Objective Probability: A New Old Concept for Sociology

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Abstract

Sociologists today face the alluring temptation of transforming action into data while squeezing out interpretation. We argue that sociology is ill-prepared to make this choice primarily because it has been inoculated against any concept of probability that does not limit it to a question of method. This article engages in a reformulation of the concept of objective probability as new to sociology at present but old to sociology in the past as it represents a lost style of reasoning for the consideration of non-numerical probabilities. In contrast to other probabilistic approaches like frequentistism or Bayesianism, objective probability consists of actual states of the world rather than large numbers of observations or degrees of belief. We argue for three main advantages of the concept of objective probability over these contemporary contenders: first, it bridges the divide between prediction and action; second, it lessens the methodological gulf between probability and interpretation, and third, it offers a framework for the explanation of action that can unify computational, algorithmic, and cognitive approaches.

> I must premiss that we, all of us, use this word ["probability"] with a degree of laxity which corrupts and rots our reasoning to a degree that very few of us are at all awake to.
> – Charles Sanders Peirce, *Notes on the Doctrine of Chances*

Introduction

A growing segment of the sociological community appears interested in taking a path toward data science, drawing technical tools from it, methods, novel data sources, and even larger goals like changing the bar for what a sociological explanation should be able to do (Watts, 2014; Lee and Martin, 2015; McFarland, Lewis, and Goldberg, 2016; Salganik, 2017; Molina and Garip, 2019; Edelmann et al, 2020). Computational methods and cutting-edge analytic techniques borrowed from machine learning and computer science, and the promise of novel sources of mass quantities of data, hold growing sway in the discipline. Action remains a viable concept, and interpretive approaches remain mostly secure for the time being; but questions have been raised about those tools and what their continued significance means for sociology as it confronts new challenges to foundational elements the discipline has relied upon for the last century.

At the turn of the twentieth century, statistics was professionalized and recognized as an academic discipline worthy of its own journals and university departments. Key actors in the nascent American sociological field, typically within fiercely competitive departmental contexts where statistical tools signaled a reputable scientific identity, engaged in an early adoption of statistical practices (Camic and Xie, 1994; Calhoun, 2006). During the postwar consolidation of the American sociological field, the very definition of social research became tightly meshed to the needs of statistical analysis, something observable in core texts like Paul Lazarsfeld and Morris Rosenberg's (1955) edited volume *The Language of Social Research* from 1955. The reformation of the field in "the sixties" (Strand 2020) fed an anti-positivist skepticism toward statistics dogma and the proliferation of alternative sociologies, most rooted interpretive and post-positivist orientations (Alexander, 1988).

"Variables sociology" has remained a target of scrutiny ever since, reaching its height in the late 1990s and early 2000s (Abbott, 2001). By that time, new approaches like Markov chain Monte Carlo simulations did away with the strong parametric assumptions of traditional statistics (Macy and Willer, 2002). Meanwhile, the cultural turn has fully solidified, creating a canon of established concepts and perspectives as culture became central to nearly all theoretical debates over the last few decades (Spillman, 2019).

Until recently, to treat data outside of any relationship to theory or method was unusual.[1] Debates about the theory- or method-ladenness of data have repeatedly clashed over any suggestion that data might constitute a "raw" product and an unmediated view of an underlying phenomena. Logical positivism (see Hempel, 1935) and Karl Popper's (1959/1934) principle of falsification enshrined two consequential meanings of data in this respect. First, data are representations of real phenomena out there in the world, and their representational content is *fixed*. Regardless of how the data is put to use, they can only distinguish one phenomena in the world. Second, the function of data is determined in relation to theory. Data can prove a theory wrong (e.g. "falsifying") and also serve to illustrate or demonstrate a theory. Thus, the primary scientific output should be some revision of theory. But as the philosopher Nancy Cartwright (1983, p. 100ff) argues, these are not so much rules of positivism as they are of theoretical science, for which physics is the model *nonpareil*. A phenomenological science, by contrast, is something more akin to engineering, dealing mainly with intervention and manipulation of the world, and consisting only of local and contextual "phenomenological laws" because they only hold "on account of more fundamental ones" (like the

[1] There is no apparent heritage of this in sociology. Data are typically treated as a relational category defined by their connection to theory (Parsons, 1939), or with concepts *cum* indices (Lazarsfeld and Rosenberg, 1955), or, in more contemporary fashion, with methods and sampling. Nevertheless, Lazarsfeld (1958, p. 125) at least implies that data may not be so conditional on method, theory, or even a research question. It can "accumulate" on its own: "The crux is, of course, the meaning of the term 'data.' Experiments might be the only source of data for the physicist, and they might be useful for the social scientist, but they are for us certainly not the main material we are working with today. We have data which have been accumulating for about 2,500 years. Plato developed very interesting propositions on human behavior in society, and if we now developed models which somehow organized or clarified what Plato had to say, we should be doing an important job."

laws of physics, which are "true in themselves"). The theoretical sciences allow us to make arguments, according to Cartwright, while phenomenological sciences allow us to make explanations.

The burgeoning discussion of data science has engaged many topics, not least of which is what exactly "data science" is and whether it presents any novelty. It has questioned and challenged conventional views of data, but perhaps missing from the conversation so far is a serious consideration of what a "science of making knowledge out of data" essentially means. What kind of science would that be? It would seem to be one that tends strongly toward phenomenological rather than theoretical science. This means, firstly, that it contains a whole motley of changes to the nature of data: for instance, its higher resolution and granularity, its denial that anything is non-data, its continuous collection and updating, as well as its departure from tightly controlled methods of sampling. Data circulates more freely when we relax our assumption that it stands only as the representation of some specific slice of reality, typically the slice that it was gathered to represent. This does not mean that data (*ipso facto*) becomes theory-laden as a result; in fact, quite the opposite is true. Data that is not sampled according to tight specifications avoids a theoretical definition, which means that it can appear more in the form of the probabilistic distribution of *signs* in the world rather than samplings of a predefined phenomenon. The seeming paradox of "big data" is that the perspective it provides gets *smaller* as its volume gets *bigger*, which only makes sense should it have the effect of deviating from the aggregate contexts and categories that *we* (the analysts) have generated for it (Kitchin, 2014; Leonelli, 2016; Desai et al, 2022).

Each of these points is consistent with Cartwright's (1983) idea of phenomenological science. So too are they consistent with the "new experimentalism," or a model of science based on "intervening" which the late philosopher and historian Ian Hacking (1992) distinguished from a science based on "representing." "Experimentation has a life of its own" from this point of view, and takes place independent of high-level theory. This is a version of scientific knowledge that does not presuppose an elaborate theoretical framework; rather the ability to causally interact with a phenomenon is far better. Hacking coins the term "entity realism" to refer to the results.

While the conversation around data science thus far in sociology has tended to emphasize what we might call a *minimalist* version of it, we argue a much more important version of data-centric sociology can be retrieved from engaging it more philosophically. While computational sociology involves new pedagogy and a heavy dose of software training, it presents no essential challenge to the existing epistemic goals and practices of sociology. Hence, the sociological imagination and data science, including allied computational and cognitive sciences, can appear to "converge" with each other as compatible, even complementary pursuits (Foster, 2018, p. 151; see also Evans and Foster, 2019).

A more *maximalist* consideration of data science, however, has gone mostly undiscussed in sociology. This presents a version of data science that does verge on a philosophical reformation, because in some capacity, data science presents a new "type of science." An example of this version of data science can be found in Duncan Watts' (2014) claim that extending certain methodological techniques from data-centric fields (like out of sample testing) will likely reveal a deep incompatibility between data science with sociological explanations by revealing the latter to be mistaken versions of an elaborated common sense. To avoid this, Watts argues that sociologists should recalibrate their explanatory goals to focus on counterintuitive prediction.

Like Watts, we too believe that a maximalist version of data science should be accounted for in sociology. But unlike Watts, we argue that data science *qua* science is not subsumable to new methods adapted to high volume data sources. A maximalist treatment of data science is much more conceptual than that, and for a specific reason: because of how data science challenges entrenched notions of *probability*. When data leaves the place where the logical positivists and the 20th century rationalization of methods had placed it, it shows how across the sciences the question of what probabilities are aside from the actual frequencies that constitute a probabilistic pattern remains mostly unanswered. Probability has been turned into a technique with little attention given to conceptual questions at all. Yet, massively large datasets pose problem for the tried and true ways of "taming chance" (Hacking, 1991) using a "statistical model of data" that typically rests on frequentist principles (Suppes, 1962). These ways of modeling data are engineered to make frequencies stand out as significant typically by eliminating large parts of a dataset as random noise (e.g. non-data for the researcher's purposes).

But what happens when our "'*p*-values' become disappearing small" when so much resolution can be found in our data that *everything* seems to have some relation to everything else (Lee and Martin, 2015, p. 1)?[2] This leads some scholars to take flight to Bayesianism. Because we (the analysts) retain mastery over probability according to the Bayesian concept, we do not need to assume that our statistical models even contain probability. We therefore do not need to worry about things like significance tests. We can constantly update our models with new data as it collects and accumulates, and aside from metaphysically muddled matters like objectivity, all we need to be concerned with is whether new data makes us more or less confident in our prior beliefs (see Bartlett and Lynch, 2019; Western, 1998; de Finetti, 1974). While such a subjectivist approach has significant virtues, Bayesian probability vastly underserves the cartographic potential of data science.

The two most popular concepts of probability today would—frequentist and Bayesian—might appear like all that remains from the "probabilistic revolution" that unfolded between roughly the mid-17th century and the 1930s (Keynes, 1921; Carnap, 1950; Gigerenzer, et al, 1987; 1990; Stigler, 1986; Desrosieres, 2001; Golthorpe, 2007). Revisiting the history, however,

[2] Benjamin et al (2018) argue for reducing p-values for statistical significance from $P < 0.05$ to $P < 0.005$ in order to ward off dangers now made more threatening in an era of apparent data abundance, like p-hacking, multiple testing, and the proliferation of false positives.

reveals frequentism and Bayesianism essentially punting on a key logical issue in the calculation of probability, while another perspective, one far less well-known, did not.

This is the concept of *objective probability*, the main focus of which is not on epistemology but on *ontology*. While frequencies and prior estimates can give us grounds for credence, they tell us nothing about why probabilities are capable of changing our beliefs. What if probability cannot be found in either our models or in our data? What if, instead, we associate it with features of the world that can make "certain degrees of belief appropriate" to have (Rosenthal, 2012, p. 222)? For objective probability, phenomena independent of our state of mind or information create our expectations. While frequencies remain significant as a sign of probability, so too are expectations. But what they both presuppose are *symmetries* that can generate uncertainty in the link between initial states and outcomes.. In the different possible outcomes of a coin flip, for example, there is heads or tails. A probability function can represent the proportion (e.g. finite frequency) of these outcomes relative to each other. On a sequence of 10 flips, the coin might land 5 times heads and 5 times tails. How should we interpret this result? Does the 50/50 function represent simply the frequencies we have observed and therefore the probability of either heads or tails on the next flip? Does it represent our confidence in believing how often the coin will land heads or tails across all future flipping? Or does it represent something about the coin itself, and the dynamics of the flip, something that we can know a lot about but not *everything* about, and which makes the symmetrical 50/50 chance an often-realized possibility?

Probability began its life in the 17th century as a property of games of chance and was thus contextualized and localized to rolls of the dice or draws of the cards, and even providing a context suitable for moral judgment like justice and equity (Daston, 1988). When it becomes a sign of the future, probability does so as more of a phenomenological law, rooted in experience and learning from practice, that replaces the testimony of authorities (Hacking, 1975, p. 34; Foucault, 1994/1966,

p. 63). Over the course of the 19th century, however, probability increasingly lost these worldly referents as computation took hold of "avalanche of numbers" and population data accumulated on an unforeseeable scale. Eventually, probabilistic patterns and distributions came to be almost exclusively associated with frequencies.

Data science, we argue, deviates from this trajectory and reveals the contrast between subjective (or epistemic) and objective (or ontic) probabilities to be a red herring. By moving from statistical models into datafication and data-cartography focused on short-term prediction and effective intervention, data science rediscovered the learnability of objective chance. Its approach to modeling is to build simple models on a lot of data rather than theory-laden models applied to relatively small amounts of data. as rooted in experience rather than in the abstract formulas common in theoretical science. Often this rests on attention to difference-making that is highly contextual and experimental, as it seeks to engage with a phenomena by specifying causes or the proxy of causes, rather than fitting them under unified principles. Machine learning is experiential learning because the inferred knowledge is not explicitly programmed but instead acquired "by the program itself" from big data sets (Wheeler, 2016; Williamson, 2004). For us, all of this means that sociologists have not been making probabilistic inferences in the way that we can and should. Sociologists can draw from data scientific practices and methods; but a more important contribution, we argue, is the invitation to reframe what we mean by probability.

Amid the professionalization of statistics as a consequential form of "inference expertise" at the turn of the 19th century (Gigernezer et al, 1990, pp. 120-21), a data-centric sociological perspective sought to draw attention to its limitations, using objective probability to do so (Weber, 1981/1913; 2019/1921-22; Du Bois, 2000/1905). Something similar seems called for today as data science mobilizes as a profession in formation (Avnoon, 2024). Our relation to it should be less focused on new methods and applications, a focus which so far seems particularly strict and is accompanied by a worrying tone of various kinds of displacement, automation, and potential irrelevancy, than how sociologists have come to make inferences using data. Objective probability, we argue, is an intellectual tradition that would embrace big data for *probabilistic* reasons rather than those that may appear, to many, as simply a positivist rebrand with all the attendant problems.

In what follows, we first provide an alternate genealogy of probability, one which includes objective probability as a late 19th century entrant. We detail its core principles by tracking the appearance of objective probability as it was independently arrived at around the same time by the American polymath Charles Sanders Peirce and the German physiologist Johannes von Kries. Among those subsequently influenced by objective probability include Max Weber and W.E.B. Du Bois, and yet the concept has remained almost entirely overlooked in sociological thought. Our task in recovering objective probability is also explaining why it disappeared, which offers further lessons on what it would mean for sociology to allow it to return.

A Genealogy of Probability

From Pascal to Large Numbers

In Paris in 1654, when Blaise Pascal and his friend Pierre de Fermat were asked by the so-called Chevalier de Méré[3] to answer some questions about gamesplaying, they approached the task with a kind of mathematical confidence. Their interests were untypical. The distinction of math from morality was fuzzy, and neither Pascal nor Fermat gave credence to the modern difference recognized between objectivity and subjectivity. The Chevalier asked them what is reasonable to do during certain scenarios of gameplay. Pascal and Fermat responded with answers like: a ⅓ division of

[3] Elements of this story are surely apocryphal. For instance, the Chevalier was not a "Chevalier" (or knight in French). He was a commoner and writer by the name of Antoine Gombaud who happened to have gone to school in the small French town of Méré. It is not clear whether he was a gamesplayer, let alone a bad one.

pots would be "just" if the game is still to be perceived fair, even should it be canceled prematurely with one player in the lead. Central to the analysis, we can find Pascal and Fermat (1899/1654, p. 307) resting a great deal on a specific phrase: "*le hazard est égal*" (the equality of chances). If, regardless of what the chances specifically are, the gamesplayers have grounds for equally expecting them, then the game situation will be fair, because it will be equal relative to the game's objective chances. Thus, as Pascal and Fermat determine what is possible in a game of chance, they are led toward understanding closer details about the games themselves and what playing them must be like for a probabilistic analysis to apply. A game of chance has rules and a kind of gameplay. More specifically, however, it consists of various ways of making "equal chances" pertain across all the gameplay.[4]

Christaan Huygens (1714/1657, p. 5), writing shortly after Pascal and Fermat, argued that to play a game of "equitable chance" is to play a game that "works to no one's disadvantage."[5] The test of such a game is whether every player has the reasonable expectation that every possible outcome could happen to them *and* to everyone else who is playing. For Huygens, "expectations [are] equal when they could be fairly traded for one another." To calculate probability, on these terms, one would not concentrate on outcomes or results of games; they would focus on specifying (e.g., "putting a value on") expectations for *investment* in games: "by Fair gaming [players can] procure the same Expectation." For expectations to be equitable, Huygens (1714/1675, p. 5) adds, the game

[4] A similar approach is retrievable from "Pascal's Wager" (Pascal 1995/1657-58, p. 126) which also rests its case on the problem presented by equal chances. We cannot know what fate has in store for us after death; this is an objective fact about our situation. Pascal makes a recommendation on this basis. To wager on the existence of a deity with the power of the Christian God means to orient oneself toward certain chances, much like in a game of cards. To orient oneself toward the chance of this deity means to hold certain expectations—expectations that Pascal does not consider "subjective" in any contemporary meaning of the word. Pascal's approach to chance applies to possibilities that objectively exist: mortal agents cannot dictate what the afterlife is, objectively speaking, and any answer they give now can bear little truth; the evidence will always prove elusive. If the choice cannot be rational, then, it is still possible for it to be "reasonable" in the probabilistic sense that was Pascal's sole reference for the word; for the mortal agents in question, they can adjust their expectations to their chances.

[5] Here we cite the 1714 English translation of Hugyens' text *The Value of All Chances in Games of Fortune: Cards, Dice, Wagers, Lotteries & etc, Mathematically Demonstrated*, which had been in wide circulation in its original Latin version prior to this point. While there is no evidence that Huygens met Pascal or Fermat, he was certainly aware of their correspondence, having visited Paris in 1655.

itself must be structured in a fair way, and exist in a state of fairness, by ensuring that all possible success "depends entirely on Fortune." Only when these conditions apply could the results be characterized by probability.

These classical formulations provide a historical reference point for contemporary probability that is both unusual and familiar. We cannot find Pascal, Fermat, or Huygens making a clear distinction between probability and expectation, which would appear to make them much more like Thomas Bayes, who made his famous proposal only a hundred years later (1763). For Bayes, there are no absolute probabilities independent of expectation, which can be perfectly arbitrary in Bayes' view. Expectations carry a kind of neutral equality—everyone has them and they are inevitably subjective. Probability allows only for *a posteriori* calculation according to Bayes, so what we treat as signs that match our expectations are indicators of states of the world distinguishable from authoritative testimony. Yet for the classical thinkers, expectations could be objective if and when the game was characterized by equal chances. Through gameplay those probabilities would appear differently, but nonetheless whatever the chances are will dictate what players can believe, what they should invest, and what they will find fair and equitable.

According to Ian Hacking, probability emerges from these conversations as a "mutation in the Renaissance idea of the sign. It came into being with a frequency aspect and a degree-of-belief aspect. In the early days one could be indifferent as to the two directions in which probability might lead one" (Hacking, 1975, p. 45). Thus, in Bayes' formulation from 1763, expectation is treated as *sui generis* in relation to probability, and it can even be "computed" testimony dispensed and attested without an authority: "the probability of any event is the ratio between the value at which an expectation, depending on the happening of the event, ought to be computed, and the value of the thing expected on its happening." Yet, according to Jakob Bernoulli, arguing in his *Ars Conjectandi* published in 1713, probability estimates require a comparison of cases that seem to be observations of the same thing, making probability conditional on a frequency of observations as a sign of phenomena.

