ideal limit to actual frequencies,⁶ one encounters a difficulty. Actual frequencies diverge non-systematically from ideal probabilities. They do not 'stay closer to' the ideal frequency. They keep bouncing around. Probability theorists employ the fiction that actual probabilities converge on ideal frequencies (probabilities) 'as the number of cases goes to infinity.' But in empirical investigations, the number of cases never goes to infinity. McShane's summaries and analyses of this set of problems (149-69) are remarkable for their clarity. His proposal for an alternative to the prevailing conundrums is deeply insightful. I will return to this issue in the last section of this paper.

A third example comes in McShane's clear and illuminating analysis of a very concrete statistical investigation: the distribution of three species of buttercups in two grassland areas near Oxford, England. He shows how several points from Lonergan's treatment of the complementarity of classical and statistical procedures illuminate the deeper assumptions and significance of these concrete studies – and, by extension, empirical investigations in general. As McShane puts it, this 'example illustrates in the simplest way the oscillations of [the classical and statistical] attitude within the process of inquiry' (71).

McShane narrates how, in the first of the two grassland

⁴ CWL 3, 412.

⁵ See Carl B. Boyer, *The History of the Calculus and Its Conceptual Development* (New York: Dover Publications, 1959), 267-87.

⁶ See for example, CWL 3, 81-85.

settings, statistical procedures were used to confirm randomness of distribution of the three species. In the second setting, however, 'there was a marked heterogeneity shown by a significant non-randomness in the distribution' (71). The non-randomness in this case was due to the phenomena of the 'clumping' of like species. He then explains that this rather surprising result led investigators to search for a classical explanation of the clumping. They once again used statistical procedures to demonstrate a remarkable statistical correlation of each of the three species with a distinct kind of microhabitat: 'Ranunculus bulbosus was found to occupy bands of land running along the tops of ridges; Ranunculus acris occupied the sides of the ridges and Ranunculus repens lay in the furrows' (73). This statistical correlation eventually led to the discovery that each of the three species is classically correlated with (adapted to) differences in drainage and moisture content of the soil. As McShane notes, the statistical results provided 'clues' for further classical investigations, and the classical results provided ever more refined, scientifically significant categories for statistical investigations. In other words, use of the intrinsic statistical notion of 'significant difference' can lead to investigations of what might be called 'classical residues' in a body of statistical data, as well as the reverse.

McShane's buttercup example is much more illuminating than the old chestnuts of coin flips and billiard balls (both of which McShane also treats as a matter of course, but within a far richer context). This example also underscores Lonergan's insistence that statistical investigations are far more closely tied to the concrete than are classical investigations.⁷ It also reveals how statistical investigations seek 'to disentangle a complex of causes ... to discover what causes are the important ones and how much the observed effect is due to each' (75-76). This use of statistical methods to disentangle causes is absolutely essential when actual experimental separation of variables is not possible, especially in cases of human studies where physical, experimental of separation of variables would violate ethical norms. McShane's analysis of this series of

⁷ CWL 3, 121.

investigations shows how one can appropriate ‘what scientists are doing when they are investigating’ even when scientists themselves have absolutely no explicit, thematic awareness of Lonergan’s ideas on these topics. McShane also shows the foolhardiness of trying to use statistics to determine the probability of an individual event; a population, not an event, is the proper object of mathematical probabilities. McShane’s analysis of this example should be required reading in introductory courses on the use of statistical methods of research.

Karl Popper on Science and Statistics

McShane clearly studied the work of Karl Popper carefully at the time he was writing *Randomness, Statistics and Emergence*. He comments on Popper at several points in the book.⁸ It is somewhat surprising, therefore, that he does not enter into an explicit dialectical encounter with Popper on the issue of the scientific status of statistical investigations. In this section I will outline Popper’s positions on science and statistics. In the next section I will show how McShane, drawing on Lonergan’s work, offers resources for a nuanced dialectical critique of Popper’s position on statistical science.