Hacking (1975) summarizes this line of thought by indicating that probability in its modern form had been first prepared by the concept of partial signs, ones for which signification is not definitive and thus always requires some interpretation. Probability lacks the "intermediary figure" (typically, an authority with *divinatio*) who establishes a similitude relation among events by testimony rather than interpretation. Equally possible cases are said to be objective: they are the sign of an underlying probability. Thus, "equally possible" (while still strictly speaking different) is an *epistemic* distinction that requires turning quantity into quality. For classical figures like Pascal, Fermat and Huygens, equally possible events exist within an enclosed space of rules. On these grounds, they did not claim to know *more* than the players; they simply made different inferences than they did, which could include inferences about events that none of them may ever experience. The difference, here, will grow into the distinction between objective and subjective probability in modern versions where the analyst of probability *does* claim authoritative knowledge over the players and even the action itself.

From Determinism to Frequency

For Pascal, Fermat and Huyghens, it is possible to learn probability by making inferences from the "rules of the game," localizing and contextualizing probability to whatever game we happen to be playing. For Bernoulli, we arrive at an understanding of probability that is rooted in what we *cannot* know. For Bayes, probabilities remain conditional assessments that are never definitive. A key entrant at this point in the genealogy is Pierre Simeon de Laplace's (1814/1820) philosophical essay on probability. Laplace was mostly summarizing what has become conventional wisdom at this point, he spent few words defending his claims, but he gives what for many is the canonical definition of probability, or at least would be treated this way thereafter. Probability is the ratio of "favorable to possible cases," according to Laplace; it must sum to 100, and the paradigm example of it is a game of chance..[6] That is all we need to know of the concept. Probability is more relative to ignorance than knowledge because of imperfections in human intelligence that mires us in having to guess at future events. If we can know anything at all about it there must be something determinate about the world (which *could* be known, but not by us). In Laplace's philosophy, because we (humans) cannot know all that could possibly happen, we have to use probability to deal with uncertainty that can never be removed from our epistemic profile.

There is a key ambiguity in this formulation, however, that arises when Laplace's (1814/1820, p. 370) tries to square the circle he has created. Laplace makes the Bernoullian point that an increased observation of "simple events" will help us inch closer to their "true possibility [as it] makes itself more and more known." It becomes "more and more probable that it will fall within limits which, constantly tightening, would end up coinciding if the number of simple events becomes infinite." But then Laplace adds a confounding phrase: "To determine the law according to which this possibility is discovered, we shall name it x … If we consider the different values of x as so many causes of this result, then the probability of x will be …" He then repeats the formula of the ratio "of possible to favorable" cases (1814/1820, p. 370).

Here, it seems, even an imperfect human intelligence can know that probability estimates are the *result* of something. We can know the sources of possibilities (e.g. why what has happened was possible *to* happen), and we can know this equally of every possibility that does happen. Even if

[6] As Laplace (1814, p. 323) put it: "The theory of chance consists in reducing all the events of the same kind to a certain number of cases equally possible, that is to say, to such as we may be equally undecided about concerning their existence, and in determining the number of cases favorable to the event whose probability is sought. The ratio of this number to that of all the cases possible as the measure of this probability, which is thus simply a fraction whose numerator is the number of favorable cases and whose denominator is the number of all the cases possible."

what has happened is highly variegated with lots of internal differences, it was all equally possible—we can find what will *repeat* across cases. Laplace mostly ignores the metaphysical or "objective" aspects of this in favor of the epistemic aspects of it. He moves quickly to the math and the data, after having identified *equal possibility* as entirely a knowledge problem.

Laplace never used the words objective and subjective. Nevertheless, "subjectivity" and "objectivity" would enter the language of probability not long after Laplace gave his lectures (Daston, 1994).[7] Denis Poisson, writing in 1837, used the French words *chance* and *probabilité* to describe the duality, with the latter referring to "the reason we have to believe that [an event] will or has occurred," and the former referring to "events in themselves and independently of the knowledge we have … [An] event will, by its nature, have a chance more or less large, known or unknown; and its probability will be relative to our knowledge, as far as it is concerned" (Poisson, 1837, p. 30-31). Meanwhile, his contemporary Antoine-Augustin Cournot (1843, p. v) is arguably the first to introduce the familiar meaning of the two terms to speak about probability, when he wrote that it was necessary for him to use "les deux épithètes *d'objective* et *de subjective*." For Cournot, "mathematical probability expresses a law that [natural] phenomena are subject to and whose existence does not depend on the scope or limits of our knowledge. Mathematical probability, taken objectively, is to be determined from experience" (ibid, p. 121). According to Daston (1994, p. 336), neither he nor the other probabilists "went so far as to identify probabilities baldly with frequencies" at this time.

In 1835, the Belgian astronomer Adophe Quetelet (2013/1835) would publish his influential treatise on "social physics," in which he took the mean values of a normal distribution and rendered

[7] The main catalyst was the non-scholastic rendering of the terms given by Immanuel Kant not long before Laplace's lectures. Notably, for Kant, "object" and "objective validity" were not the same concept, as the latter is associated with what is "synthetic *a priori*" as a precondition for experience. Further, Kant typically associates "subjective" with "empirical," as an extension of critical distinction between "pure" and "empirical cognition" (see Strand, 2021). Nevertheless, as Daston demonstrates, the "philosophical winds blowing from Kant's Konigsberg" carried a distinction far and wide, so that by at least the 1830s, associations of "subjective" with internal states of the mind and "objective" with external reality independent of them were commonplace (Daston, 1994, p. 334-35).

them concrete. Variable attributes somehow existed apart from a multitude of individuals—*l'homme moyen* (the average man) was born. It was the regularity of events, having been rendered properly quantitative, that so astonished Quetelet, and in his view, the only way to see these regularites, and the statistical physiognomy they conspired to create, is with a large enough number of observations.[8]

In Quetelet, we observe the first strong associations between frequency, objectivity, and epistemic factuality.[9] If Laplace would not predict, this was because our knowledge could only be post-hoc in his view. Quetelet saw the "avalanche of numbers" as an *invitation* to predict, no matter how morbid the subject matter.[10] The Englishman Henry Buckle would popularize this aspect of Quetlet's principles in his widely read *History of Civilization,* published in 1857.[11] The logician John Venn would soon thereafter describe probability strictly in alignment with frequency in his influential *The Logic of Chance*, where we can find him imagining a long series of proportions: "As we keep on taking more terms of the series we shall find the proportion still fluctuating a little, but its fluctuations will grow less. The proportion, in fact, will gradually approach towards some fixed

[8] The imagery of the body is taken quite literally by Quetelet (1849/1845, p. 178) in his non-reticence to shift from method to metaphysics: "[The social body] subsists in virtue of conservative principles, as does everything which has proceeded from the hands of the Almighty: it also has its Physiology… we find laws as fixed as those which govern the heavenly bodies: we return to the phenomena of physics, where the freewill of man is entirely effaced, so that the work of the Creator may predominate without hindrance. The collection of these laws, which exist independently of time and of the caprices of man, form a separate science, which I have considered myself entitled to name social physics."

[9] For instance, the "remarkable constancy with which the same crimes appear annually in the same order, drawing down on their perpetrators the same punishments, in the same proportions, is a singular fact" (Quetelet, 2013/1835, p. 6).

[10] "We might even predict annually how many individuals will stain their hands with the blood of their fellow-men, how many will be forgers, how many will deal in poison, pretty nearly in the same way as we may foretell the annual births and deaths" (ibid, p. 6-7).

[11] Buckle's book is sprinkled with the sort of statistical fatalism that Hacking (1990, p. 115) describes with the apt phrase "society prepares the crimes." The following kind of logic is found throughout Buckle's text: "... it is surely an astonishing fact that all the evidence we possess respecting [suicide] points to one great conclusion, and can leave no doubt on our minds that suicide is merely the product of the general condition of society, and that the individual felon only carries into effect what is a necessary consequence of preceding circumstances. In a given state of society a certain number of persons must put an end to their own life. This is the general law; and the special question as to who shall commit the crime depends of course upon special laws; which, however in their total action must obey the large social law to which they are all subordinate. And the power of the larger law is so irresistible, that neither the love of life nor the fear of another world can avail anything towards even checking its operation" (1857, p. 15-16).

numerical value" (Venn, 1888/1866, p. 164). Venn credits Quetelet with the idea that probability is a measurable property of the world.[12]

When Quetelet associated probability with objectivity, and in turn Venn picking up his threads and making probability indistinct from frequency, the recipe proved decisive and still does. For many at the time, however, it was controversial, because they had not become accustomed to assuming that large numbers did not pertain to single cases like a decision, an action, or a fated human life. The neo-Kantian philosopher Wilhelm Windelband aptly summarized the tenor of fear and paradox provoked by statistical regularity: "Soul, freedom of will and whatever became without further ado things that are simply not existent" (1870, p. 254). If crimes and suicides appear with statistical regularity, if we can use these facts compiled over the last several years to predict what will happen this year, what will stop us from annihilating the concept of moral freedom, which had been once considered so untouchable by the world (Neumann, 2019, p. 110; Lütcke, 2020; Porter, 1995, p. 23)?

Thus among certain (mostly German) observers at the time, this left probability *qua* statistics in a kind of purgatory. By "retaining a scientific worldview," Queteletian statistical facts put objective stock in the patterns it revealed and did not really pay attention to the significance of this as a challenge to moral freedom, which could safely become apparent only in single cases. But by not presenting "lawful" claims beyond simply pointing out regularities, it was not clear exactly what else statistics could mean: "what is the *object* of statistical reasoning?" (Neumann, 2019, p. 112). Laplace never worried about this question, and even if knew about the full brunt of statistical facts, he would not have worried about it. Quetelet thought he found an answer in the conception of a statistical fact, and Durkheim mostly agreed with him in the *Rules of Sociological Method* (Porter, 1995; see also Durkheim 1982/1895, p. 155ff). Yet, as Gabriel Tarde (1903, p. 114) noted around the same time,

[12] As Salmon (1981, p. 132) puts it, "'Since Venn is convinced that the only kinds of probabilities are frequencies, he objects to the notion of 'objective probability' as another manifestation of unwarranted *a priorism*."

for sociologists to hand metaphysical questions like this over to Quetelet would prohibit them from paying much attention to the single cases that must obviously comprise the "bell-shaped curve." According to Tarde, for a statistical average to be an object requires some account of the "imitation" we can see between the single cases it counts.

The Subjective and the Objective

By the early 20th century, to associate frequency with objectivity (no questions asked) was not unusual. Developments in statistics and math had made it standard practice. The axioms worked out by the mathematician Andrey Kolmogorov in the early 20th century appeared to make frequency the best kind of knowledge one can have of probability. Thus, it is not unusual to find frequentists in the early twentieth century being fully explicit in regarding probability *qua* frequency as safely objective, which meant fundamentally that unless you had observed a certain number of frequencies, you could have no credence at all in what you are claiming.

Richard von Mises (1964/1928, p. 19-20) used an example first proposed by Laplace to make this point. Suppose we are playing a game of cards in which we have to draw 14 at random, with each card featuring one letter of the alphabet. We do so and are astonished to learn that from our 14 randomly distributed cards the word "Constantinople" appears. For Mises, we would assume (correctly) that "something utterly improbable" has happened in such a circumstance. But why? Because, in Mises' view, randomly drawing 14 cards that yield gibberish occurs with far more frequency than drawing 14 that spell "Constantinople." Thus, within the collective known as "card-giving" the *lack* of frequency is an (im)probability, and for Mises frequencies are the only thing that could make that probability objective.

The biologist (and noted eugenicist) R. A. Fisher agreed with Mises' reading of frequencies as objective, arguing that, quite simply: "the concept of probability as an objective fact [is] verifiable by observations of frequency" (Fisher, 1934, p. 25). For Fisher, s anything unmeasurable by frequency is epistemically suspect at best: "It will be noticed that the idea that a probability can have an objective value, independent of the state of our information, in the sense that the weight of an object, and the resistance of a conductor have objective values, is here completely abandoned … [The] occurrence of events in the sense that the probability of an event can be identified with the probability of the truth of the proposition that the event shall occur" (ibid, p. 4). In this logic, then, frequency must be "reasonably thought of" as revealing "an objective probability, independent of the state of our evidence." Knowledge of frequency, "as it is increased indefinitely, would lead the probability inferred to tend to 0 or 1, that is to a statement *without* uncertainty" (Fisher, 1954, p. 45-46).

Mises and Fisher are the best-known of the advocates of an objective view of probability that makes probability just another word for frequency. As we can tell, their purview seems to have extended primarily in an epistemic direction. Frequencies are objective because essentially because they surpass whatever could be observe in a single case, which becomes, at least epistemologically speaking, an unjustifiable point of focus. Outside of observing sufficient dealings of the cards, we could not say that our getting "Constantinople" is improbable and "kcwszxqotpynml" is not. We need to *sample* the cards to be sure. Nevertheless, an assumption is made here that others seemed to notice at the time and which, as we argue, has become very apparent now: there is a locality to frequencies being the royal road to probability (and objectivity), and it is within the "collective," as Mises puts it, of the card game. But what constitutes that collective? How can the frequencies be frequencies of the same thing?

For Keynes, in his *Treatise on Probability* (1921), we can find him agreeing that "all probabilities must be based on statements of frequency," but he also insists on not finding "probability as being identical with statistical frequency." Rather, for Keynes (1921, p. 101), probability fundamentally consists of the "proportion of true propositions in a *class*." Specifically, how "truth-frequent" are some propositions relative to other possible propositions? And how can true and non-true propositions be equal in any way? According to Keynes, the "fundamental tenet of a frequency theory of probability is … that the probability of a proposition always depends upon referring to some class whose truth-frequency is known within wide or narrow limits" (p. 101). Thus, he regarded probability as "the degree of belief which it is *rational* to entertain in given conditions, and not merely with the actual beliefs of particular individuals, which may or may not be rational" (p. 4, emphasis original), and his meant that Keynes did not think that the knowledge one needed for probability could be grounded in probability alone.

The geophysicist Harold Jeffreys's book *Theory of Probability* was an early defense of Bayesian methods. In it, Jeffreys argues for a view of probability as a "reasonable degree of belief" and is thus always to be considered conditional on some prior possession of relevant information. In his words, "because $P(p|q)$ depends on both p and q it cannot be an objective statement, since different persons with different knowledge would assess different probabilities of p. … The probability of a proposition irrespective of the data has no meaning and is simply an unattainable ideal" (Jeffreys, 1961/1939, p. 406). His colleague E. T. Jaynes supported Jeffreys' approach, although Jaynes regarded objectivity as a possible goal of a belief orientation: "Our goal is that inferences are to be completely 'objective' in the sense that two persons with the same prior information must assign the same prior probabilities" (2003, p. 373). But Jaynes understood the difficulty involved in pursuing what he called an "objective Bayesianism," as "there is no single universal rule for assigning priors—the conversion of verbal prior information into numerical prior probabilities is an open-ended problem of logical analysis" (88).

If for Keynes, Jaynes and Jeffreys, frequencies could lead to probability in the form of more truth and reasonability, a more personalist approach to the same question introduces other psychological mechanisms in the space between information and belief. Thus, as the economist Leonard Savage argued, probability is more specifically a reference to "the *confidence* that a particular individual has in the truth of a particular proposition" (Savage, 1954, p. 3). Two people "faced with the same evidence may have different degrees of confidence in the truth of the same proposition" (ibid, p. 3). But only they can determine what the probability is "by interrogating [themselves], not by reference to the external world" (ibid, p. 51). Likewise, for the Italian actuary Bruno de Finetti, we cannot even conceive what probability is until we have put something at stake, which could include the truth of a proposition. A better indicator is to take a risk or *make a bet.* Any calculation of probability must be mathematically coherent, of course, but for Finetti all such a calculation can do is render explicit a quite personal state of mind: "We could be guided by the hope of a risky gain and risk everything, or we might prefer the modest tranquility of those who feel safe from the tricks of fortune. We are perfectly free with regard to this choice; everyone can do as he wishes. The probability calculus cannot say we are right or wrong" (de Finetti, 1989/1931, p. 195). Finetti (1974/1933, p. x) offers a bold proclamation on these terms (in all caps): "PROBABILITY DOES NOT EXIST" (see also Western, 1998, p. 34). He compares beliefs in the objective existence of probability to beliefs in "phlogiston, the Cosmic Ether … even Fairies and Witches … Probability, too, if regarded as something endowed with some kind of objective existence, is no less a misleading misconception, an illusory attempt to exteriorize or materialize our true probabilistic beliefs" (1974/1933, p. x). The only alternative is to allow "subjective probabilities [to] exist as the degree of belief in the occurrence of an event attributed by a given person at a given instance and with a given set of information" (p. 3-4; emphasis original).

Thus, no statistical artifact is *not* conditional on a prior estimation, which in more recent Bayesian applications gives "surprise" a distinct epistemic function (Evans and Foster, 2024; see also Parr, Pezzulo and Friston, 2022, p. 16-17). To "finally" assess a probability is meaningless because you can constantly change it by inputting more data. Scientific inquiry must prepare for "permanent learning," with theories simply updating as "conditional probability statements" (Western, 2001, p. 371; see also Lynch and Bartlett, 2019; de Finetti, 1974).[13]

From a Bayesian perspective, the probability of single cases can find a conditional probability statement that inspires more or less confidence, but we cannot say anything objective about the world. For the frequentist, on the other hand, if we cannot infer the world is probabilistic, we also cannot tell whether our own perceptions of it are based on probabilities or not. Any such assessment along those lines is limited by uncertainty and error in sampled data. A probabilistic model of how the data was generated relative to a "known probability procedure" cannot be entirely the product of unmodelled uncertainty (Berk, Western and Weiss, 1995). Only frequencies can provide certainty on the presumption that an infinite sample size of independent and identical trials will more or less yield the same result. For the Bayesian, *we* need probability to feel confidence (or lack thereof) in our beliefs; for the frequentist, *data* needs probability to not be entirely error-prone.

This dichotomy largely sets the parameters for discussions of probability today, which means that the notoriously slippery concept of probability is secured within pincer-like jaws. More commonly, we might say, it has been black-boxed. It remains unclear, in this sense, how "probability comes to us, and charges our models with something explanatorily vital." How can the "noise of the world" be smoothed into something stochastic like a "probabilistically patterned outcome" if we do not do it ourselves (Strevens, 2021, p. 14637)?

The present situation is not, in this sense, all that much of a departure from what Savage took note of in 1954, following a point raised earlier by the French physicist Henri Poincaré: "It is unanimously agreed that statistics depends somehow on probability. But, as to what probability is

[13] An epistemology of permanent learning puts the onus on more and more data-gathering, and more and more testing of prior model estimates. As one observer mentions: "it's not that we haven't learned anything, but rather that we'll never learn anything" (Phillips, 2019, p. 248).

and how it is connected with statistics, there has seldom been such complete disagreement and breakdown of communication since the Tower of Babel" (Savage, 1954, p. 12). For his part, Poincaré (1982/1903, p. 32) observed that "every probability problem involves two levels of study … the first—metaphysical, so to speak—justifies this or that convention; the second applies the rule of calculus to these conventions." The danger, for Poincare, arises when the rule of calculus we have designed ourselves comes to decide our metaphysics for us.

Randall Collins (1984, p. 331) took note of something similar in sociologist's strong preference for "statistics over words" when discussing probability. This results in the general assumption that the former is a metaphysically neutral method in comparison with the speculations of the latter, when in fact statistical models (especially the LISREL models of the day) constitute "substantive theory of how chance processes operate in the world." In more recent times, Andrew Abbott (1988; see also Abbott 2001) identified similar dangers as the "general linear reality" that often slips into sociological explanations. In this case, general linear models lend sociology *its* metaphysics and thus its strange version of the world, one in which only fixed entities with variable attributes exist and relate to each other in a monotonic casual flow—a world completely absent of sequences.

Both Collins and Abbott draw our attention to the pitfalls of limiting probability to a calculus, and so if we sociologists find ourselves in a situation where, as they both suggest, our preferred modes of probability calculus *have* become our metaphysics, then it would seem only a metaphysics incompatible with that calculus could sufficiently test it. Big data appears to thrust us into a very different world, one in which the meaning of significance changes. Our conventional ways of measurement and calculation become a drag on us in this world.

Yet on a different side of the family tree, along a different branch from Laplace, a different perspective lies. A cousin to both Bayesianism and frequentism, objective probability does not give into the 19th century tendency to deontologize chance. It does not relocate probability into statistical models by drawing it entirely out of the world. Neither, for that matter, does it give into the over-subjectification of probability by shifting it into states of mind or penchants for risk. Two probability theorists, who also happen to be near perfect contemporaries without any apparent knowledge of each other, can help us understand this omitted chapter in the genealogy of probability: Johannes von Kries and Charles Sanders Peirce.

Introducing Objective Probability

The meaning of "subjective" and "objective" that hovers over modern probability theory derives from a combination of Kant and Laplace. Yet, Kant actually had thoughts on probability, and he made different associations with objectivity. If, for Kant, "in the case of probability … the ground of holding-to-be-true is *objectively valid*," then probability is rooted in knowledge of an object as a "real possibility." For probability to be objectively valid means being able to make a "disjunctive judgment" in which two propositions are not related by "logical sequence" but because both "occupy a part of the sphere of possible knowledge; all of them together occupy the full sphere." What Kant elaborates here is what it means for probability to lie in the object itself, such that it can affect a theory. Probability must "lie in the object itself" for a theory to be confirmed by it.[14]

For those who held probability to be objective in this particular way, the probability toolkit could not be entirely subsumed by large numbers of observations. While the 19th century avalanche of numbers that seemed to make the world "teem with frequencies" (Hacking, 1990, p. 97), probability theorists like Peirce and Kries both took a more skeptical approach that sought to unlink probability from statistics, even if they did not deny a role to frequencies. For Kries, the inspiration

[14] Thus, to be explanatory, a hypothesis must loop into a "real possibility." Kant rejected the idea of the metaphysical hypothesis directed at the existence of God, say, or the soul. Both were objects that, as far as we know, have no real possibility. Theories about them cannot, therefore, acquire probability.

came from medical research which seemed to expose the limits of statistical inference relative to the expertise of seasoned physicians (Firoretti, . Peirce's interests were piqued mostly by his experience in astronomical measurement and coastal surveying. Additionally, both Kries and Peirce set off in pursuit of a novel concept of probability because of their skeptical view of psychological measurement, in particular, the version that had been proposed by Gustav Fechner in the 1860s and his hypothesis on "just-noticeable differences."[15]

Fechner's approach seemed to show that the perception of difference had a logarithmic relation to the world. In other words, our sensuous perception of potential differences in light, sound, heat (etc) is proportional to a logarithm of the *actual* intensity of a stimulus as measured with an instrument. The Fechner law was attractive to many because it seemed to show the capacity to notice differences had some kind of objective (e.g. "logarithmic") relation to the world, which we perceive as measurable *differences*. Peirce was so inspired by Fechner that he tried his own hand at psychophysical research, developing arguably the most experimental version of psychology of the time. Yet what Peirce found was a little different than Fechner. If, in Fechner's experiment, the best we can do in terms of registering differences is a Gaussian distribution, Peirce found that it was possible to actually improve our observations with our own "personal equation." This means we can *better* discriminate between chance occurrences and patterned ones and actually *narrow* the distribution with practice. Kries, meanwhile, argued that Fechner omitted an important point in formulating his measurement: in order to call our specific perceptions of different intensities as perceptions of the same phenomena, there must be some baseline that is equal or identical. We create such a baseline when we mathematically render the difference between particular sensations on a scale of more or less intensity. But, logically, this means that somewhere we will simply stop

[15] Fechner, it should be noted, is also the discoverer of the "Weber law" which he named after his mentor Ernst Weber, which also involved a logarithmic connection of perception to differences in the world. Give a test subject two boxes of slightly different weights and see if they can tell the difference. The difference between two pounds and 10 pounds is much more noticeable than that between 100 and 108.

noticing differences because what we are trying to notice really is the *same* here, consisting entirely of shared characteristics that will never be "just-noticeable" relative to each other. For Kries, the danger is that this baseline might only be a convention that introduces an arbitrary comparability and commonality between different phenomena.

For Kries and Peirce, Fechner's "law" had application to certain shortcomings in Laplace's influential view of probability. As noted, Laplace's mysterious *x* property refers to an objective source of probability but makes this solely an epistemological problem. Thus, if the source of probability is epistemic on this account, then a frequency count or a prior/posterior estimation can solve it. For Kant, however, the source of probability must lie in the object itself. Specifically, it must somehow be part of a whole that he refers to as an object's "real possibility." Kries and Peirce agree with Kant on this point, which (*pace* Laplace) entails that we human knowers are not always or inevitably in a state of indifference or ignorance about the world, which makes probability our epistemic choice of last resort.

For Kries, a deterministic relation is one that extends with full predictable continuity in time and space and thus everything could be known according to the initial conditions that are its extension. Thus, the law of determinism states that "every event which actually occurs is inevitably caused by the totality of previously existing events and circumstances." For Laplace, this necessarily applied, so it meant that probability could only be epistemic as an expression of our limited knowledge. There was no sense in which the relationships between events and circumstances featured *any* uncertainty in a deterministic universe. Kries followed Laplace in his ideas about the difference between the all-knowing "demon" and the ignorant human mind, which can know "laws" but can "never (neither *ex ante* nor *ex post*) achieve a such detailed knowledge of the conditions that a certain event seems to be necessarily connected with them" (Kries 2023/1886, p. 84). We are always limited to "the insight that the conditions (in so far as we know them) constitute a certain possibility which is more or less large" (ibid, p. 186).

Such "conditions" characterized only in an "overall, general" manner constitute a "Range" (*Spielraum*) that can (disjunctively) be divided into conditions that would affect the outcome of an event and conditions that would affect its non-occurrence (see Kries, 2023/1886, p. 24ff). Should we roll a dice, for example, the probability of any outcome is 1/6 because the part of the range that allows the roll of a number between 1 and 6 is the ratio of factors for and against a particular outcome. As Kries argues, those *Spielraum* to which our "probability statements" are the most valid are configured as games of chance. Every case, every event or outcome that occurs within them "are valid in all cases … and they are valid for everyone who wishes to form an expectation concerning the action in such a case" (Kries 2023/1886, p. 95). The validity of a probability claim is rooted in the fact that, with games of chance, even if "we do not exactly know the actions of the conditions in an individual case … we otherwise definitely know in which size ratio … the ranges of the configurations entailed in the realization of one or other outcome are to each other." In other words, for a game of chance, the ontological set-up is as such that whatever is equally *possible* is also equally *probable*. For other types of *Spielraum*, the two do not overlap; some possibilities are more probable than others.

For his part, Peirce arrives at an logically equivalent principle as Kries' Range principle as he elaborates upon dispositional traits (e.g. habits) as the object of frequency counts, making every single count feature its own kind of constancy. As Peirce puts it, we can say of a single die that it "has a certain '*would-be*'; and to say that a die has a 'would-be' is to say that it has a property, quite analogous to any habit." Yet, as Peirce points out, "this statement would by no means imply that the habit consists in that action—so to define the die's 'would-be,' it is necessary to say how it would lead the die to behave on an occasion that would bring out the full consequence of the 'would be'; and this statement will not of itself imply that the 'would-be' of the die consists in such behavior."

The would-be, in this case, is a *potential* that can be inferred, though it may be reserved and never find expression on any occasion. Nonetheless, it defines what is objective about the dice. To reveal it fully, according to Peirce, "it is necessary that the die should undergo an endless series of throws from the dice box, the result of no throw having the slightest influence upon the result of any other throw, or, as we express it, the throws must be independent each of every other." Although, for Peirce, a frequentist statistical artifact like this is not necessary for making an inference about the dice: it only stands as a sign for which probability is the *object* (not the other way around). Thus, the "would-be" serves as an inference to the parameters that a measurement attempts to reconstitute. This means, moreover, that what is revealed through a large number of observations is only a description of what *repeats* across each of them and that with knowledge of a single case we can structure our expectations about other cases.

In a wide enough Range (*Spielraum*) in Kries' terms, or a "continuum" in Peirce's words, everything can appear probabilistic, even existence itself; but this is a metaphysical hypothesis that requires entertaining the equal possibility that nothing might ever have happened.[16] On a universal scale this is pure confabulation, because far from being probabilistic, such speculations can only be deductive. For neither Peirce nor Kries does this mean we actually *learn* anything in this way. Nor, for that matter, do we learn through induction as simply the collection of particulars rather than repetitions (see Rorty, 2014/1963).

Thus, both Kries and Peirce allow for forms of probabilistic inference different from a calculated probability, but also logically distinct from deduction or induction. Peirce focuses on "abduction" while the comparable term in Kries (1916; 2023/1927) is "synchysis." In both cases, the logical form is more like a *loop* than deducing from first principles to conclusions or gathering

[16] For Peirce, "a continuum is a collection of so vast a multitude that in the whole universe of possibility there is not room for them to retain their distinct identities ... the continuum is all that is possible, in whatever dimension it be continuous … Thus, the question of nominalism and realism has taken this shape: Are any continua real? … [The] faithful nominalist … says no" (Peirce, 1992/1898, p. 160-61).

particulars to infer the future. With abduction, we revise a structured expectation based on a deviation and a further inference. Likewise, to form a concept through synchysis, we do not start with a definition and then go hunt for examples; we start from an example and attempt to find more. As Kries puts it, "each new item is evaluated with respect to the existing concept by means of a subsumption judgment based on an impression of belongingness," but as with abduction, this could lead either to a revision of the concept or the inclusion of a new example of it. Because they do not associate probability with statistics, Peirce and Kries connect their probability to this novel kind of logic. We can only get access to a "would-be" through the abduction, while a synchytic concept is the epistemic equivalent to a *Spielraum* or "Range" within which we can place scattered examples.

In an applied sense, then, probabilistic inference includes a kind of guessing, but in Peirce's words, it would only be on an infinitesimal chance that this would not reflect "the influence of phenomena." In both cases, the promptings are forced upon us; both Peirce and Kries acknowledge this debt to empiricism in arguing that perception has a non-representational content, and both thinkers associate this with "chance" (or *Chance* in Kries' words). This does not mean, however, that our perceptions are not interpreted; quite the contrary is true. For Peirce, "chance" only presents a kind of firstness that never characterizes our perception; for Kries, *Chance* (or *Chancen*) refers to opportunities or chances for drawing analogies. But just as Kant's notion of "reflective judgment" comes with a subjective feeling of the judgment and its continuity with the nature of the *sensus communis*, so too does, in Peirce's words, the goodness of fit of an abduction *feel* a certain way as a probabilistic inference.

> An Abduction is a method of forming a general prediction without any positive assurance that it will succeed either in the special case or usually, its justification being that it is the only possible hope of regulating our future conduct rationally, and that Induction from past experience gives us strong encouragement to hope that it will be successful in the future.

Though Kries pays much less attention to this, he does suggest that analogies drawn between separate cases can be more or less precise, and when they are really precise (as they are in, say, a game of chance) we do not have to (subjective) evaluate our expectations as much—we can have more *confidence* in other words (see Kries, 1916, p. 596).

Neither abduction nor synchysis relies on determinate rules, or on the apprehension of particular or "chance" phenomena entirely; this means that our certainty about them is strictly provisional. The implications here are quite different from Laplace's agnosticism, even while objective probability does overlap on several points with Bayesian probability. For neither Peirce nor Kries does our epistemology reflect our (inherent) incapacity to know of the *circumstances* that prompts us to guess. Rather, we can inquire about what makes our epistemic objects probabilistic and objectively contain chance, as a way of explaining why they can surprise us and why we can only learn about them via abduction and synchysis, which in agreement with Bayesians presents a non-terminal epistemology constantly open to revision. Peirce (1878, p. 293) puts this point clearly: "probability, to have any value at all, must express a fact. It is … a thing to be inferred upon evidence." A posterior estimate or a frequency count can only count as a *sign* of probability rather than being the probability. *Pace* Bayes' rule, then, any prior estimate that does not derive from objective knowledge will remain arbitrary: "antecedent probabilities … are not and cannot, in the nature of things, be statistical facts" (Peirce, 1902, p. 777).[17] To have probability hinge on our

[17] Both Kries and Peirce engage with a claim from Laplace to demonstrate their point. John Venn called it the "rule of succession," and it claims to assign a probability to an induction from past to future events. Suppose a series of events happens, and after *m* have happened, on *n* of them the event *E* did occur, and on (m – n) of them it did not occur. The rule of succession claims that on the next occasion in the series an event of type E will occur; it gives this probability as: $(n + 1)/(m + 2)$. Peirce gives the example of a non-visitor to the Bay of Biscay being able to say the same despite never seeing what they can calculate a probability for: "that is, if we put $m = 0$. In this case, the probability that it will rise the next time comes out ½ or, in other words the solution involves the conceptualistic principle that there is an even chance of a totally unknown event." For Kries, the example is the mineral composition of an unvisited celestial body, and how we can say there is a ½ chance that it contains iron or gold. The arbitrariness of the calculation arises because "the disposition of equally possible cases has to be established in a compelling manner, not in any arbitrary manner."

conditional estimations presents us with the metaphysically problematic "right to talk [as] if universes were as plenty as blackberries, if we could put a quantity of them in a bag, shake them well up, draw out a sample, and examine them to see what proportion of them had one arrangement and what proportion another" (Peirce, 1878, p. 300).

For Kries' part (2023/1886, p. 121ff), objective probability borrows an image from the physicist Ludwig Boltzmann and his famous "gas law," which had been discussed since the late 1860s.[18] This is the image of "a system of point-masses distributed in space" and what may be "anticipated as the state of such a system at a given point in time" (Kries, 2023/1886, p. 24; Treiber, 2015). If we imagine an initial "state space" as a space in the world constituted by temporally prior and posterior states and allowing for an array of possible outcomes, probability is the constancy of those initial states across "patches" at distal points in space and time (Rosenthal, 2012, p. 109). Every corresponding patch will include some of the initial states, but each patch will not contain *all* of them; thus, neighboring patches will include a different composition even as they are part of the same Range. From our starting point, these patches will objectively overlap and mirror each other, making them appear equally possible to us. But they will not all share the same probability.[19] A state space, according to Kries (2023/1886, p. 127), must demonstrate certain properties for a claim of probability to logically work. Specifically, the relationship between the states needs to be *original*:

[18] Notably, Boltzmann was himself inspired in in formulating his approach to the statistical mechanics of gases by the aforementioned Henry Buckle's *History of Civiization* and his extrapolations from Quetelet into rudimentary social statistics, particularly concerning "voluntary actions" like marriages, crimes and suicides (see Porter, 1986, p. 113-14).

[19] The gas law states that if we partition an enclosed space in half and then fill it with a certain gas so that one half of the space has more molecules in motion than the other (e.g. is hotter), they will tend to equalize over time after we remove the partition (e.g. transition to heat equilibrium). For Kries, "one always proceeds in the same way" to explain this. We do so by taking the action (or "kinetic movements") of gas molecules as the unit of analysis:

> Given the indeterminacy which is stipulated even for the initial state, arrangements that have the largest probability are just those constituted to transition to a uniform distribution. If the physicist characterizes this as a transition from an 'improbable' state to a 'more probable' one, it is clear he does not have in mind an expectation justified by some epistemological state. Rather he takes the word 'probable' in a different sense: an absolute one … it is the most probable state among all those which represent the largest class of original varieties of possible behavior (Kries, 2023/1886, p. 122).

The gas law, in this case, conveys an absolute probability as an objective statement of fact, but to know that we need to follow the action that transpires between what we designate as initial and subsequent states.

some of the initial states cannot be entirely reducible to earlier formations such that they can introduce a novel set of possibilities. Those states also must be *comparable*: they must all be states of the same thing with the same outcomes equally possible for them. Finally, they need to be *indifferent*: among the range of possible actions, any one trajectory is not contingent on any other.

What Kries describes here are the logical foundations of probability.[20] Originality, comparability, and indifference all signal the objectivity of systems capable of a probability estimate. More generally, Kries uses the phrase *Spielraum* or Range (what Keynes [1921] translates as "field" and other potential translations include "play-space," "lee-way" or "ontological action-space") in a manner that mimics what Kant describes above as "real possibility." Among other things, this means that objective probability is concerned with cartographic questions like what is probable *where*, how far in space and time does probability extend, and how do we case single events within the correct ("original") *Spielraum*. The Range principle still constitutes an epistemic intervention, but it assumes a cartographic meaning rather than one focused on subjectivist priority, as in Bayesianism. Just as if we observe the random movements of billions of gas particles, if we look too far away, we will find contravening factors that will make whatever we find or calculate arbitrary; but if we look too closely, we will end up looking at something different, with entirely different boundaries. The "Range" of gas molecules, in this sense, conveys the comparability of all of their movements, but this is not a result of how they are "bumping" into each other. Rather, their kinetic movement signals the objective space they are all in, and thus the molecules are resolving *our* uncertainty through their *action*. For Kries, the Range principle is still a principle; but the accuracy with which we apply this principle can *increase* with more knowledge of what we are applying it to.

[20] A later observer like Rudolf Carnap (1962/1950, p. 225) dedicated a lot of attention to Kries on exactly this point, ultimately disagreeing with his skepticism about "quantitative degree of confirmation" is largely inadmissible for probability because of "certain logical factors," but still recognizing that Kries was among the few theorists to push the "logical conception of probability" the furthest (p. 31).