Popper was something of a maverick among philosophers of science in the 1930’s and 1940’s. He deliberately distanced himself from the hegemonic ‘Received View’ of logical positivist philosophy of science that reigned in those years.⁹ In particular, he thought that logical positivism faced insurmountable obstacles regarding the meaning of theoretical terms and the confirmation of theoretical claims. Scientific theories, Popper recognised, make universal claims. As such, their theoretical statements cannot be made to conform to the positivist criteria of meaningfulness, because universal, theoretical statements can be neither logically ‘derived from

⁸ McShane, pp. 21 n., 24, 30-31, 67, 136 n., 153, 245-46. There are possibly also indirect comments on pp. 60 and 147.

⁹ For a masterful historical account of the rise and fall of the ‘Received View,’ see Frederick Suppe, ‘The Search for Philosophic Understanding of Scientific Theories,’ pp. 3-232 in *The Structure of Scientific Theories*, second edition, Frederick Suppe (ed.) (Urbana, IL: U of Illinois Press, 1977).

experience' nor 'logically reduced to elementary statements of experience.'¹⁰ Such elementary statements are, after all, particular, and there can be neither logical derivation nor reduction of the meaning of a universal to a particular. Likewise, insofar as one regards the telling feature of a scientific statement to be its empirical verification, Popper claimed that none of the outstanding and widely recognised examples of scientific theories would be acceptable as scientific. It is of course possible to verify particular observational predictions that are derived from the universal laws and principles of a theory. However, no finite number of verified particular predictions ever constitutes the verification of a universal principle, let alone the conjunction of several such universal principles. Hence Popper's strong and disturbing claim, 'Theories are, therefore, *never* empirically verifiable.'¹¹

Popper argued that his critique of the positivist account of science posed a serious problem because, if science means 'empirically verifiable' and theories are not empirically verifiable, then there is no clear way of separating scientific theories from metaphysics. The positivists (and indeed Popper himself) wished to draw that line very strictly. Hence Popper argued that a new 'criterion of demarcation' was needed to strictly separate statements that are scientific from those that are non-scientific (i.e., mathematical, logical, and metaphysical). Popper's own criterion of demarcation is 'falsifiability' rather than verifiability: 'it must be possible for an [authentically] empirical scientific system to be refuted by experience.' Popper explains that, whereas the universality of theoretical statements prevents them from being confirmed by singular statements, the logical form of inference known as *modus tollens* makes it logically possible to refute (falsify) a system of universal statements by singular statements.¹²

¹⁰ Karl R. Popper, *The Logic of Scientific Discovery* (NY: Harper and Row Publishers, Inc., 1968), 34-36 (emphasis in original).

¹¹ Popper, 40.

¹² Popper, 41. Popper acknowledges that there are sly ways to evade 'naïve' falsification (42), and later refines his criterion by supplying additional, methodological 'rules' or 'conventions' (78-92). Those details need not occupy us for present purposes. As an aside, it should be noted

Popper's discussion of singular, empirical statements is quite subtle. He astutely points out the difficulties involved in presuming a simple basis for empirical statements in perceptual experiences, in the ways that positivists did. For Popper, experiences do not justify statements; 'statements can be justified only by statements.'¹³ He further argues that statements such as 'I perceive a patch of red' are purely subjective¹⁴ and not subject to intersubjective testing. In addition, he points to the indispensability of using theoretical terms in reporting experimental data.¹⁵ All this leads him to an unusual but ingenious way of defining experiential, or rather 'basic,' statements. First, basic statements are defined in virtue of their logical form as those statements that are potential falsifiers of a theoretical system. Second, basic statements make assertions about events, not experiences.¹⁶ Finally, basic statements divide into those that are 'accepted' and those that are not accepted (rather than those that are 'affirmed' or 'denied' as Lonergan would claim). That is to say, acceptance rather than verification is basic to science as empirical because it is a matter of intersubjective testability and falsifiability. Recourse to reports about my perceptual experiences will not do. The empirical basis of a science is intersubjective agreement. As Popper puts it:

Any empirical scientific statement can be presented
... in such a way that anyone who has learned the

that Lonergan also has a kind of 'demarcation criterion' regarding the difference between scientific and metaphysical claims (see *Insight*, CWL 3, 548). But whereas Popper offers a dismissive tolerance of metaphysical statements – dismissive because metaphysical statements offer no 'contact' with evidential statements, tolerant because his theory cannot claim they are meaningless – Lonergan's metaphysical statements are grounded in data of consciousness, and thereby are really albeit indirectly related to scientific statements (CWL 3, 5).