For both Peirce and Kries, then, a calculated probability and an expectation are not two different things. One is not objective and the other subjective. For both thinkers, solving the Laplacean mystery epistemologically will generate a kind of probabilistic knowledge, but we should be suspicious of it. To claim that statistical measures are probabilistic is, strictly speaking, incorrect, particularly if this leads us to treat it inferentially, as a sign of the future. To avoid that risk requires giving up the assumption of inevitable ignorance. Peirce makes this point as follows:

> Laplace maintains that it is possible to draw a necessary conclusion regarding the probability of a particular determination of an event based on not knowing anything at all about [it]; that is, based on nothing. When a man thinks himself to know nothing at all as to which of a number of alternatives is the truth, his mind can no more incline toward or against any one of them or any combination of them than a mathematical point can have an inclination toward any point of the compass … Laplace holds that for every man there is one law (and necessarily but one) of dissection of each continuum of alternatives so that all the parts shall seem to that man to be "*également possibles*" in a quantitative sense, antecedently to all information. But he presents not the slightest reason for thinking this to be so, and seems to admit that to different men different modes of dissection will seem to give alternatives that are *également possible*s (Peirce 1960/1878, p. 764).

Peirce here aligns with a similar argument Kries (2023/1886, p. 5ff) makes a few years later where he will, likewise, point out the arbitrary foundation necessary for the numerical calculation of probability. According to Kries, in the widest ("universal") Range of all everything will happen, so there is no probability. This is Laplace's omniscient point of view, for which the initial states preceding every possible outcome can be known. But as we divide the universal Range down further and further, creating "disjunctions" in Kant's words, we move away from determinism and into probability. What could happen everywhere, only happens somewhere—but not all the time.[21]

[21] For example, while it is easy enough to calculate the chance of a meteor hitting some part of the Earth, the difficulty comes in the objective presumption needed to make the calculation (Kries 2023/1886, p. 6-7). How will we divide the surface of the Earth into parts so that, whatever specific places fall into each division, they have an equal possibility of being struck? Do we simply "[count] the names of the countries and seas"? If we ask someone "which part of the Earth's surface will be struck by a meteor," they might say Denmark instead of Mexico, or Timbuktu instead of Cape Horn (Kries 2023/1886, p. 6). But these names are meaningless for the calculation of probability, as a meteor is not oriented to territorial sovereignty (as we are). The task, instead, is to find the range that a meteor strike can be placed within, which is entirely contingent on objective knowledge about it, like its speed, its heading, combined with the same objective knowledge of Earth and its rotation.

Both Peirce and Kries introduce a nearly identical principle in their arguments, one which will play a substantial role in at least one version of sociology that comes out of this tradition. "Objective possibility," according to Kries, stands as a kind of logical displacement of frequency counts. It requires that we create a distinction in what we identify as initial states within a Range between those that are *ontological* and those that are *nomological*. The ontological concerns initial states that *are* actually alterable (e.g. variable), while the nomological designates those that are not.[22] In the set-up of a roulette game, variability comes from the movement of our hand as we toss the ball onto the spinning wheel. This makes it an ontological aspect of every possible result, as the source of action. Everything else about the game remains across the initial states and outcomes that bookend the moment of action (when the ball is tossed).

We cannot, therefore, assume that because we have nomological (law-like because it is constant within the space) knowledge, we can make a prediction. In Kries' terms, nomological knowledge merely concerns expectations of what is objectively possible. Thus, in his (Kries, 1888, p. 201) example, if a coach driver gets drunk before he drives a passenger in his coach, and if he then misses a turn, which leads a passenger to be late for his appointment, and if that passenger then gets struck by lightning, we could not say that the coach driver's drunkenness caused the passenger to get struck by lightning, as this whole antecedent sequence would not increase the probability of that happening. If, however, the coach driver rolls his coach in a ditch and kills the passenger, we could say that the antecedent conditions do increase the probability of that. Kries, here, argues against John Stuart Mill (1974/1843) and his logic of necessary and sufficient conditions, as "any theory of causation for which there is no other causal connection than the constant ordered sequence of A and B—which bases a causal connection only on an unexceptionable regularity of succession—will, ultimately, prove sterile" (Kries, 1888, p. 201).

[22] As Kries puts it, we would not say it is objectively possible for a body to immediately sink when placed in water, as this would involve "altering nomological determinants … by changing things that are actually inalterable."

For a Millisan, the above sequence finds a causal relation that has been "broken" or "counteracted" by another source of variability. The coach driver's drunkenness is not a causal factor of the passenger being struck by lightning, although it seems to be. It has been canceled by the lightning. In the Millsian logic, we can glimpse the ontology of model construction common in multivariate statistics: fixed entities with variable attributes, monotonic casual flow, the univocal meaning of attributes (Abbott, 1988). For Kries, on the other hand, a coach driver's drunkenness can kill a passenger; so too can lightning. The death of the passenger is within both of their Ranges, so we can make nomological arguments for these connections. Yet *ontologically,* they might do nothing to increase the probability of death in a single case. The constancy between antecedent conditions and outcomes does not apply as a probabilistic tendency across every single case, which the Range principle attempts to capture based on objective knowledge of the factors involved.[23]

For Peirce (see 1960/1878, p. 766), too, the problem we run into here, especially should we adhere to frequentist principles, involves finding probability in *single* cases. It seems obvious to him that probability is a single-case phenomenon, and that as much as we can make abductive inferences toward what would-be the case if certain things held constant in action, outside of single cases, our concepts cannot give us an understanding of practical consequences.[24] For Kries' part, "the probability of the general case must be set equal to the sum of probabilities of all the individual cases it contains" (Kries, 2023/1886, p. 6). Much like Peirce's famous declaration of the pragmatic

[23] As Kries puts it, "The specificity of the case lies only in the fact that, because of the very special and not foreseeable configuration, an action that is generally suited to the highest degree to lead to a certain outcome did not produce it. [...] No causal connection is interrupted, but the culpable act has not fully developed its causal effectiveness in the usual way, because of the special formation of the singular case" (Kries, 1888, p. 210).

[24] Peirce accounts for this by considering the single (mortal) human life. "If man were immortal," Peirce writes, "he could be perfectly sure of seeing the day when everything in which he had trusted should betray his trust, and, in short, of coming eventually to hopeless misery. He would break down, at last, as every good fortune, as every dynasty, as every civilization does. In place of this we have death. But what, without death, would happen to every man, with death must happen to some man … [D]eath makes the number of our risks, of our inferences, finite, and so makes their mean result uncertain" (Peirce, 1923, p. 72). Only some things will mark our experience as human individuals. Only in infinite time would we experience all possibilities. Whatever exists on a finite timeline, then, can neither be distinctly individual nor indistinctly collective; its existence can only be probabilistic. Probabilism is therefore most distinguishable for its dialectic between "definite arrangements" and "continuum of possibilities" that distinguishes "Chance [sic] … as an objective phenomenon" (Peirce, 1898/1992, p. 207).

principle that puts the onus on the "practical consequences" of our concepts, Kries here makes an analogous point about probability. Kries does not give probabilistic knowledge such a form that technical tools are necessary to access it. When Kries calls an account "adequate" it is because it has been tested by judgments that are *closest to action*. As observable in the ideal gas law, only in action does probability become observable. While we can form (nomological) expectations about what is possible to happen prior to observing action, those expectations are not explanatory.

For an objective probabilist, then, probability is not (*pace* both frequentists and Bayesians) founded on our subjective ignorance. It is instead founded on our objective knowledge. Objectively, Laplace's mysterious x refers to what creates possibility sets that are equal to each other, as expressions or actual manifestations of the same Range or "would-be." The imagery here is immediately cartographic. How far a Range extends, how impactful certain initial states are, and what objectively constitutes the background would-be of what we can observe and measure in the foreground—these are the novel questions and points of focus that objective probability introduces. Accounting for this perspective in the long arc of probability theory, it seems clear that significant, logical questions were overwhelmed by the avalanche of numbers soon after Laplace made probability epistemic. If the crude subjective versus objective distinction left behind is our inheritance of that (non)resolution, then thinkers like Kries and Peirce, independently of each other, tried to resolve the mystery using analogous solutions. As they did, probability escapes the epistemology cave, and we can finally recognize it as being only indirectly "objective" and "subjective" as the frequentists and Bayesians understand those terms. What Kries and Peirce arrive it is more classical than anything else: probability applies to spaces and games, which means that, much as Pascal, Fermat and Huygens might have themselves suggested had they not thought of "objectivity" in primarily medieval terms, it was possibly only an accident of history that has left us with a strongly epistemic association with probability.

The Puzzling Influence of Objective Probability

Pragmatism and Probability

For many, the efforts of those like Kries and Peirce were convincing enough to make probability conceivable outside of numerical formulation and statistical calculation, despite the latter's monopoly over inference expertise.[25] A probability calculus was *not* a statistical calculus for either Kries or Peirce; but what it requires therefore remains open to suggestion. Objective probability would travel from these arguments into sociology, psychology, philosophy, and economics via forking paths.

On the one hand, in the pragmatist tradition leading from Peirce forward, it has, arguably, not been appreciated yet just how deeply probabilistic it is. This is not only a reflection of Peirce, and his refusal to treat probability as strictly epistemic, but more generally his embrace of an ontological probabilism, which makes commitments that other and later pragmatists build upon in different ways. For instance, when William James (1896/1884, p. 149) argues for replacing "freedom" with "chance" in his argument in support of indeterminism, this is not an epistemic claim. As James (ibid, p. 151; emphasis original) puts it, chance means "pluralism," but more specifically it means that "possibilities are really *here*." In fact, James (ibid, p. 180) joins the two terms "chance-possibilities" to make his point, implying a similar kind of in-between actual and potential status as applies to Peirce's ontological probabilism, as centered on the "would-be" and objective possibility. This is how James avoids determinism, which is quite different from embracing strict indeterminism. Like Peirce, he does not admit of pure particular instances, unconnected from any potential or possibility, and thus completely unpredictable. Rather, a "pluralism" of chances in any given instance carries into his realism about possibility.

25 Alongside Kries and Peirce we could, in a lesser sense, place Kries' contemporaries Hermann Lotze and Wilhelm Lexis, in addition to those mentioned above.

In George Herbert Mead's (1926) approach to the philosophy of the present, the characteristic emphasis is on possibility as an ontologically real and present-time dimension alongside probability, which appears as action-relative. "Present reality is a possibility," according to Mead (1926, p. 83), "it is what would be if we were there instead of here. Through the mechanism of significant symbols the organism places itself there as a possibility, which acquires increasing probability as it fits into the spatio-temporal structure and the demands of the whole complex act of which its conduct is a part." Mead (ibid, p. 83-84) makes the important point that "the possibility is there in nature, for it is made up of actual structures of events and their contents." To view them as a possibility is to make a "mental or working hypothesis." But the uncertainty involved is only something that can be resolved "upon the accomplishment of the act" (ibid, p. 84).

The significance of putting a term like "hypothesis" into action, and of engaging with the terms possibility and probability in a non-epistemic sense, is arguably brought to its fullest expression in John Dewey's (1938; 1958) notion of inquiry. For Dewey, "ends in view" are dynamic predictions made in the thick of the situation. As an action situation, inquiry is not neatly parsed into the "objective state of affairs" that could be described with scientific precision by an external observer, and for whom prediction is appropriate, and the "subjective point of view" of the actor, and for whom, by implication, prediction does not apply, lest we "squeeze out" the creative, purely voluntaristic element. Instead, the "state of affairs" is, according to Dewey (1958, p. 100ff), irreducibly composed of an entanglement of what are typically classed as objective and subjective elements. The act of perception of a given state of affairs on the actor's part introduces such a subjective element. However, perception is not just purely spectatorial or contemplationist, but the "initial stage" in a dynamic action cycle. Perception is for something, in other words, and this something is anticipation and prediction.

As Dewey puts it: “the terminal outcome when anticipated (as it is when a moving cause of affairs is perceived) becomes an end-in-view, an aim, purpose, a prediction usable as a plan in shaping the course of events” (1958, p. 101). In a more concrete sense, perceptions are predictions, as “projections of possible consequences; they are ends-in-view. The in-viewness of ends is as much conditioning by antecedent natural conditions as is perception of contemporary objects external to the organism” (p. 102). Thus, objects appear as “potentialities” in this perspective, but this is not a subjective claim rooted completely in the instant that we perceive them. For Dewey (1938), an “organic interaction” with the world “becomes inquiry when existential consequences are anticipated; when environing conditions are examined with reference to their potentialities; and when responsive activities are selected and ordered with reference to actualization of some of the potentialities” (p. 107). But to be oriented to “potentialities” is not a “‘mental’ event.” Resolution of the indeterminate situation is active and operational (p. 107). Objects are “sets of qualities treated as potentialities for specified existential consequences,” but whatever potentialities they acquire does not mean they lose others; rather, it is as a *range* of potentialities (modeled on the form of a “would-be’) that objects exist (p. 129). For Dewey (1938, p. 296), a principal task of his approach to inquiry is to redeem probability as having every bit the same “logical status” as necessity has in an explanation, though only the former is deductive. A later observer of the trajectory of probability theory from Peirce forward noticed the same things in his framework, especially as Peirce drifted further away from frequentism.

Kries’ Diffuse Influence

For Arthur Burks (1963, p. 321), one of the parallel cases for the “*a priori*” or logical approach to probability found in pragmatism is observable in Kries’ approach. For Burks, this other approach comes by way of Keynes and his *Treatise on Probability* (1921, mostly written in 1907) which was the earliest, and for a long time the only, English-language analysis of Kries and the probabilism that comes out of the critique of Quetelet. Keynes (1921, p. 93-94) singles out Kries for demonstrating a probabilistic style of reasoning built around the firm, and in Keynes' view advantageous, position that probability is arbitrarily rendered into numbers and math. He reserves special praise for Kries' principle of *Spielraum* (which Keynes translates as "field") and how it disentangles probability from frequency counts. The impact on Keynes' own work and, through his introduction, on those within his Cambridge milieu at the time has turned out to be quite extensive in retrospect, though this is not the only line of influence (see Buldt, 2016; Heidelberger, 2012). Kries' objective probability is more weblike in its diffusion than the pragmatist tradition, but it similarly rests on extensions of a heterodox probabilism.

For his part, Keynes (1936, p. 135) uses the example of a beauty contest to illustrate the inherent link between expectations and objective sources of probability. To ask a sample of people to guess the beauty of people in a series of photos will reveal nothing about the probability of being beautiful. That is totally conventional, which means all we are measuring is the objective probability of the average idea of beauty. We can use the Range principle to infer that if this average idea exists, it must be because of an external perturbation, like the flow of information in the society, the structure of the media, and the structure of racial and gender inequity. Thus, Keynes argues against a numerical approach to probability by taking up Kries' suggestion that numerical probabilities arise only out of Ranges in which different cases appear as equally possible. For Keynes, if the only possibility for reaching an agreement on numerical probability is by sticking to a convention, then we certainly cannot claim objectivity to our claims. As Keynes (1921, p. 29) puts it, "where general statistics are available, the numerical probability which might be derived from them is inapplicable because of the presence of additional knowledge with regards to the particular case"

The philosopher Ludwig Wittgenstein (Hays, 2022; Heidelberger, 2001) took up the idea that probability refers to a "range," likely under the influence of Keynes who (it is widely suspected) introduced Wittgenstein to Kries' work (Fioretti, 2002). They both shared it with Frank Ramsey (1926), who adapted it to an early version of decision theory. For Wittgenstein in his *Tractatus-Logico Philosophicus* (1922, p. 123, 139) an interval of time exists between *p* and *q* in an *if … then* relationship ($p \rightarrow q$). The connection between these propositions, if it is to be true, must therefore remain within the same *Spielraum.* As Wittgenstein puts it, "The truth-conditions of a proposition determine the range [*Spielraum*] that it leaves open to the facts." For his part, Ramsey (1926, p. 158-59) takes the example of "the probability of recovery from chickenpox" in which probability is, likewise, bound up within the proportions of a "range." For Ramsey, the probability of "recovery from smallpox" can be rendered numerically as a frequency, but Ramsey specifies this in alignment with Keynes' adoption of Kries' principle of *Spielraum*. While the probability of recovery is a "degree of belief," a certain proportion of initial states (smallpox) remain constant with that outcome (recovery). Both Wittgenstein and Ramsey rejected frequentism for similar reasons. They both adopt a concept of probability more akin to objective probability than any other: it is a possibility set and a distribution of either truth-values or outcomes over some set of elementary propositions or initial-states. Probability, in this definition, renders logic "dynamic" as a relationship that *contracts* time and maintains something original over time (see Hay, 2022, p. 133).

Ramsey (1926, p. 173-74) would use unknowable circumstances spanning over a future period of time to identify the "quantity of belief" or "degree of certainty of belief" as part of the subjective value of goods or goals. The value of one good over another is not only a function of desirability but also belief in the probability of attaining it. In the 1950s, the aforementioned statistician and economist Leonard Savage (1954, p. 80ff) formalized Ramsey's approach, drawing it together into "expected utility," which has since become the cornerstone of revealed preference and mathematical rational action theory in modern microeconomics.

After Kries proposed his ideas, the German psychologist Carl Stumpf (1909) took a contrary point of view, embracing frequentism, but he would nonetheless agree with Kries that probability and statistics are two distinct problems (Kamlah, 1982; Benedictus, 2015). Stumpf, along with the philosopher Hans Reichenbach—who early on (1919) supported Kries' approach to objective probability—would influence the work of Edward Tolman (a student of Stumpf's) and Egon Brunswik, in addition to Kurt Lewin as part of Gestalt psychology.[26] Tolman and Brunswik (1935) would later propose that environments have a "probabilistic" texture, and this explains their externality and causality in relation to actors. Action in the world unfolds on the basis of "hypotheses." In this case, "the organism's task in any given case is to correct whatever hypotheses it brings with it to fit the real probabilities of the actually presented set-up" (Tolman and Brunswik, 1935, p. 75). Their point comes across as much akin to Kries in that probability objectively inheres in the environment and we establish a hypothetical (e.g. rooted in expectation) relation to it, as conveyed by our action. Brunswik would pursue these points further (1939; 1943) and argue that "environmental probability" is the primary source of the variation exhibited in action.

Less clear pathways from Kries include Edmund Husserl's phenomenology and, through it, Martin Heidegger's ontology (Lobo, 2019; Bult, 2015). Husserl (2001/1900-01, p. 147-48; 1993/1890-95, p. 271ff) engaged with Kries quite extensively, in fact, agreeing with him on the distinction between "nomological" and ontological" science, and using this to situate phenomenology in the first camp. The segue from Husserl to Heidegger largely turns on the notion of what is ontological, and Heidegger drew liberally on the idea of *Spielraum* for that purpose.[27] From

[26] Stumpf served as Lewin's advisor, while Brunswik worked early on with Karl Buhler, who was himself an assistant in Kries' laboratory, shortly after Kries published his *Principien* (see Bult, 2015).