¹³ See Popper, 93-98.

¹⁴ Popper, 44.

¹⁵ Popper, 84.

¹⁶ On this point Popper and Lonergan share a remarkable level of agreement, although because of their diverging views on the need for a grounding of judgments in reflective insights (grasping the virtually unconditioned), their residual disagreement is significant.

relevant technique can test it. If, as a result, [someone] rejects [our] statement, then it will not satisfy us if he tells us all about his feelings of doubt or about his feelings of conviction as to his perceptions. What he must do is to formulate an assertion which contradicts our own, and give us his instructions for testing it. If he fails to do this we can only ask him to take another and perhaps more careful look at our experiment, and think again.¹⁷

Hence, for Popper, empirical science is empirical insofar as there are intersubjectively accepted basic statements about events. It is science (rather than, say, metaphysics) insofar as there is intersubjective agreement in advance that certain basic statements, if accepted, will constitute a falsification of a system of universal, theoretical statements.¹⁸

Although this intersubjective and volitional dimension of Popper's thought is frequently overlooked, it is in fact quite foundational to his whole enterprise. For Popper agreement is a matter of choice, both about what statements are to be accepted, and about what 'methodological rules or conventions' one will adhere to in attempting to falsify hypotheses and theories. These choices, says Popper, are founded in value judgments:

I freely admit that in arriving at my proposals I have been guided, in the last analysis, by value judgments and predilections. But I hope that my proposals may be acceptable to those who value not only logical rigor but also freedom from dogmatism; who seek practical applicability, but are even more attracted by

¹⁷ Popper, 99. Although Popper clearly believed that he had abolished 'subjective' statements about perception and reflective grasp of the virtually unconditioned from philosophy of science, one can easily notice his failure to do so completely in this remark.

¹⁸ I would note that Lonergan offers 'relevant techniques' for attentiveness, understanding, judging and intersubjectively discussing claims about the data of consciousness. These are no less subjective than discussions among well-trained observers concerning the data observed by looking into a microscope. See Ian Hacking, 'Do We See through a Microscope?' *Pacific Philosophical Quarterly* 62 (1981).

the adventure of science, and by discoveries which again and again confront us with new and unexpected questions, challenging us to try out new and hitherto undreamed-of answers.¹⁹

Hence, for Popper the foundations of empirical science do not lie in a bedrock of sense perceptions. Rather, the foundations of empirical science are value judgments and choices. In the next section I will return to this issue, and suggest that although Popper is basically correct in this claim, still he has not entirely appropriated value judgments and choices as they relate to the praxis of empirical science.

Popper's account of the nature of scientific knowledge has received a kind of tacit reception within large domains of the scientific community. I have had numerous conversations with practising scientists who will eventually say something like, 'We don't know what the reality out there is really like. What we discover and prove is what it is not like.' I have come to suspect that these sorts of statements reflect the vertigo and anxiety of being a scientist in the twentieth and now twenty-first centuries. The shocks of the revolutions in twentieth century science shook modernity's confidence in the solidity of Newtonian mechanics. Contemporary scientists live with an intense (albeit marginalized) awareness of the possibility that their current theories may be proved incorrect. Often enough, they are well aware of the 'anomalies' that trouble and tend to subvert their theories. I think that psychologically scientists protect themselves from full confrontation with this fact.²⁰ This is indeed better and more realistic than smug confidence in the unrevisability of Newtonian physics and its mechanistic worldview (including its essential assumptions of an absolute space and an absolute time). However, as I shall suggest below, Lonergan offers a better basis for practising this humility than does Popper.

¹⁹ Popper, 38.

²⁰ For an illustration of a thinker who dares to contemplate this fact without psychological protection, see Max Weber, "Science as a Vocation," *From Max Weber: Essays in Sociology*, trans. and ed. H.H. Gerth and C. Wright Mills (New York: Oxford UP, 1946), 129-156.