[27] For example, in *Being and Time* (Heidegger, 1998/1927, p. 136 [145 in original]) we can find original phrases like: "*Der Entwurf ist die existenziale Seinsverfassung des Spielraums des faktischen Seinkönnen*." This is translated as: "Project is

there, we can encounter claims that also influenced Bourdieu in Maurice Merleau-Ponty's (2005/1945, p. 513-14) emphasis on finding a "phenomenological basis for statistical thought" and, less evidently, on Michel Foucault's adoption of some Heideggerian ideas, particularly *Spielraum,* in a more or less unadulterated way to define specific "clearings" or spaces that are neither determined nor undetermined, but which feature "lee-way" and open a "range of possibility" (see Nichols, 2015).[28]

From Planck to Popper

As Kries (2023/1927) points out, the theoretical physicist and initiator of quantum mechanics Max Planck actually proposes something closely akin to what he does in probability calculus. For Kries, theoretical physics often engages with his approach. In fact, in Planck's (1922/1918, p. 13) acceptance speech for the Nobel Prize in physics in 1918, as he explains the "elementary quantum of action" as one among two constants in the quantum understanding of physics, he describes the "'elementary region' or 'range' of probability'" necessary to give the kinetic movements of particles a "statistical treatment." He gives no citation for the quote, but if Planck draws from Kries, the latter cautions such an application: we should not mistake an outcome that is "maximally probable" from one we can expect to be "lawfully determined."

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the existential constitution of being in the realm of the factical potentiality of being." That last phrase strongly resembles Kries' earlier use of *Spielraum*, though Heidegger never appears to draw this connection. Nor does he in this passage from his important "On the Essence of Ground" (1998/1930, p. 133) essay: "Every accounting for things must move within a sphere of what is possible [*Spielraum*] … In accordance with its essence, such grounding always necessarily provides a given range of what is possible [*Spielraum*]."

[28] Nichols (2015, p. 253, see also p. 62ff) makes this point with a thorough re-reading of Foucault's oeuvre from the standpoint of the major influence on Foucault of Heidegger, which Foucault himself admitted both early and late in his career. The following argument, in particular, suggests the difference that the seeds of probabilistic reasoning could have made in Foucault's novel approach: "Foucault speaks in this idiom most explicitly … where he defined an "*ensemble pratique*" as a "homogeneous domain of reference," defined not by "the representations that men give of themselves" (a history of ideas or self-consciousness), nor "the conditions that determine them without their knowledge" (a structuralist historical materialism, for instance), but "the forms of rationality that organize their ways of doing things … and the freedom with which they act within these practical systems." Nichols argues that Foucault's "*ensemble pratique*" is essentially incomprehensible without some knowledge of *Spielraum*.

In the philosopher Ernst Cassirer's (1956/1936, p. 118) words, Planck's contribution marked a death knell of determinism (after its, in Cassirer's view, brief rein) and meant that statistical statements become "inexact" statements, as they cannot tell you about the "fate" of an individual particle; they cannot be statements referring "to an individual thing or event but to definite collectives." The historian and philosopher of science Gaston Bachelard (1984/1934, p. 125) suggested, at the time, that because the quantum revolution rested on "objective indeterminacy in all physical observation," it "sets limits to the assignment of realistic attributes" to particulars. The kinetic, chaotic shifting of gaseous matter, for example, appeared to offer definitive proof that scientific "terminology should reflect the fact that we are describing a collective and not an individual reality." According to Bachelard, scientific terms should not be received nominally or as substantial, referring to "individual objects … known by its position in space and time … understood to be separate and distinct entities" (ibid, p. 126-27).

We can see a convergence of perspectives that start with Kries and Peirce in objective probability. On the question of quantum mechanics and particularly, as Kries, Cassirer and Bachelard each alluded to, the Copenhagen Interpretation, Niels Bohr and Werner von Heisenberg deviated from Planck's "quantum of action." The philosopher Karl Popper would essentially rediscover Peirce and the pragmatist approach to probability in his attempt to devise a theory of probability that would be compatible with the Copenhagen Interpretation (see Suarez, 2013). Popper would call that approach "the *propensity* interpretation of probability" (Popper, 1957; 1959). Far from being a claim about the uncertainty of belief, "uncertainty" in quantum theory, according to Popper (1959, p. 26), refers to the objective uncertainty of "scatter-relations" in which "particles have paths, i.e., momentum and positions, although we cannot predict these, owing to scatter relations." To accept that point, Popper (ibid, p. 28) argues, we must accept something more radical: namely that "probabilities be 'physically real'—that they must be physical propensities, abstract relational properties of the physical situation," and thus what we take to be real objects are really just "propensities to realize singular events." Notably, Popper (1958/1935, p. 108) earlier had used Kries' "range" principle to develop a formal approach to his famous principle of falsification—referring to basic statements extending across a "subclass" and having their truth value come to reflect the "amount of 'free play' … which it allows to reality." Thus, in his words: "Range and empirical content … are converse (or complementary) concepts … [The] ranges of two statements are related to each other in the same way as their logical probabilities" (p. 108).

By confronting what he thought was a problem the new physics had introduced, Popper was led toward a heterodox objective probabilism compatible with neither frequentism nor Bayesianism. The proper understanding of probability, in his view, was lacking from these perspectives: "physicists make use of probabilities without being able to say, consistently, what they mean by 'probability,'" despite the fact that the quantum revolution had the benefit of nearly 300 years of thinking about probability to work with (Popper, 1958/1935, p. 134). A not dissimilar dilemma arises today, we would claim, across most fields as they become more data-centric, which means it is particularly acute in sociology. So why, at this earlier historical point—the climax of the probabilistic revolution—would sociology remain so "hesitant" (Du Bois, 2002/1904)?

American Sociology and Objective Probability

The early adoption of statistics by sociology largely filtered through the British line of development and occurred in the wake of Fisher, Karl Pearson, and Francis Galton's development of statistical principles (Camic and Xie, 1995; Platt, 1998). Accordingly, a key reason objective probability would be brushed aside in sociology is that in the Anglophone sphere, the field has always been marked by a commitment to frequentism (see also Keynes, 1921, p. 34). For some, this stands out given the popularity of Bayesian approaches in many allied fields for some time and its lagging absence in contemporary sociology (Lynch and Bartlett, 2019). Even when objective probability is glimpsed, it is typically filtered through the lens of variable sociology (for an early example, see Lazarsfeld and Obserschall, 1965; see also Parsons, 1948). An important reason why concerns the genealogy of the action concept in American sociology.

Action *sans* Probability

Roscoe Hinkle (1963) pointed out long ago that the "antecedents" to the action orientation in sociology prior to Parsons' *The Structure of Social Action* (1937) were profoundly influenced by pragmatism. In particular, as Hinkle (1963, p. 713) describes, Mead who was a key influence on other approaches at the time. Specifically, the relative "disorganization [of] features external" to actors meant that uncertainty characterized action, and in a theory like the one most commonly associated with W.I. and Dorothy Thomas, this means that situations had to be "consciously defined." Hinkle also emphasizes how these pre-Parsonian approaches to action did not have a similar principle as *Verstehen*; they instead drew attention to a rival concept like "life-history because it reveals the subject's own view of his situation, his train of experience or life movement" (p. 714).

When we get to Parsons (1937), "human action" appears in terms "of some form of the relation of means and ends." This means-ends schema has some fundamental "normative" implications. Mainly, given that we know what end an actor hopes to achieve, we can use the "norms of rationality" to determine whether their choice of means is optimal to achieve that end, with rationality itself being defined, in the classical utilitarian tradition, as the optimal choice of means (given an exogenous standard such as minimizing costs) to achieve a given set of ends. For Parsons, "this normative aspect characterizes the concepts of all the sciences dealing with human conduct." Therefore, "there seems to be no evading the fact that the subjective analysis of action involves in some form the schema of the means-end relationship" (Parsons, 1937, p. 284).

According to Parsons, there is an indelible link between the development of modern science and the emergence of such "norms of rationality" to evaluate action, which could only be judged to be rational to the extent that they approximated the norms of reasoning that have been institutionalized in post-enlightenment (western) science. This included criteria related to how we may generate new beliefs that correctly follow from ones we already have (e.g., logical rules of reasoning such as *modus ponens* and *modus tollens*). Also included are rules of belief fixation and belief formation (e.g., those that exclude what have come to be known as "Gettier cases" in contemporary analytic philosophy; see Hetherington, 2016). Essentially, actors are rational when they "test" their beliefs against material evidence the way a scientist would, while also ruling out improper ways of forming beliefs. In this last respect, "the simplest and clearest type of rational action involves in the first place what I may call an 'empirical' end—that is an end whose reference is to a future empirically observable state of affairs" (Parsons, 1990, p. 320).

For Parsons, prediction was only relevant in means-ends action schema for those possible future events that could occur but whose means of occurrence did not involve the actor's direct agency. For instance, an actor may be interested in predicting whether it will rain or whether a volcano may erupt. This type of mundane prediction is only relevant within the pursuit of pragmatic or concrete ends. A surprising implication of Parsons's reasoning here is that, when it comes to the pursuit of normatively defined ends, prediction is not a relevant aspect of action.

For Parsons, the element of prediction needs to be dissociated from the idea of the ends of action because failure to do so will result in a reduction of "value element" to the non-subjective conditions of action. As Parsons notes (1937, p. 248ff), the idea of an "end" is ambiguous between two dominant meanings. On the one hand, the "end" of the action can be thought of as "the subjective anticipation of a desirable future state of affairs toward the realization of which the action of the individual in question may be thought of as directed." For Parsons thinking of ends in this way implied two things. Ends defined in this way presumed that people strive to reach concrete states of affairs. According to Parsons (1937, p. 250), when ends are defined in this "concrete" way, it is easy to see the scenarios they depict split into two components. Those elements of the concrete end that involve events that would have happened regardless of whether the actor did anything or not (independent of action) and those elements that could be (e.g., counterfactually) attributed to people's actions. Concrete ends entailing an element of (scientistic) prediction do not count as outcomes on which action has an independent effect. In other words, I can predict lots of things about the world that do not involve me and my actions; however, normative ends become factors in action only to the extent that I begin to foreshadow states of affairs that differ from those predictions that do *not* involve my actions and to the extent that I begin to prefer one of those over the other. As Parsons (ibid, p. 251) notes, the ends of action can be defined as "alterations" from a prediction of what the future would look like had we acted as desired versus a future in which we did not act at all.

Ends are thus conceived as future states of affairs in which actors see themselves altering the impersonal flow of events, not just passively predicting their inexorable occurrence. If action were purely based on epistemic predictions of causal contingencies, these would squeeze out and exclude what Parsons thought of as the creative or "voluntaristic" element of action: "[m]ere prediction of the probable consequences of proposed alternatives is not, however, a sufficient basis of choice. It is, in addition, necessary to have criteria on the basis of which one alternative is preferred to another" (1937, p. 53). Prediction, conceived in an entirely scientistic manner, which is all that it can be from this point of view, is antithetical to agency and creativity in Parsons's world.

Thus, by the time Parsons and Shils (1951) attempt their defining statement for the theory of action, actors can only be oriented to definable "objects" (both social and nonsocial).[29] Nothing

[29] At the very least, however, Parsons and Shils (1951, p. 5) include an extensive account of "orientation" (and "learning"), which seems to have been subsequently lost from the theory of action. Hence, "a specific combination of

made them coexist with probability; they appear, for that matter, certain and definite—a far cry from the "scatter-relations" that were preoccupying Popper (1959) around the same time. It wasn't Weber but the cultural turn that would make action into the site of subjective meaning, like a "text" to be read (cf Ricouer, 1973). The effect is to withdraw probability further from the action on the assumption, seemingly derived through analogy to statistics, that the order and regularity that sociologists are trying to understand stands in contrast to a fundamental chaos. Thus, we can only observe meaningful order because of the key prior that we can always safely assume: namely, the "contingencies of interpretation" (see Geertz, 1973, p. 46; Berger and Luckman, 1966, p. 51; see also Alexander, 1988, pp. 312ff; Reed, 2019, p. 32).

Yet, like any good story, this one comes with a twist. The tale of probability and meaning, specifically their strict departure from one another, in sociology could have unfolded very differently, and likely would have, if the interpretation of probability as a statistical measure had not been secured as it had been by the early 20th century. This particular twist in the story involves translation.

A Tale of Translations

Parsons was brought on to help the economist Alexander Henderson (who himself became involved on the recommendation of Friedrich von Hayek, after Hayek was forced to pass) translate Weber's *Wirtschaft und Gesellschaft* in the original four chapter format that Weber originally intended for the text. As he went through the original German-language version, Parsons could not help but notice how frequently Weber uses the word *Chance*. What Parsons could not have known is that here we can find a direct descendent of Kries' objective probability (see Turner, 1983; Strand and Lizardo, 2022;

selections relative to … objects, made from among the possibilities of selection which were available in a specific situation, constitutes an orientation of action for a particular actor."

Tribe, 2019; Dahrendorf, 1979).[30] Parsons pivots away from the term based on what he expects will be a deep confusion in understanding the text. Thus, on page 100 of Parsons and Henderson's 1947 original translation, while still in the first chapter after an 86-page introduction written by Parsons himself, we can find the following footnote:

> This is the first occurrence in Weber's text of the term *Chance*, which he uses very frequently. It is here translated by 'probability,' because he uses it as interchangeable with Wahrscheinlichkeit. As the term 'probability' is used in a technical mathematical and statistical sense, however, it implies the possibility of numerical statement. In most of the cases where Weber uses *Chance*, this is out of the question. It is, however, possible to speak in terms of higher and lower degrees of probability. To avoid confusion with the technical mathematical concept, the term 'likelihood' will often be used in the translation. It is by means of this concept that Weber, in a highly ingenious way, has bridged the gap between the interpretation of meaning and the inevitably more complex facts of overt action (Parsons and Henderson, 1947, p. 100n21).

Significant about this footnote is just how much of Weber's original text will need to be modified, in most cases substantially, if one follows this rubric, not to mention that Weber would have known that Kries distinguishes between these terms and does not use them interchangeably. Nevertheless, Parsons and Henderson make the entire text to fit this mold.

The sentence to which Parsons and Henderson append this footnote is, in their version, translated as: "On the other hand, even the most perfect adequacy on the level of meaning has causal significance from a sociological point of view only in so far [sic] as there is some kind of proof for the existence of a probability that action in fact normally takes the course which has been held to be meaningful" (Weber, 1947/1919-20, p. 100). The new translation of *E&S* by Keith Tribe, which does not follow their rubric, renders the sentence very differently as: "On the other hand, even the most evident meaningful adequacy has significance for sociological knowledge only to the extent that a correct causal statement can be given—as proof the existence of a (specifiable) *Chance*

[30] Dahrendorf (1979, p. 62) mentions the "more than one hundred places" in the first part of *E&S* that Weber "uses chance as a word or a category." Though, he adds, "one cannot but be surprised about how little attention the literature on Weber has paid to the term." Keith Tribe, the most recent translator of the text, remarks upon Weber's "relentless use of *Chance*" (Tribe, 2019, p. 65).

that action does tend to follow an apparently meaningful course with specifiable frequency, or something close to it (either on average, or in a "pure" case)" (Weber, 2019/1919-20, p. 88; italics in the original). Parsons and Henderson assume, in this case, that probability must be numerical and epistemic—by implication, it seems, they seem to safely assume that it requires a frequency count for "proof"—or it cannot be relevant for sociological knowledge. "Likelihood" assumes a lower grade form as mere subjective probability that can keep the sentence whole despite Weber's apparent conflation between "meaningful adequacy" and probability. Such a view is limited because it assumes that any relationship between meaning and *Chance* must be a conflation. But Weber is not conflating concepts here, because he is working with a different understanding of probability than Parsons and Henderson can conceive.

Bracketing "meaningful adequacy" from probability serves the Parsonian effort well, however, and has surviving consequences to today. Because Parsons secures grounds for meaningful adequacy independent of probability, the adequate explanation of social action can be given over safely to "culture concepts" in various guises (Lizardo, 2019; Kuper, 2000). We must appreciate this as being in *lieu* of probability, which is itself given over entirely to statistics. This allows Parsons, and those after him, to posit all sorts of things in the space now reserved for subjective meaning severed from any relationship to probability. According to Tribe (2019, p. 78n8), Parsons used his translator's discretion to craft the phrase "subjective meaning" mostly himself. Weber would have found "meaning" constantly prefaced by "subjective" to be a redundancy. Nevertheless, this theoretical portmanteau is largely what has, thereafter, sustained internalized values, beliefs, "webs of significance," worldviews (etc) as definitional components of social action. More generally, it allows "interpretation" to become distinguishable from "strategization," as the former alone concerns highly contingent mental states like typification and classification, whereas the latter demonstrates a fixed rational choice (Alexander, 1988). We must see this, however, as ultimately a by-product of the rigid designation that Parsons gives to probability when he comes to translate Weber's key text in the 1940s, and which he in turn lends to Weber himself; an understanding of probability that has been completely narrowed to a "technical mathematical and statistical" method.

A second mediation of objective probability into sociology through Weber comes by way of the phenomenological sociologist Alfred Schutz. Schutz's phenomenological rendering of Weber's interpretive sociology, which conventionally and particularly in the 1960s and 1970s with the development of ethnomethodology, was touted as an alternative to Parsons in post-functionalist action theory (Heritage, 1984). Yet, like Parsons, Schutz ultimately falters on the issue of probability, making the phenomenological departure from Parsons much less radical than traditionally thought.

Schutz had attended Weber's lectures during the summer of 1918 in Vienna. While these lectures' contents are not yet fully known, Schutz would shortly thereafter engage in a critique of what he saw as shortcomings in Weber's sociology. For Schutz, this would subsequently lead to an intensive study of Husserl, eventually culminating in Schutz's first book (1967/1932), outlining the principles of his social phenomenology. Notably, in this book, Schutz might be the first "Weberian" (or self-described Weber-influenced) scholar to comment directly on Weber's probabilism and connect it to objective probability as proposed by Kries.

In his discussion of the idea of causal adequacy in the context of Weber's probabilism, Schutz (1967/1932, p. 231) acknowledges that "the concept of causal adequacy was first advanced by the physiologist Johannes von Kries in connection with certain problems in the calculation of probabilities. He aimed to contribute to the theory of legal accountability in criminal law, but he introduced the idea as a general concept independent of any specific application." Schutz was aware of the primary source of Weber's probabilism, but nevertheless, he demonstrates a limited understanding of the relevant concepts and radically underestimates the overall influence of objective probability in Weber's sociology. Schutz seems convinced that Kries influenced Weber only concerning his conceptualization of the idea of causal adequacy, bypassing the overall influence of Kries on Weber's conception of *Chance*. As Schutz reads and critiques the original German-language *E&S* edition, he does not ignore Weber's "relentless use of *Chance*." But he calls Weber out by accusing him of a fundamental ambiguity, which Schutz emphasizes with what we can assume is a frustrated exasperation (italics original): *"For whom does this probability exist—the actor, or the social scientist who observes him?"* (Schutz, 1967/1932, p. 151-52).

For Schutz, probability appears to carry an exclusively epistemic meaning, which for him means that probability only exists if an actor (or analyst) can recognize and presumably calculate it. Schutzian "objective probabilities" are, therefore, the subjective probabilities imputed by a third-person (social-scientific) observer to others' actions (Schutz, 1967/1932, p. 153). For Schutz, the notion of objective probability as externally existing chances independently of any observer is utterly unimaginable; instead, "[o]bjective probability … is a category of interpretation" (ibid, p. 237).

Neither Parsons nor Schutz present irrelevant arguments, because they still reflect how, for better or worse, sociologists understand the relationship and division of labor between prediction, probability, culture, experience and interpretation today. Despite their influence, however, and the distance he draws between action and prediction, later social theorists who have attempted to probe the inner core of action have not neglected probability. Jeffrey Alexander (1988, p. 315), for example, references Keynes' *Treatise on Probability* (1921/1907) in his later development of action theory, and he finds Keynes' approach significant for how it implies that "calculations about probable consequences depend upon knowledge of the future." This knowledge is inevitably "sketchy," as in no circumstance can the future be definitively known. From his reading of Keynes, Alexander agrees that "probability … cannot be calculated in a mathematical way" (Alexander, 1988, p. 315). Thus, the calculation of the future is inevitably limited from the perspective of action. But for Alexander this only places a stronger emphasis on "adequacy at the level of meaning," which for him means that, even in the highly institutionalized circumstances of, say, a corporate investment decision, interpretation will still underlie the strategic action.

Thus, the common idea that actors do not (or cannot and *should* not) predict and that sociologists can (and, according to some [Watts, 2014] *should* or *must*) predict goes hand in hand with the now well-established presumption that when actors predict, they do so poorly (Tversky and Kahneman, 1973; Kahneman and Tversky 1973). From Parsons and Schutz to contemporary behavioral economics, the only way to accurately predict is as a frequentist does, on the presumption that correcting errors in data reveals an objective pattern that would, presumably, hold for an infinite number of cases. To make actors the predictors would also reject an essential condition of their autonomy by giving the same emphasis to "knowledge" in action that Parsons, in particular, sought to avoid by emphasizing values in action (Parsons, 2010/1939, 114-15; 1977, p. 127; Alexander, 1978, pp. 180-81).

Since Anglophone sociology has remained mostly untouched by objective probability, this makes the choices mentioned at the beginning, those that arise at the prospect of a data-centric sociology—between interpretation or prediction, and between a theory of action or data analysis—appear as *forced* choices. It is easy to presume that a data-centric sociology necessarily entails less interpretation, less concern for action, and a diminished role for theory (Watts, 2014). This apparent rule has its exception, however, and we can find it in an important antecedent to Parsons that Hinkle (1963) fails to mention.

Du Bois and the Probabilistic Theory of Action

W.E.B. Du Bois appears to be the only figure who straddles both traditions of objective probability. Studying at Harvard as an undergraduate, Du Bois was strongly influenced by James (Lewis, 1993, p.

91ff). During his two year (1892-94) sojourn in Berlin, Du Bois spent much of his time in close contact with Gustav von Schmoller and Adolf Wagner, both of who came from the same milieu as Kries in reacting to Quetelet and statistical law and finding it unclear exactly what the "object" of statistical reasoning was (Neumann, 2019). For Schmoller in particular, the solution was clear: there simply was no statistical law, because there could be no material cause of action; there could only be ethical causes of action.[31]

The themes we can find in Du Bois' seminal "Sociology Hesitant" from 1905 (which remained unpublished until 2000) anticipate much of what Dewey (1938) will later discuss in the context of inquiry as putting prediction into action, specifically *when* this happens, which in Du Bois' own novel characterization of sociology means attempting "to measure the limits of Chance in human action" (2000/1905, p. 44; 1978/1904, p. 53; 1944, p. 57-58). His titular emphasis on "hesitancy" reveals an approach framed by its similar focus on action. Du Bois recommends adopting such a focus hesitantly in a way that does not commit to Comtean positivism or Quetelet-style "statistical facts." To recognize probability in action means devoting the lion's share of analytic attention to emergent patterns, particularly those of such persistent realization that, in Du Bois' view, they can faithfully be categorized as "Laws," but not at the expense of the partiality of Chance. For those attuned only to the law, events of this sort will appear unpredictable, yet it is not as if they are impossible (Du Bois, 2000/1905, p. 38; see also Itzigsohn and Brown, 2020, p. 53). To measure the limits of Chance is to commit to probability as both objective and non-epistemic, and to make such inferences requires nothing like a frequency count. It requires something more like a concentration on the joint distribution of both law *and* Chance.

This leads Du Bois to make a substantive claim about free will and Chance that echoes James and is phrased in the context of determinism:

[31] Schafer (2001) contends that Schmoller's historicist and moral critique of statistics "left a deep imprint" on Du Bois.

> Protagonists of "free" will are found to be horrified deniers of "Chance." And strenuous defenders of orthodox Science are found talking as though the destinies of this universe lie largely in undetermined human action—indeed, they could not avoid such talk and continue talking.
>
> Why not then flatly face the Paradox? [Why not] [sic] frankly state the Hypothesis of Law and the Assumption of Chance, and seek to determine by study and measurement the limits of each? (Du Bois 2000/1905, p. 42).

In advocating to bring the 'Hypothesis of Law" and the "Assumption of Chance" together in a kind of ratio, Du Bois, here, echoes James (1896/1884) in the latter's emphasis on the present moment being "real as possibility," but the distinction he draws appears fully consistent in its application with Kries' distinction between the nomological (e.g. "hypothesis of law") and the ontological (e.g. "assumption of Chance") as distinguishable components in single-case probability. Thus, Du Bois takes a position (as no one else does) at the convergence of both traditions of objective probability. This becomes even more apparent once we consider that "human action" was a kind of life-long interest of Du Bois, and in later years he returned to much the same language as he used in 1905 to refer to what, for him, defined his research as a sociologist.

In an unpublished piece from 1946, Du Bois argues that the "outer world" remains contingent on our "assumption" that it exists; moreover, we relate to what is outside of us (the world, other people) on the basis of "hypothesis," which we will maintain so long "as it works."[32] If Du Bois (2000/1905) contrasts Chance to law in a kind of proportional sense, then both are involved in any single-case instance, and because of this, we cannot deny the lee-way or action-space that applies even in seemingly lawful contexts.[33] Du Bois both in 1904 and, retrospectively, in 1944

[32] In this unpublished sketch (1946c), we can find an early version of a book Du Bois tentatively entitled "Prolegomena to a Science of Human Action" and he includes a chapter outline, with several indicative titles: "The Working Hypothesis," "The Assumed Truth," "Welcoming Error and Rearranging Hypotheses," "The Limits of Chance." We would expect these for a book on statistics rather than one on human action.

[33] A good demonstration of this from Du Bois' (1935, chap. 4) own work would be his analysis of "The General Strike" in *Black Reconstruction*, where Du Bois draws attention to the fact that "walking off the plantations" was Chance in the world of Law in antebellum south, it remained completely unexpected for all parties involves, except for those who did it.

characterizes his sociology as an attempt to "measure the element of Chance in human conduct" (Du Bois, 1944, p. 58). In 1944, Du Bois views this as a return to James's pragmatism and a turn against the German critique of Quetelet (as represented by Schmoller). Nevertheless, he is conscious of playing them off each other, knowing that the pivot from one to the other is not as fundamental as it might seem.

Du Bois' probabilistic approach indicates that this "turn to Chance" was done independently of, though indirectly aligns with, Weber's (2019/1921-22; 1981/1913) own slightly later turn to *Chance* via Kries, though only Du Bois had the benefit of participating in the American wing of objective probability that started with Peirce. If the chronology Du Bois gives in "Sociology Hesitant" is about right, then we could have both Du Bois and Weber turning toward objective probability at about the same time independently of each other, with Du Bois misreading this later on as a turn against Weber, when it was, in fact, an unwitting turn *toward* Weber. Because of the omission of objective probability, however, this might appear as heterodox speculation at best, or word salad at worst, about a version of probabilistic inference that seems hardly interested in statistical inference. How sociological explanations have historically been considered "adequate" demonstrates the most distortion in not seeing these two claims as a contribution toward the same thing.

Concepts for a Probabilistic Sociology

In the standard 19th century version of the story (Hacking, 1990), chance is "tamed" as it becomes known, and it becomes known when a sufficient accumulation of "large numbers" of observations allow the tools of statistical inference to find non-arbitrary connections between statistically defined independent and dependent forms of variance. The goal is to prove the marginal likelihood that these relationships are not due to chance. For objective probability, however, chance is only tamed by the constitution of a potential in an objective sense of the term, which we observe through the distribution of probabilities within that range. *Spielraum* include initial conditions that remain continuous with certain outcomes, just as fire is an initial condition for smoke, or tea an initial condition for caffenation. A judgment of possibility is objective only within a range of conditions that maintain an ordered continuity with outcomes. Within social orders such connections have been constructed and maintained to have duration and iterability. Projects can be made by actors, but those projects remain bound by their "average significance."

As constituted by "environmental probabilities" (Brunswik, 1943), our knowledge of the environment is probabilistic as a sign of what the environment *objectively* is. We can know signs of context- and range-specific objectivity the longer we are in them; and the better we know them, the more grounded are our expectations and firm our state of belief. To find objective probability in the environment is to locate and specify a positional content akin to what has been described as field theory (Martin 2003), which is supplemented here by a layered investigation of *Chance* as the most elementary unit of sociological analysis. To do that, we propose the following framework as one that can incorporate a data-centric focus but without losing a substantive view of sociological explanation.

A maximalist data science could make all substantive points of focus arbitrary relative to a view of the empirical that remains entirely bound by data tools, epistemology, and goals, on the premise that it accumulates limitlessly. But it still requires some way of understanding what the data shows, which means we must have an interpretation of probability. As we have argued, so far, a third alternative exists alongside the frequentist and the Bayesian interpretations. To the sociological toolkit of theory, data, models, and methods, *concept of probability* should be added. The de facto concept has, for a long time, been the frequentist concept. As Bayesians have shown, a host of problems across quantitative and qualitative research can be traced back to frequentism, and it creates unnecessary limitations on sociological research. An objective probabilist would agree with much of what Bayesians argue with regard to the unappreciated significance of probability. But what kind of difference would it make for our toolkit to make *that* concept the concept of objective probability?

Computation, Algorithm and Cognition

As others have suggested (Foster, 2018), sociologists might appeal to different layers of analysis to fully draw out the implications of daty-centricity. This has less to do with levels like macro, meso and micro distinction, then it does a concentration on different indices and measurements of objective probability. The computational cognitive scientist David Marr's (1982) "tri-hypothesis" approach is a useful model for this purpose in featuring a concentration on what Marr calls the computational ("programming"), the algorithmic ("software") and the implementational (or "hardware"). The first recommendation this adoption makes is that any given sociological explanation must be capable of working at all three levels of description; it must abide by the constraints set by each level. A computational description is concerned with the basic form of the information problem a given system is solving; an algorithmic description is concerned with how the system solves that problem; and the implementational description is concerned with the physical realizability of that solution. The computational description is typically abstracted from particular examples, but, in order to be *tractable*, it must somehow involve the transmission of information. The algorithmic description must refer to a process that can account for how the computational works: making a tractable computational model *intelligible*. The implementational description must make the computational model and the algorithm *fathomable*. We must be able to account for the realizability of the computational model, and the algorithm must be capable of working within some physically realizable system (like a computer or the evolved human body).

To speak most generally, the computational problem of society would seem to be simply how people orient themselves to its existence. To find predictive relationships in large data sets indicates a sparse connectivity, and this would suggest that, similarly, has something to do with sparsely connected patterns of orientation. What kind of algorithm could "run" this? The benefit of adding a physical realizability constraint is that it previews what the algorithm could be. The physical realizability of anything that creates sparse connections will, in a probabilistic sense, be akin to a so-called "Helmholtz machine" as "statistical inference [engines] whose function is to infer the probabilistic causes of sensory input … [A] device of this kind can learn how to perform these inferences without requiring a teacher to label each sensory input vector with its underlying causes" (Dayan et al, 1994, p. 889). For predictive processing, cognition is only physically realizable as a probabilistic inference engine focused on the possible causes of incoming sensory input using a cumulative, generative model, which physiologically reduces the presence of free energy in the brain. For an algorithm to satisfy both this physical constraint and the computational constraint means that it must have something to do with both learning and constructing. For this reason, a probabilistic loop becomes attractive, as it can obey both the computational constraint, and thus retain its answer for why people engage in action at all, and it can do this without running completely against a cognitive constraint.

Thus, the fundamental computational problem of probabilistic sociology can be phrased quite simply as where do orientations to the existence of social order come from? Algorithmically, any solution to this problem relies on a recursive loop to create well-grounded or non-arbitrary expectations among those holding the orientation. Cognitively, such a schema is "implemented" via action and learning in which "direct contact" with objectivity yields a generative model that can iteratively guess inputs in order to perceive them. Successful guesses reduce the degree to which inputs are perceived. The opacity of computational outputs stems from their producing "tractable" cartographies but without algorithmic or cognitive "intelligibility" (Zerelli, 2022). An opaque, conceptual black box arises when human modes of explainability cannot be coherently and unambiguously defined in a computational (or machine learning) context.

Early on, Marr (1976) categorized Type 1 and Type 2 "theories" to distinguish algorithms ("methods") that can successfully reproduce a data pattern. Type 1 theories grant the highest degree of epistemic access to computationally robust patterns, because it is possible to know what a "theory is supposed to be doing," or the competence the algorithm should "run" to adequately express the pattern. Type 2 theories, by contrast, yield almost nothing to human intelligence; nevertheless, they are still tractable in consisting of a "closed system of effectively computable operations" (Leslie, 2019, p. 41). The best Type 2 theory will portray the "simultaneous action of a considerable number of processes, whose interaction is its own simplest description" (Marr, 1976, p. 2); but it will not "explain" anything, because we cannot see what a "theory is supposed to be doing."

The history of social theory mimics this distinction (see Bourdieu, 1973). To explain patterns via an objectivist structure yields a kind of Type 2 theory, in which the computational problem is social order or organization. The simultaneous action of structural parts cannot explain a pattern; though this can map it out (Levi-Strauss, 1964).[34] A theory of action promises more of a Type 1. It will give an intelligible terminology for the reproduction of a data pattern and thus we can know "what the theory is supposed to be doing." But we should ask further *why* is such a theory fathomable? Why does it appear, in other words, intuitive? If a theory is supposed to reproduce a data pattern, and we understand this is what it is "supposed to be doing," intuition is not a subjective concept; it appeals to adequacy instead, which requires neither a subjectivist or objectivist (neither "God's knowledge" nor "hearsay") point of view.

Intuitive science treats both as partial; the knowledge gained from either alone is arbitrary. If,

[34] For Claude Levi-Strauss, structuralism was essentially born from an experience of opacity: namely, his encounter with Nambikwara facial tattoos (see Levi-Strauss, 1958, p. 243).

computationally, the information problem is simply an orientation to the existence of social order, habituated learning and expectation-formation that requires recursion or looping will render the word "subjective" non-inferential and entirely nominal. The same is true of the word "objective" as a reference to knowledge alone. Under these stipulations, analysts are "able to see what a theory is doing" only to the extent that a theory is algorithmic, which means it includes in its formulation an answer to the information problem.[35] This means that a theory can account for how what the analysis is treating as an outcome that could have been expected—in other words, it accounts for how that outcome can fall within a range. Any measurement of social order, whether "subjective" (like interviews) or "objective" (like aggregate statistics) must relate its findings to adequacy: how subjective beliefs are shaped because of objective conditions; how objectivist patterns are apparent subjectively within the context of action. If certain properties are settled on as "subjective" this is because these traits reduce what the predictive processing framework calls "expected free energy" which only makes sense relative to something like an objective probability structure that we can never be quite sure about, can feel like we are taking a risk in relation to, and which we will become most aware, or aware at all, of when what we expected is not compatible with it. A probability structure can only signal this to us; we can only guess at what it is from our observations rather than know it directly. This works in a Krisean way through what remains consistent in the range that extends between our guesses and states of the world. The match can never be perfect, or else there would be no noticeable sensory stimulation, or no ontology. That is what remains probabilistic to us, because it is unexpected (or equally expected, which here means the same thing); and the greater the gap is, the more contribution *we* need to make in constructing its meaning.

A probabilist like Bourdieu (2020/1984-85, p. 34) argues that actors are oriented toward

[35] The point here is comparable to the way in which Western (1998) redefines theory in Bayseian terms as a prior estimate that can be updated through exposure to multiple tests. The logic here, and in objective probabilism, is different from a falsificationist and interpretivist view of theory.

social fields only as those fields take the form of unique "distributions" and typically the cognitive costs of acting in a perpetual "conflict with the world," as Bourdieu puts it, are high enough to be prohibitive. As an information question, such an orientation to distributions includes having expectations linking initial conditions to outcomes by finding Kriesean continuities between them—in other words, what can adequately cause an outcome to appear given certain initial conditions because the proportion of that outcome that contains those initial conditions is larger than the proportion of those initial conditions that are not included. Bourdieu (2022/1985-86, p. 21-22; 2021/1983-84, p. 243ff) argues that "informational capital" involves learning the structures of the world and exhibiting them in "spontaneous statistical calculations" (ibid).

All "forms of capital" (cultural, social, economic; Bourdieu, 1986) are subsidiary to informational capital, as this capital expresses likelihoods, probabilities and predictions; in this sense, there is a sparse connection in social spaces with capital, not everything can lead to everything else. Thus, any "form of capital" must entail a distribution within a clear enough *Spielraum* boundary that an expected sense of "utility" will bear an "average significance." The utility can be expected, but not in a rational or subjective sense, as is associated with Bayesianism, but because it guesses at least part of a range: something remains constant between the guess and objective probability.

As an algorithmic concept, informational capital maintains what we might call this Bayseian model evidence in the form of expectations that are partial to the lowest log likelihood. If this is to be non-arbitrary, it can only signify probabilistically by referencing input conditions that remain continuous with outcomes within *Spielraum*—a point that revolves a loop of *Chance* and expectation. *Spielraum* can be rendered, in computational terms, as having the properties of a Markov blanket (see Pearl, 1988, p. 97).[36] A subrange of a given *Spielraum* cannot be incompatible with the larger whole,

[36] A Markov blanket (popular in predictive processing) tries to identify something akin to both parametric specificity and "relative autonomy" by specifying the probabilistic interactions between a field and its environment and focusing on the *boundary* between the two. Formally, it can be written as:

though it does not include that larger whole. Very much like Kries' logical approach, a Markov blanket attempts to find factors that are redundant.[37] Both a "blanket" and a *Spielraum* are spatial constructs bound together by a "normic law" which is, above all, practically reliable (Pearl, 1988, p. 477-78; Schurz, 2002).

Strictly speaking, then, the logic here is probabilistic but not statistical. Normic laws are obeyed for reasons of practical reliability, but this does mean determinism. Reliability, here, only signals the objective existence of a range. Probabilism emerges thus from the dance of *compulsion* and *authority* in a kind of direct contact with the world. To borrow terms from Peirce (see Bernstein, 1964), a kind of percipiuum, or "direct contact" occurs here in a distinctively passive sense, as an unmediated connection to the world. Peirce would call this "firstness" in his semiotic model, which he associated with "pure possibility" or "chance." He does not associate it with "objectivity." Ultimately, this led him to commit to a kind of frequentism (Burks, 1964; Suarez, 2019), as the reduction of chance in "secondness" and the habits of "thirdness" in a community of experience creates a Venn-like overlap between frequent experiences at ever more granular levels.