When he turns to the question of the scientific status of probability and statistics, Popper's reflections bear some remarkable affinities with those of Lonergan. Like Lonergan, and especially as amplified by McShane (131-48), Popper recognised the erroneous conflation of probability of events with probable truth of statements (judgments). Like Lonergan and McShane, Popper delves into the significance of the relationships between actual and ideal relative frequencies. However, in the overall context of Popper's positions on empirical science, probability statements are going to present a special difficulty. We might say that Popper set himself a difficult task because he got his cognitional theory wrong. But the way he handled that task is quite impressive – as well as instructive for students of Lonergan.

How is it possible to falsify a statistical claim – whether statistical law or probability? On the one hand, there is no great difficulty in finding basic statements that could falsify a classical law such as 'Light always and for every inertial observer has the same velocity.' For example, 'Smith and Jones are both inertial observers, but obtained unequal measurements of the velocity of light' will falsify the universal, theoretical statement. On the other hand, how does one find basic statements that will falsify Gregor Mendel's statement that the probability (ideal frequency) of smooth peas is $3/4$? If Lonergan and McShane are correct about events diverging non-systematically from ideal frequencies, then an actual observed frequency differing from $3/4$ does not falsify the claim. Even a multiplicity of experiments, all resulting in actual frequencies different from $3/4$, would not falsify the probability claim. (Indeed, none of Mendel's experiments came up with even a single instance of an actual frequency equal to $3/4$).

So it would seem that, by Popper's standards, statistical investigations regarding probabilities cannot be regarded as empirical science. Statistics, then, must be metaphysics.²¹ (Some people have suspected this all along!) But here we witness the brilliance of Popper, for he met this challenge head on and creatively. While displaying great respect for the work

²¹ Popper, 197.

of von Mises (like McShane), Popper recognises that von Mises's 'axiom of convergence' is an untenable assumption for an empirical science.²² He sets out, therefore to develop alternative accounts of randomness, convergence, and the fundamental theorems of probability theory.

His argument is lengthy, detailed, technical, and meticulous (dazzlingly so).²³ However, the general thrust of his ideas can be simplified without too much distortion for present purposes. Popper first develops his own, modified definition of randomness.²⁴ He then considers a random sequence, infinite in length, of events characterised by a set of definite properties (say heads or tails), and develops a definition for the probability p of the occurrence of a property P (say heads), in that sequence. He then considers a selection (or sample) of finite length n from that sample. He then notes that the actual number of occurrences m of P in that finite sequence will constitute a relative frequency m/n of that property, and that, in general, m/n will be different from the probability p . This difference can be called the deviation of m/n from p . Popper defines that deviation as $D_p = |m/n - p|$. Popper then considers the question of how one might falsify the hypothesis that the probability of occurrence of property P is p . He offers his answer to the question in terms of the behaviour of D_p . The mathematical theory of probability says, in effect, that p is the true probability if D_p converges toward zero as n tends toward infinity. But this is no help in a finite world or for scientists working under even more restricted circumstances. Popper therefore does something that in a rough way resembles Lonergan's approach – he asks what scientists (at least physicists) are doing when they are doing statistical investigations. His answer:

the physicist might perhaps offer something like a *physical definition of probability*, on lines such as the following: There are certain experiments which, even if carried out under controlled conditions, lead to

²² Popper, 151-54.

²³ See Popper, 151-214.

²⁴ For McShane's critique of Popper's reconstruction of randomness, see pp. 30-31.

varying results. In the case of some of these experiments – those which are ‘chance-like’, such as tosses of a coin – frequent repetition leads to results with relative frequencies which, upon further repetition, approximate more and more to some fixed value which we may call the *probability* of the event in question. This value is ‘...*empirically determinable* through long series of experiments to any degree of approximation’; which explains, incidentally, why it is possible to falsify a hypothetical estimate of probability.²⁵

In effect, the scientist’s answer – ‘to any degree of approximation’ – says that D_p is ‘sufficiently small.’ Popper recognises that logicians and mathematicians will raise a series of objections to the scientist’s answer, for, as he notes, that answer ‘modifies the concept of probability: it narrows it.’ He further observes that this amounts to a ‘methodological decision’ to modify the mathematical definition of probability for scientific purposes. Popper himself follows this lead, and adopts this methodological decision of the practising scientist as a principle of his philosophy of science.²⁶ He goes on to note that, in practice, scientists settle their decision of what is an acceptably (or unacceptably) ‘small’ D_p by their choice of the number n – and he shows how the two are intrinsically related.²⁷ As he points out emphatically, it is only by means of this (or some comparable) methodological decision that probability statements become falsifiable.