Objective probability overlaps with Peirce and with pragmatism but with the difference that "objective," for Kries, is a designation that does not essentially depend upon frequency or, in Peirce's terms, "observations in the long run." A sign has only a probable meaning because it remains linked to what it cannot render entirely determinant to anyone, which means that we always do have to guess, even though we are never wrong. Any outcome is objectively probable when we act on signs; those that stick around are those that seem to work, which means they help us perceive *less* about the

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$$\mu \perp x | b \Leftrightarrow p(\mu,x | b) = p(\mu | b)p(x | b).$$

Thus, any variable μ is conditionally independent of x if b is known. This means that knowing x would give us no further information about μ if b is already known. This is a basic way of defining adequate knowledge of a *Spielraum* boundary, though this does not imply that such a boundary is there because of a field-environment interaction; it more comprises the distinction to begin with.

[37] Not incidentally, Kries's *Probability Calculus* is listed as an influence by Andrey Markov (1908) in his early probability text (Seneta, 2016, p. 401).

world. In this case, the expectation/*Chance* loop is algorithmic because it creates observable and measurable patterns by canceling out what we learn is not relevant within a particular range.

To liberate data from interpretation, data science focuses on probabilistic distributions of signs. In machine learning, typically these are construed as a *maximum* log likelihood. ML algorithms measure for the presence of "information entropy" by disaggregating probabilistic distributions at deeper levels of priority as signs that, eventually, need no added (arbitrary) interpretation than what is already available from an increasingly sparse connectivity (Marino, 2022). Thus, a meaningful sign is a probabilistic indicator of connective strings of priority that, through understanding (*Verstehen*), we can approach intuitive "adequacy" (e.g. lowest log likelihood) as opposed to simply encountering an unknowable "chaos" (*pace* Berger and Luckman, 1966).

According to objective probability, measured frequency is not a kind of epistemic *ultima ratio* and will only convey something objective about the world if it tells us about the configuration *Spielraum* (e.g. "fields," "topologies," "action-spaces," "clearings") as objectively existing initial conditions that, with some "leeway," continuously link to outcomes within Markov-like boundaries. Independent and dependent forms of variation convey the sparse connectivity maintained through expectations by decreasing perceptual disanalogies and mismatches apparent in active inference. Those links allow for expectation-formation and adequate causation, both as objective forms of knowledge that do not require statistical estimates, in a manner that mimics Bayseian prior probability. *Pace* Bayesianism, however, the priors will be objective to the world. The corresponding probabilistic inferences are not limited to numerical, subjective, or epistemic claims, as they are instead fundamentally ontological, but in the "intuitive" (Krisean) sense of the word, which means they are constituted by "ranges" in the world maintained by strict terms of accessibility.

Orders, Tests and Outcomes

A basic example might flesh this out, but we add the same one that mirrors Kries' (2023/1886, chap 2) own account of a game of roulette—a *Spielraum* created by design, in which the range of possibilities and adequate causation are clearly specified. The sociologists Luc Boltanski and Laurent Thevenot (2006/1997, p. 78) reference a similar example of a *probabilistic order* to make an analogous point. They draw attention to the careful maintenance of adequate causation, accompanied by a limitation of the range of possible consequences, which for them are epitomized by the athletic contest—the football match, the basketball game, the Olympic games. Take the following example as a real world effort at similarly designing a *Spielraum* in which the distribution of outcomes can be *expected*. Most probabilistic orders cannot prevent all chance causation as clearly as an athletic game can; but the following example puts objective possibility and objective probability into a certain kind of relief as a *Spielraum* constructed to do exactly that.

The philosopher Alan Garfinkel (1981, p. 41) gives us a scenario in which a professor grades their class on a curve. He explains the rules as follows:

> Suppose that, in a class I am teaching, I announce that the course will be 'graded on a curve,' that is, that I have decided beforehand what the overall distribution of grades is going to be. Let us say, for the sake of the example, that I decide that there will be one A, 24 Bs, and 25 Cs. The finals come in, and let us say Mary gets the A. She wrote an original and thoughtful final.

In this case, the order is the distribution of grades and their corresponding probabilities (e.g., one A, 24 Bs, 25 Cs and corresponding percentages). As we can see, this structure is a closed range of possibility: there is not another grade possible, nor is a different distribution possible. As Garfinkel notes, this naturally begs a question about who gets the A and why. Here the explanation is that Mary, the student who received the A, "wrote an original and thoughtful final." Such an explanation, however, takes no account of the probability structure. Perhaps many students wrote original and thoughtful finals, and yet only Mary received the A. More accurately, Garfinkel argues, we should "[point] to the relative fact that Mary wrote the best paper in the class."

The probabilities here are objective as a feature of the world rather than anything that emerges from an epistemic calculation. There is no other possible distribution than the one the structure allows, and we must keep this in mind if we are to ask the right question about anything that might happen within the parameters of the order. As Garfinkel writes: "In cases like these, the imposed structural conditions radically alter the kinds of explanations we give because they constrain and truncate the contrast spaces," or the difference between what is made real versus what is originally possible. The "contrast space" for Mary getting an A is Mary getting a B or C, and similarly for each of the other students. It was not possible for them, or Mary, to receive anything but one of these three grades within the predetermined distribution. Importantly, because of this structure, we must appreciate that we are dealing with a fully relational set of empirical outcomes. Mary got an A because John got a B: the two outcomes are not independent. Within this context, Mary's paper was better than John's, which implies that anything truly unique to Mary's paper can only be of secondary importance for the outcome. If her paper was "original and thoughtful" this matters only because it made her paper relatively better than John's paper when the professor-judge puts the two side by side and tests them. Thus, the quality of John's paper is not independent of the quality of Mary's. Rather than simply looking at them as two papers, we see them, instead, as two positions in or parts of the structure.

Probabilistically, the relevant features of an order, then, are the available positions, as these indicate a range of possibilities. A probabilistic order renders some things unlikely (and unthinkable), while what is possible within an order is not equally probable. In this specific case, students are not predetermined to fit a given position, yet their position-takings are not independent of others. In this order, the evaluator who makes the assignments of worth becomes consequential for the capital immanent to the order, which acts as a control on the future. The professor decides that originality and thoughtfulness in the written text matters for deciding futures (e.g., who will end up in what position). While all fields feature capital in some form or another, only some will find an evaluator or assignment-maker determining the positions and deciding who will be in them.

At least in principle, it does not matter who Mary is, that Mary is a "Mary," that she was born to a certain family, that she is Black, or that she is a she. What is relevant for this order is that one A was possible and that, on some metric, Mary was better than (e.g., had more field-specific capital than) other students who had the potential to have received it. The rest of Mary's traits, should they have any bearing on the outcome, are "inessential perturbations" or accidents, or what we can understand as pure difference or chance relative to the probabilistic order and its control on what matters. Not everything is capital, as we explain further below, though a field makes certain traits, qualities, or abilities into capital, and this capital can accumulate by making all other attempts (e.g., different traits, qualities, and abilities) to control the future, at least as this constitutes the stakes of the field, relatively improbable. Moreover, "outcomes" cannot be just anything. Specifically, they cannot be anything new. While new people can be found in positions they were not common to before, an entirely new position is, at least in this order, impossible. To create such a position would break or imperil the entire order.

Garfinkel's example shows that we must account for the full range of possible outcomes to explain any particular outcome within the order. This makes our explanations particular to the order they try to explain. If what we are explaining is a probabilistic order, we must know that; otherwise, we will ask the wrong questions. This example shows us that, probabilistically understood, an order refers to a range of possibilities and chances that persist and have duration. Beyond this, those possibilities cannot simply be observed from a distance to infer their effect. In this case, the students were also oriented to the order, which gave them a fixed sense of what is possible (e.g., there will only be so many As, Bs, and Cs). On that basis, they loop into the range of possibility and make probabilistic judgments about what is likely to transpire for them given that this is the range of possibility. Their experience in the class acts in this way to anchor their judgments, which makes them either more or less realistic. Thus, Mary might have anticipated getting the only available A because she had done well in the class before. Because the A seemed realistic to her, she could find the motivation to obtain it; it was not so much of a risk to wager on a future in which she obtains the A. A different story might apply to John, for whom obtaining the only A was not a realistic judgment. He could still have this judgment, of course, but whatever allows him to have it must work against the weight of the past.

Even if the distributions are not as specifically enumerated as they are in this case, we can still find them by tracking the frequency of occurrence and repetition. Even further, if the structure of a grading curve involves the assignment of grades to a final paper, this indicates the presence of a test and what we might call a test space in which the experience of tests of one sort or another, whether by design or happenstance, create a trajectory linking a starting point of open possibilities with a point of arrival ("the actual") when those possibilities have become objective to us. This is what allows us to calculate probability to begin with because a test ensures that each statistical "trial" or observation serves to record comparable things. Actors also learn probabilities via tests and trials, which don't have to be as highly formalized as those we typically encounter in an educational setting.

For a probabilist, a test refers to the resolution of uncertainty through learning that depends on action, particularly action in conjunction with a source of potential resistance that asks us to try. This is the most basic engagement with a real but unknowable range of possibility. When we act we, at least initially, "invite chance in," even if this only through engaging in some new part of the world. When we do this, we will encounter things we never could have expected, which allows for learning. Expectations form as a kind of reaction from our having become active; they show how we orient ourselves to *Chance*. We try to push open a door, for example, but there are two stout men holding it shut on the other side. We try to write the novel but cannot keep our character arc clear. We try to kick the ball into the net, but someone jumps out to block it. These are basic "action-reaction effect flows," but more than that they are a learning process: they link initial conditions to outcomes via action, which is how we learn probability.

Tests are sites of single-case probabilities like these, and it is only our expectations that keep each trial analogous with others, testing and learning the same range. Any change to the initial conditions could lead to different outcomes, and to our surprise our expectations fail; while we have learned the probabilities of a given range, we have not learned them all. In any case, it might seem like chance has intervened when it should not.

Thus, tests can serve to bind together instances of time, which is essential for learning probabilities. We try in certain ways, in unknown circumstances, and associate instances together. We rely on our expectations by making what we presently perceive similar enough to what we have engaged with in the past to appreciate that it is not unique. Thus, it does not try us in the same way at $Time_2$ as it did at $Time_1$. The same is true of statistical probability: this way of experiencing time suggests something quite the opposite of a lottery. Here, we presume that the present will be like the past, as the past is where our observations come from. Yet, we must accommodate what a concept like "testing" draws our attention to: single-case probabilities that arise from the potential and range of possibility of probabilistic environments (*Spielraum*) that measurements of past frequencies, because they find probability only in countable outcomes, may have difficulty predicting. We must not consider single-case probabilities as random, as our own expectations would suggest otherwise.

In Garfinkel's (1981) example, we find a refined and controlled test that, by design, channels actors into a limited engagement with a small range of possibility, and a correspondingly limited distribution of outcomes. Each student can only demonstrate their knowledge and paper-writing skills; the test recognizes no other action than these as adequate causes. The range of outcomes are observable from the start in the distribution of potential grades, which Garfinkel uses to highlight a structural explanation. The applicable practical knowledge conveys the specified continuity of initial conditions to outcomes, in which case all that matters for bringing about an outcome is the quality of the writing and the knowledge it demonstrates. The only probabilities we might discern of the initial conditions that could lead us to expect who will get the one possible A are those that pertain to the actions (writing, knowledge) made relevant by the test. Such a careful determination of adequacy allows for a perception of social justice in the final distribution.

In a computational sense, a test marks the key variable around which all else because it draws the major boundary and distinction. Computational neural networks render this similarly as prior probabilities that can largely predict posterior probabilities. But this implies that, as an information problem, the prior probability must be what orients actors toward some self-organization that indicates a posterior probability. This means that something about the link of a prior probability to a posterior probability must pertain to the construction of expectations that define the kinds of errors that will be minimized, which is just another way of saying how other potential prior probabilities are made *not to matter* for the posteriors. The test, in this case, becomes revealing of the link to that outcome distribution, and this is essentially an algorithmic claim. To know what the test will test, even and particularly when it is not made explicit, conveys a "third knowledge," an intuitive, practical knowledge, rooted in expectation/*Chance* loops, as actors self-organize relative to expectations, even if they deviate from them.

This is what it means, then, to interpret probability objectively as constituting "objective space" (Bourdieu, 2021/1983-84, p. 85). The constraints at the three levels of description work on each other and, in particular, forbid a computational description as being mistaken for objective. This point is key, we argue, in mounting a front against the appropriation of probability for the purposes of authority. The consequence is, effectively, an alternation in the world of habituation, the world where habits form, and where "projects" in Merleau-Ponty's (2005/1945, p. 522ff) words will come from. This is much less the case when the range has not been predefined and we cannot easily tell the difference between adequate and chance [*Zufall*] causation.[38] An educational test attempts to resolve only those certain things that it makes unknown about each student. The test presents them with a range of possibilities; some of those possibilities become actual for each student while other possibilities will never happen to any of them.

Probabilistic Order

A particular test can give rise to a particular probabilistic order. An *apparatus,* for example, is an order in which action matters, but with such clearly defined and restrictive tests, such specific ways of self-organizing, that we are asked to try with very little range of possibility. This is characteristic of all machines, for instance, including Foucault's description of Bentham's Panopticon, which portrays a *Spielraum* of almost no possibility, as actors are made nearly fully predictable by the enlistment of very predictable architecture as an initial state. *Social fields*, however, are more characteristic of a wider range. For example, as a social field, religion might render some things impossible, but the potential of religion can (and will) take shape in ways no actor who self-organizes with orientation to the field can presently envision. In any present moment, most of those possibilities of a field will seem entirely improbable; still, as the test of the field changes over time, as its boundary maintenance shifts the limits of the "blanket" making other priors more important, the potential of a field will develop in new directions, which are first revealed through action. A still different probabilistic order is what we can refer to as a *game of chance*. Like an apparatus, it too has only certain, restricted possibilities; but because what happens in action seems to have little relation to the probabilities of

[38] Merleau-Ponty (2005/1945, p. 522) makes the point that while the French Revolution was a "project," it carried an "average and statistical significance" to which it remained bound. This meant that while there could be no going back to a pre-1789 world, the continuities (rather than breaks) produced the Bonaparte period in a way that is not a return to the past but is not the fulfillment of the Revolution.

outcomes—they all appear equally probable. We can try to flip a coin a thousand different ways; none of them will seem to have an adequate link to heads or tails turning up next. Often, these types of probabilistic order can also enlist non-human objects and their opacity to our full learning to maintain an unpredictable, even chaotic link between initial conditions and outcomes.

The occasion for interpretation becomes when basic gripping needs and can have this supplement in order to loop in and minimize error. In some orders, like Foucault's Panopticon or Mbmebe's "postcolony," interpretation does not seem to find any purchase in error minimization; in others, like Salem Witch Trials, it assumes a far greater significance, as an orientation distribution appears to connect to no stable initial conditions. The same applies more generally to what Weber describes as the "incongruity of merit and destiny," which indicates a mismatch of initial conditions and outcomes, and thus provokes the "specifically senseless experiences" that invite interpretation as a consequential agentic power for error minimization.

Tests can be deliberately designed to try people in specific ways, with a specific mode of striving or trying against a potential source of resistance, to reveal a specific aptitude, competence, or quality in the performance, taking only specific things into account as adequate causes of the outcomes. This shows the presence of rules, rulemaking, and an organizing authority, which constructs the range, defines and authorizes the test, and maintains the order, and thereby works to forbid senseless experiences. Ultimately any probabilistic social order is rooted in the expectations it creates—the orientation to *Chance* it creates via looping. The educational test presents itself as a test of knowledge and effort, presenting both as adequate cause; but the formalism, in this case, may not be the source of the expectation—it becomes expected that the racial identity of those tested, along with family income, are what it really tests. Sociology can reveal this underneath the official story. Presumably, computations can reveal objective social space at finer and finer levels of the "mosaic." But the "essence of the thing" is a judgment of possibility. Should we zoom in as close as we can, this is what we would see. Such an approach means that counts of frequency and statistical analysis, or even data tracking across infinite actions, are simply additional ways of learning about ranges of possibility alongside the equally legitimate learning done by actors themselves, as they engage in their own trials. This orientation to *Chance* is conceptualized (following Weber) as a judgment of possibility.

While probabilistic reasoning emphasizes the role of structured social order, it does not also annihilate history or deny any role to the unexpected. This is largely because probabilistic orders are not monolithic; in fact, they cannot be "real" if by this we mean having definite features in and of themselves. Order consists of a specification of Chance that can be more or less complete but not entirely random. Probabilistic orders can be more or less extensive and unpredictable, which depends in large part on the role given to chance mechanisms ("inviting chance in") and the degree of codification of tests ("taming chance"). Tests themselves can suppress almost all *Chance*, and in such cases the order will distinguish a rigidity akin to what we describe below as an "apparatus." As in the case of categorical tests, the future-shaping effects of capital, and the range of possibilities created on these grounds, tests may operate strictly in accordance with a specific index, say of one's race or gender identity, with the ambiguities of cues omitted (often by resorting to a crude biologism, indexing only phenotypic traits like skin color). Yet other tests can displace the effect of these by calling out different qualities and revealing different traits, dissolving those otherwise made so prominent in the "absorption of uncertainty," ones that find variation outside these categories and break them by creating new groups in new orders.

Thus, to single out a given "fact" as adequately causal, we must build a "complex of relevant contributory factors," and then consider whether, if a given factor were to be taken out, the same "course of events" would have happened in the same way. The paradoxical aspect is that Weber acknowledges the "complex of relevant factors" as built by an act of "imagination," and yet we also make a "judgment of objective possibility" based upon "rules of experience," a phrase that Weber (1981/1913, p. 179-80) uses persistently in his later arguments. More specifically, the difference here indicates how Weber grounds his arguments in principles of probability even though these sorts of questions cannot be resolved quantitatively.