²⁵ Popper, 199. The last emphasis is my own; otherwise, the emphases are Popper’s. The abbreviated quote is from Born and Jordan’s book on elementary quantum mechanics.

²⁶ Popper, 199.

²⁷ Popper, 200-203. Popper’s argument is more complicated than suggested in the main text. More specifically, Popper examines not just D_p , but rather the probability that the actual relative frequency, m/n will fall within D_p of the ideal frequency, i.e., probability p . In effect this places the ‘measurement’ of m/n within a set of other such measurements. Although Popper does not say so, this means that the practising scientist is choosing n in light of the tacit knowledge (or at least a tacit belief) that the ideal frequency p is implicitly re-tested in all sorts of related experiments. See CWL 3, 98.

As noted earlier, Popper recognises that methodological decisions rest upon value judgments. Yet he does not tell us just which value judgment grounds the scientist's (or his own) methodological decision regarding the testing of probability hypotheses. If pressed, he would almost certainly agree that the value of the 'adventure of science,' the value of keeping the 'game' going,²⁸ motivates this decision. After all, his long and detailed treatment of statistics and probability is followed immediately by his discussion of quantum mechanics – and surely the history of quantum mechanics has been an adventure of science par excellence. But earlier Popper explained that, to his way of thinking at least, the value of the adventure of science amounts to the value of 'discoveries which again and again confront us with new and unexpected questions, challenging us to try out new and hitherto undreamed-of answers.' Should we take him at his word? Surely Popper's career reveals a mind that has spent long periods of time absorbed in what Lonergan called the 'intellectual pattern of experience.' His writings on probability alone give clear evidence of that. But is Popper really affirming the value (in Lonergan's terms, 'the objective') intended by the pure, detached, unrestricted desire to know? Here I must hesitate. For all Popper's laudable and dogged pursuit of questions, it seems that there are questions that he is not really interested in pursuing after all. Metaphysical ones, for example, although he is not as dismissive as the positivists he criticises.²⁹ More substantively, he is not interested in questions pertaining to the data of consciousness – or whether there even are such data. This is revealed in his dissatisfaction with reports about 'feelings of doubt or feelings of conviction,' which certainly could describe experiences of (data of consciousness on) inquiring and grasping the virtually unconditioned. It may also be that his understanding of freedom (as portrayed in his political writings) is subject to further pertinent questions that he was not interested in entertaining – e.g., concerning the limits and snares of merely immanent forms of human self-criticism. In other words, he is not interested in pursuing

²⁸ Popper, 53.

²⁹ Popper, 38.

questions of human moral impotence and divine grace. For these reasons I now turn to consider how McShane's parallel consideration of probability as verifiable can be used in a dialectical refinement of Popper's approach. This is not to deny, however, that Popper's reflections on the foundations of statistics and probability are admirable and bring to light issues that few others considered.

McShane on the Practice of Statistical Science

Loneragan fretted about the problems associated with verifying (not just falsifying) probability hypotheses. It is likely that these concerns were responsible for the relatively few and technical changes made for the Second Edition of *Insight*.³⁰ McShane takes up this issue in an extended discussion under the heading 'Foundations of Statistics' (149-69). There he grapples with the problems of whether and how actual relative frequencies converge upon ideal frequencies (probabilities). 'We will be concerned ... not so much [with] ... axiomatic formulation as [with] ... empirical origin and reference' (149). Like Popper he points out the difficulties involved in this non-analytic form of convergence. Like Popper, McShane appeals, beyond the mathematical definition of convergence as n tends toward infinity, to the actual practices of empirical scientists. But unlike Popper, McShane (equipped with Loneragan's normative guidance into self-appropriation) can appeal to the conscious activities of the scientist for assistance in approaching this problem.

As a first step in this direction, McShane draws attention to an implicit 'broader insight' underlying the 'intuitive notion of probability' (161) that is already imminent and operative in the practices of statistics. This insight underlies the operative assumption that, though possible, 'indefinitely long runs of either heads or tails ... are somehow not to be expected.' But rather than attempt to formulate that unformulated insight, McShane takes a reflective step back and focuses instead on 'the process toward that developed theory as illustrative of the general process of developing science and mathematics' (153). That general process is familiar to students of Loneragan's work

³⁰ See CWL 3, 'Note to Second Edition,' 9.

as the invariant structure of consciousness, underpinned by the pure, unrestricted desire to understand correctly, choose the good, and love unconditionally. When it comes to the more specific practices of statistical investigation, that general structure is differentiated and specialised by the questions that focus upon empirical non-systematic processes and coincidental manifolds. In other words, attention to the actual statistical practices of empirical scientists leads to an exploration of the kinds of insights, judgements of fact, judgments of value, and decisions that surround the non-systematic.

To speak of non-systematic processes implicitly presupposes, in turn, some notion of systematic processes. But just what is a systematic process? It is a temporal sequence of events, the data from which ‘possess a single intelligibility that corresponds to a single insight or single set of unified insights,’ other things being equal.³¹ The context of Lonergan’s remark makes it clear that the single insight, or at least the most prominent members of the single set of unified insights, will be insights into what Lonergan calls ‘classical correlations’ that grasp explanatory relations of things to one another. But what exactly are classical insights? Ultimately this is a question settled by self-appropriation, not by definition. Definitions of classical insights presuppose insights into those insights. Insights into classical insights presuppose that one has had such insights and has invested considerable effort into attending to them and understanding them correctly. Understanding classical insights correctly is itself an ongoing, self-correcting (even a hermeneutical, circular) process that begins with obvious examples (such as Lonergan and McShane present) and passes on to consider more subtle examples. This amounts to saying that the proper foundation of the concept of systematic process is the practice of classical empirical method. Practising that method, as Popper rightly notes, is ultimately a matter of decision.

Likewise, decisions to practice are the foundations of the notion of non-systematic process. Lonergan defines a non-systematic process in terms of a systematic process: “whenever

³¹ *CWL* 3, 71. See also McShane, 34-51.

a group or series [e.g., a systematic process] is constructed on determinate principles, it is always possible to construct a different group or series [e.g., a non-systematic process] by the simple expedient of violating the determinate principles.”³² This means that the very notion of non-systematic processes relies upon the practices of classical method for its heuristic formulation. But of course the practices of empirical statistical methods also rely upon additional decisions to methodically pursue inverse insights into non-systematicity, and to pursue the further types of insights into empirical (vs. abstract mathematical) probabilities. Such decisions also entail commitments to pursue judgments regarding the correctness or incorrectness of those probability insights: ‘whether world process is systematic or non-systematic is a question to be settled by the empirical method of stating both hypotheses, working out as fully as one can the totality of their implications, and confronting the implications with observable facts.’³³ Although many might desire a well-formulated, simple set of rules for the testing of the implications of statistical hypotheses regarding non-systematic processes, Lonergan and McShane are more realistic. They recognise that this sort of simplicity is but a pipe dream, and that the only reliable guide is the ‘pure question’ underlying the self-correcting acts of knowing and deciding. It is these concrete acts of knowing and deciding that constitute the concrete patterns of ‘oscillations of attitude’ between classical and statistical procedures in investigations of concrete sets of data. McShane nicely illustrates this in all its concreteness in his intentionality analysis of the scientific studies of buttercup ecology.

Although Lonergan’s account of the relationship between judgments of value and decisions was too brief in *Insight*, and although neither he nor McShane brought even that much to bear upon the practices of empirical statistical methods, still there are elements in Lonergan’s writing that point to a more

³² *CWL* 3, 72. Something similar holds true, ultimately, for the heuristic definitions of coincidental aggregates and randomness. See p. 73, 78-81.

³³ *CWL* 3, 76. On the subtlety in Lonergan’s thought regarding ‘observable facts’ as it parallels that of Popper, see *CWL*, 94-97, 299.

satisfactory account than Popper offers. First and foremost, Popper construes rationality on the narrow model of formal logic. (This is why Popper says that methodological decisions ‘must, of course, be ultimately a matter of decision, going beyond rational argument.’³⁴) Lonergan, on the other hand, opens up the meaning of ‘rationality’ to include all the resources and ‘more rudimentary elements’³⁵ that the human mind employs in the self-correcting processes of knowing and valuing. For Lonergan, the broader meaning of rationality derives from asking and answering questions in quest of invulnerable insights grounding judgments of fact and value. This includes but goes beyond mere logical operations. Hence rationality for Lonergan includes but goes beyond formal logic. Decisions and judgments of value need not be ‘beyond rational argument’ in this more profound sense.

Moreover, the ways that practising scientists follow the lead of their questions and figure out how to properly oscillate between classical and statistical procedures reveal their decisions to attempt to understand the concrete empirical universe – or at least some part of it. Their methodological decisions to do so rest upon rational and responsible judgments of value that it is worthwhile to attempt to correctly understand the concrete universe, even if it is in fact non-systematic. But, we may ask, why is it valuable to attempt to correctly understand a non-systematic universe? In what does that value consist?

On the one hand, there is the ‘horizontal finality’ of knowledge as a good in itself – ‘knowledge for its own sake.’ That is to say, scientists and indeed philosophers like Popper take their stand on judgments of value that the achievements of understanding, and understanding correctly, are good within a limited meaning of ‘good.’ Moreover, Popper’s advocacy of falsificationism reveals, further, that it is also valuable to know which understandings are incorrect.

But I believe that we are pushed beyond this limited meaning of ‘good’ if we contemplate the full range of Popper’s affirmation of the value of ‘discoveries which again and again

³⁴ Popper, 37.

³⁵ *CWL* 3, 306.

confront us with new and unexpected questions, challenging us to try out new and hitherto undreamed-of answers.' This is an affirmation not just of the knowledge that results from the interplay of classical and statistical methods. It is an affirmation of the value of being led wherever the pure, unrestricted desire leads. This is a matter not just of the value of horizontal, but of vertical finality. In its most general form, that vertical or 'terminal' value is to be known in a judgement that the objective of the unrestricted desire to know, to choose the good, and to love unconditionally is good, and that the pursuit of that objective is worth the commitment of one's life. That 'objective,' Lonergan argues, is God and all that God values and loves.³⁶ In addition, there is also the more limited vertical value that affirms that correctly understanding the systematic/non-systematic/emerging universe is of profound worth for the good of the human race. For human beings can

discover emergent probability; [they] can work out the manner in which prior insights and decisions determine the possibilities and probabilities of later insights; [they] can guide [their] present decisions in light of their future influence on future insights and decisions ... [thereby assuming] responsibility to the future of [humanity].³⁷

These are the proper value-foundations for the truly scientific practices of empirical statistical methods. In the brilliant radiance of these values, the more commonly cited values – winning at gambling, or getting research accepted for publication, or even impacting public policy – pale by comparison. Perhaps a more detailed exploration of the transcendent value of God that grounds the universe and all human pursuits within it would be desirable. But an extended elaboration is beyond the proper bounds of this *Festschrift*.³⁸

³⁶ See *CWL* 3, 686.

³⁷ *CWL* 3, 252-53. See also the larger context of this remark, pp. 250-67. Of course the chapters from *Randomness, Statistics and Emergence* not treated in this essay explore this larger set of issues.

³⁸ See however Patrick H. Byrne, 'Analogical Knowledge of God and the Value of Moral Endeavor,' *MJLS* 11 (1993), 103-136.

Conclusion

On this occasion of honouring Philip McShane, I have endeavoured to recall the great contribution of *Randomness, Statistics and Emergence*. In doing so, I have tried to show how that work, and the work of Lonergan that it advanced, has implications beyond its explicit discussions to issues such as those raised by Karl Popper. I hope that I have succeeded in persuading at least some readers to return to this impressive starting point in McShane's career.

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