When Weber formulated these arguments, he held no firm identification as a sociologist; yet he was seeding ideas that would later turn up as the core of his sociology. This suggests two things: first, that if a sociological (here "historical") method is not to be "arbitrary" then it must be *probabilistic*. Second, this is not a statistical recommendation, if by that we mean those judgments can only be valid if they are made on the basis of frequency counts.[39] If what we are dealing with is not an underlying chaos but more like a *Spielraum* (e.g., probabilistic order), in which action and expectation are the only clues of adequate causes, then we can break apart the objective processes that a historian documents from initial conditions, and assign likelihoods to outcomes, making judgments of objective possibility, distinguishing these factors as adequate and those as chance, based on refinement in our knowledge of a *Spielraum*.[40]

Weber first adapted Kries' ideas in the context of his methodological writings, seeing them as a way to distinguish "cultural science" as a field. The probabilistic reasoning found here has gone

[39] Thus, imagination seems "wide open to subjective arbitrariness," while rules of experience, Weber implies, are not *entirely* subjective: our judgment of objective possibility can make sense to others. For Weber, this means that while arbitrariness can take over, should we try to imagine what *would have happened* had a given factor not been present in a given causal complex (e.g., what would have happened had the D-Day invasion failed in 1944?), it is not a concern when our analytic gaze is focused on *what did happen*, with our questions focused on causal factors that explain why (e.g., did the D-Day invasion cause the collapse of the Nazi Germany?). The difference is that the first question is not bound by rules of experience, thus allowing the imaginative capability to take over, while the second question is, which tests that imaginative capability by something non-subjective. Judgments of objective possibility, which can distinguish adequate causes from chance causes, are valid only by way of "rules of experience" [*Erfahrungsregeln*], which Weber emphasizes in a coded reference to what Kries calls "rules of expectation" and "measures of recognizability." Because each of these phrases refers to a looping in, of subjective toward objective probability, they stand as a refutation of any claim that, in this case, the judgments the historian makes are like judgments of games of chance where anything goes because nothing we could conceive of will ever anticipate an outcome

[40] As Weber (2018/1905, p. 175) puts it, in clear distinction from a strict epistemic understanding of "possibility": "[A] judgment of 'possibility'—in the sense in which that term is utilized here—always implies a reference to rules of experience. The category of 'possibility,' therefore, is not utilized in its *negative* form—that is to say, as an expression of the fact that we do not, or do not completely, know something … Quite the contrary, [the judgment of 'possibility'] here implies a reference to positive *knowledge* of 'rules governing events,' to our 'nomological knowledge,' as it is usually termed."

mostly unacknowledged, however, under a broader discourse of comparativism and interpretivism. Adequate causation and ideal-types, for instance, are generally treated as purely epistemic or interpretive tools, which for many connote exactly the arbitrariness that Weber was at pains to distinguish. This only fuels the false presumption that more data (e.g., a larger-*N*, more cases) is needed to perform anything like a probabilistic judgment. Yet, such a judgment is not an "interpretation," at least not according to any current analytic understanding of it and the method it typically entails (see Reed, 2011). Drawing stronger links to Kries suggests an alternative: if our analytic judgments involve guessing at a *Spielraum*, then our concepts lead to an epistemology of looping, linking subjective and objective probability, with the premise that it is possible for the two to be more in alignment. A cause is adequate not according to an interpretation, or according to theories that analysts alone have access to, but relative to a *Spielraum* that makes *Chance* (opportunities, probabilities, risks) apparent *in action*, within the parameters of which adequate causes appear, while outside the range they will not.

Thus, a *Spielraum* is real, probably unknowable in its entirety, but nevertheless capable of grounding judgments. *Chance*, in Weber's historiographical application, is only part of a critique of knowledge. But as sociology, however, Weber will eventually conclude over the couple of decades from the time he first seems to engage with Kries, the task is to take it much further—using probabilistic reasoning to study probabilistic orders as they actually exist.

Discussion

From Future to Present

The details suggested might make action out to be a relative miracle worker; but not really. Action can be a data vector because it (and it alone) bears an inextricable relationship to the future; only it generates outcomes, which can be made computational.[41] This is consistent with predictive processing, and indicates the distinct sequencing of action as a probability (input-output) vector. But what about Marr's (1982) algorithmic or implementation levels? PP makes its case not bottom-up sequencing, but more top-down: in this case, from neuronal implementation to computation. We have drawn lessons from that approach. A cognitive social science would revolve closely around the algorithmic layer, as we have claimed: more specifically around the loop of expectation and *Chance* and the dialectical dependence of *a parte subjecti* and *a parte objecti*. This involves the implementation of input and output as physically realized and cognitively robust but also at scales and ranges more conducive to the sociological imagination. In Weber's (1981/1913) words, there can be an "objectively correct type" of social action, and that this should be where sociologists focus their explanatory goals first, which implies a *Spielraum* range characterized by adequate causation and expectation. Data science can give a computational rendering of this "type," and predictive processing principles like error minimization can offer insight about its physical realizability. But without accounting for the social "algorithm" that creates and maintains the range, as retrievable from the adequate knowledge of those capable of action, there is essentially "nothing to see here" from a sociological point of view. Granted, the algorithm remains an analytic construct: nevertheless, what it attempts to capture is physically realizable at scale, which as we will now emphasize, involves something about basic gripping action and what this implies about sequential ordering.

Rather than drawing on the past to anticipate a yet-to-occur future in an immediate present (e.g., a sequence like *Past* → *Present* → *Future*), the future, in a first step, draws on the past to generate a present (*Future* → *Past* → *Present*). Note the close connection between this account of a basic *Chance*-expectation loop (and the nested action-perception loop). Continuity in this or in any other explanation means action cannot *only* work at the horizontal level of an unfolding narrative-like

[41] It was for similar reasons that quantum mechanics provides an extensive vocabulary "action": "quantum of action," "action at a distance," "principle of least action" (Hesse 1962; Berkovitz, 2007).

process, as it appears in folk accounts. Attention becomes "present" via such non-linear temporal loops in the form of qualities like "hope," "foreboding," "danger," "blocked horizons" and the like as reconstituted action environments of real possibilities (Bourdieu, 1973, p. 75-76).

If we pursue this qualitative physiognomy in our research, it might seem restricted to the potential predicates of a personal explanation. As data, these qualities might seem to lack objectivity, presenting simply as a "feel," "sense" or "vibe." A probabilistic approach offers a conceptual framework to understand these as evidence of a *Chance*-expectation loop cutting across levels of perceptual content, concealing a relative attunement or adjustment (or lack thereof) to what objective probabilities capture in our orientation. Dispositionally, these qualities show actors *Chancen* relative to what they become accustomed to predicting as the equivalent of an "average." Probabilism makes a simple observation at this stage: if expectations cluster around an average this is because objective *Chance* itself clusters that way. Much like data science uses a past sequence to reduce adventitious encounters, probabilistic action theory creates similar temporal relations by bringing two states of a probabilistic environment (*future - past*) into relation, which indicates the structured presence of a sequence or a rhythm in the dual sense of *Spielraum* of specific outcomes and corresponding expectations.

The closest equivalent of probabilistic theory of action is found in pragmatism and particularly the distinctive connection it draws between problem-solving and qualitative experience with a concept like "resonance," but in attempting to make such a translation, we reach the limits of pragmatism. "Puzzling through" is both led forward, in this regard, into a more general space (e.g. of generalized others). This engagement—vertically orientated, subpersonal, with a horizontal direction—seems to require an equivalent of prediction in action. As we try to asymptotically approach a *Spielraum*, we solve a problem by looping into it, learning how to make the right anticipations, and practically "correct" predictions based on the signs presented to us.

Perception is not just spectatorial or contemplationist; it is far less contingent than that, serving instead as the "initial stage" in a dynamic action cycle that, in probabilist terms, is objectively possible within some initial conditions. Perception is *for* something, in other words, and it is linked to anticipation, prediction and ultimately to action. The explanation of action that marks a pivot from structural explanation often has a schematic interpretation come first as part of the following sequence: *Interpretation → Perception → Action*. For instance, we can find this in the argument that schemas are the first step in the formation of social structure because they "read" resources (Sewell, 1992, p. 17), or the argument that before social action can occur an "interpretation" must ensure that the action unfolds in a meaningful landscape (Reed, 2011, 135). Reading or interpreting assume such a seemingly irreplaceable role in empirical sociological research because prediction has been conceptually severed from action.

Suppose what we are setting out to explain is a structure or system to which we ascribe certain predictable qualities, then we cannot deprive people of having expectations and interfacing with these objects, in some form, as sources of their own predictions. What comes first, then, are attempts to adapt, via anticipation, to the existing probabilistic structure of the environment: from some initial conditions to some outcomes, linked by action. So, we get a sequence like this instead: *Action → Perception → Interpretation*. Interpretation and perception do not need to enter action directly or as a precursor; they are instead an initial condition or the mismatches of our guessing future states (outcomes). Action is first, and because what we perceive are the *differences* between our expectations and the world's probabilistic states. The range of possibilities allowable within certain initial conditions only become evident as the outcomes of action, which here serves in the role of reading or interpretation as modes of legibility. We are subsequently led forward to either adapt to or eliminate these errors, which could mean leaving a probabilistic order entirely, no longer orienting toward its range of possibilities.

In contrast to statistical reason, prediction in this sense does not require frequency counts, though this does not mean that it is a hopeless cognitive bias, to be removed with more careful attentiveness to its heuristic danger. Prediction is not *merely* "subjective" in the disparaging sense of the word. The source of observable and measurable probabilities, only becoming clear as frequencies "in the long run," *Chance*, or single-case probabilities, emerges from probabilistic judgment within some initial conditions. According to a probabilist, learning probability and counting frequencies are two different things.

From Action to Order

How much does a range extend in space and time, how far apart are two points of an order from each other, how spatially distant or temporally separate are they, are they related in a succession of order with certain points *meant* to follow others, and how subject is the *initial condition - outcome* relation to unexpected surprise? Pockets of predictability, small or larger ports of calling, identify and then work to tame (eliminate) chance mechanisms, allowing only for adequately caused action. These are ranges of expectable *Chance* (see Palonen, 2005). But we can "move off the port and out to sea," and we will know it when we do. Minor actions might lead to great effects. We won't be sure what should follow what; we cannot contract any moment into any other. Reality will start to cling to every act, every detail—our attentiveness grows along with unpredictability. Everything, we learn, can be held against us; anything could be our fault. There is no sense of adequacy here. Left in these circumstances long enough, "we begin to live another way" (Serres, 1995, p. 111-112).

For a probabilist, action is always for orientation in a manner that harkens back to Pascalian reasonableness. The difference is that learning probability is not a route to normativity. Action is not for winning a game but for gaining an orientation *to* the game, figuring out what to predict, what the chances are, what the laws are, and for maintaining the orientations we already have. To be "oriented," then, means to have expectations that find a counterpart in something objective about the world, at least as much as we can know this via its probabilistic structure. In a cognitively robust sense, we become adapted to minimizing errors or surprises, with action serving as a venue for active inference, en route to minimizing perceptibility. If the brain needs to guess what our experiences are based on the sense impressions it receives about the world around us, then our social explanations must be consistent with this basic prerogative. If sensory input can never be entirely expected in probabilistic environments, then initial conditions are connected to outcomes not with mechanical determination but according to signs that are always partial, and that require actors to *guess* them.

As we have stressed, probability is what statistical measurement can tell us about when the range of possibility (*Spielraum*) allows for analogies to be drawn between past and present cases and when the temporality in question is of long enough duration to demonstrate frequencies. But this raises questions about probability that cannot be answered by statistics.[42] Typicality, existent on this

[42] Take some of the most influential sociological research published since the turn of the century, specifically Devah Pager's (2003) groundbreaking research on the "mark" of a criminal record and how race itself—being Black, more precisely—operates as a kind of negative credential on the labor market. Pager begins her argument by pointing to a set of objective probabilities that sociologists now know well: "Using longitudinal survey data, researchers have studied the employment probabilities and income of individuals after release from prison and have found a strong and consistent negative effect of incarceration" (2003, p. 939). But why is this? Pager's original piece focused on the limitations of purely observational survey data to establish this as an objective probability. After all, incarcerated individuals could be different from non-incarcerated ones on an infinity of other (unmeasured, unmeasurable) characteristics. In addition, even if we were to believe a given observational correlation, survey research is unable to "formally identify mechanisms" (p. 939), or the intervening process connecting antecedent conditions (incarceration status) to outcomes (decreased labor market opportunity). The results have since passed into sociological lore as prime evidence for the existence and consequences of structural racism. While the mark of criminal record depressed the chances of being called back for an interview for whites, the effects of race outweighed those of a criminal record, with Black testers without a criminal record being less likely to be called back than white testers with a criminal record, and with Black testers with a criminal record being doubly discriminated against, having a miniscule probability (5%) of receiving a call back. Should an appeal to action be used to explain this structural effect further, we might find an account like the following: employers need to meet staffing needs with workers, and they apply certain habits to so, having first perceived the situation through a racial placement that accords with the structure. Potential employees (Black, male, criminal record) are believed to be not trustworthy, meaning that the hiring decision, on this basis, systematically generates "discriminatory allocation outcomes." A probabilistic account would consist of asking how racialized generative models predict discriminatory allocation decisions through the mediation of a criminal record. Calling back Black men without a criminal record would potentially challenge internalized racist generative models predicting their criminalized status without having to see direct evidence of potential threat; for instance, potential employers receive high-precision evidence that Black men are neither preemptively criminal nor bad employees. So *not making the call* is the consequential action, and it is, in fact, a form of active inference that perpetuates and self-fulfills a racist generative model. For probabilistic generative models to be racialized means to reinforce, or allow for, only certain active inferences, which means that the source of discriminatory

kind of scale and inclusive of many bits of empirical evidence, structural accounts of order will remain vaguely computational constructs, in which we can see initial conditions and outcomes in the form of correlations, but without any multi-level attention. Why, then, should we limit probability as merely our (the analyst's) computational knowledge? A probabilistic loop that creates and recreates the order is contingent on a supportive counterpart in the expectations of actors. Thus, far from supporting a cognitively "smallist" perspective, probability in action links "micro and macro," in the old locution, as prediction errors and generative models emerge at the interface with *Spielraume* or probabilistic orders of various, and often spatially wide and temporally long, ranges of possibility.

As an appropriate framing of the social world, which finds its most fundamental form in repeating loops of expectations and *Chance*, the constitution of more or less rangy *Spielraume* have certain, distinguishable properties, or what here consists of constructors of objective *Chances* that can come into intuitive expectation as actors' self-confirm. All collectives, all formations of scale, can be redefined in these terms. They can all be objectively distinguished as having something that temporally stands as prior (initial condition, chance set-up) and something that temporally stands as an outcome. Yet, the outcome, in this case, must be able to loop back into those initial conditions, such that it *could* have been expected. This implies a continuity between those initial conditions and the outcome; it is not a billiard ball collision. Rather, outcomes must somehow be rooted in initial

allocation decisions is, more strictly speaking, in practice rather than in meaning. While the criminal record functions as a "negative credential" in Pager's case, whiteness serves as a positive credential, rendering criminalized white people more employable than (officially) non-criminalized Black people (Ray, 2019). Whiteness cancels any "error" produced by the perception of official criminalization, while Blackness does the same in the other direction, producing self-fulfilling evidence that in the labor market to be Black is to be de facto criminalized. Thus, when faced with discrepant combinations violating expectations (white/criminal, Black/non-criminal), what we observe here is not a "perceptual inference" that simply updates a top-down prediction by bringing sensory input into alignment with a preexisting interpretive structure. Racialized "discrimination" in the labor market is instead active and rooted in action even if not always "willful" in the naive personal sense. The only necessary condition is a generative model for which action (e.g., decision to hire) generates the sensory inputs that it expects (e.g., active inference). The "mark of a criminal record" is comparatively negated for whites in the very same way that it is implicitly assumed (e.g., generated as an expectation) for Black applicants, whether "marked" on paper or not. Thus, the minimization of error becomes conditional on maintaining a racist labor market, extending all the way down to the action generating sensory input that a generative model expects, which in this case means that "discriminatory allocation decisions" are really a systematic (e.g. chance-expectation loop) reinforcement of racialized objective probability.

conditions as in an actor's expectations linked to some field of potential.

For a probabilist, such a loop is only ever a tendency, not a deterministic certainty. It must be continually engaged. Orientation to a ranges of possibility can be intuitively captured by the very fact of having expectations, feeling bound by them, experiencing hope (or ominous foreboding) according to them, or engaging in presumptuous action; this shows evidence of being oriented to probability in a single-case (*Chance*) sense outside of an orientation gained via frequency counts. Regardless of whether sociologists interpret action as the performance of cultural meaning or the "enaction" of institutional scripts, pursuing a desire based on a belief, as a motivation to realize a value, or solving a problem—to reference a few of the most prominent accounting schemes—probabilistic action tries to formulate the link between action and probabilistic order at any scale.

Data science is right, then: there *is* probabilistic order present even if its apparency is hidden within layers upon layers of empirical phenomenon. To loop into it is not to act according to its dictates in a precise or direct way; the point, rather, is that the empirical orders we see assume their coherence between action that loops in and is responsive to *Chance*. This might appear in percepts organized as a rhythm or the anticipated forward feeding of expectation linked along a sequence of action, with a firm sense of "what should happen next" although it is not subject to the dictates of a law. To be responsive to a rhythm in action demonstrates a probabilistic orientation. Probabilistic order can also become evident as sequences or "orders of succession" in Leibniz's words as extensions in space and time between different, temporally specific positions.

For sociology, the standard "problem of order" is typically linked to moving our explanations beyond correlations. For the probabilist, this does not mean we leave probability behind, nor can we claim to have explained action if we do leave it behind. Much as Kries suggested, if we are playing roulette, we are playing on a table that has divided up the possible outcomes in only two ways: either the ball lands in the red space or it lands in the black space. We can only expect one of the two; yet our action of flicking the ball onto the table could potentially yield all kinds of outcomes, if not for the table already being divided up this way. Thus, we can only expect these two possible outcomes, and we can only make judgments relative to them. What probabilistic sociology contends is that the equivalent of these possible outcomes is already out there as "social order," and that much as Weber seems to have intimated, we can only make judgments relative to the range available in that order.

On a frequency measure, we will see that the possible outcomes in an order for which we hold equal expectations are not, in fact, equal. There are different outcomes being created from initial conditions, contrary to our expectations. For the probabilist, this does not invite a dismissal of *Spielraum*. It only shows us that the *Spielraum* in question are different from what we expected; fundamentally, attention is cast here as a broken expectation or surprise. Challenging whether "equal expectation" has any integrity is often elemental to social justice critiques and articulations of disparity outcomes. A purportedly equitable social order implies that the chance set up or initial conditions do, in fact, allow for equal chances between comparable *Spielraum*, so whatever ratios we see should only be due to factors that adequately cause a distribution, which are those that do not affect the comparability between the outcomes (they remain equiprobable).

A contrast can be glimpsed in an order with no equiprobable outcomes, which must take us into the realm of thought experiment. In Jorge Luis Borges's short story "The Lottery in Babylon," a random mechanism assigns the world-weary Babylonians to roles, occupations, and more generally positions in the social order (e.g. "Like all men in Babylon, I have been pro-consul; like all, I have been a slave") based on an entirely opaque logic. It is impossible to hold expectations in this case, as the *Spielraum* of different outcomes reveal no probabilities: all positions in the social order are equiprobable. From a probabilistic perspective, this is the very definition of chaos. There are no initial conditions that can predict any outcome, which makes it guesswork about what might actually be an initial condition. In fact, as Borges' story unfolds, this becomes so unclear that it seems like *anything* that happens (e.g. "The drunken man who blurts out an absurd command, the sleeping man who suddenly turns and chokes to death the woman sleeping at his side …") could be outcomes because anything the probability of which is impossible to expect qualifies as a possible *Spielraum*, with unknown initial conditions creating a zone of potential outcomes that themselves become unpredictable. All that the Babylonians are left with in this scenario is interpretation.

We can use correlations to move vertically from aggregations of data points (e.g., race as a variable linked to a multiplicity of outcomes) to social action maintaining the objective probability of a given social order (e.g., a racialized social structure). Correlations assign quantitative meaning to probabilities toward which actors could be oriented. Even if they are not, correlations could still serve as a sign of how actors have incorporated probability into social action by attuning to objective *Chance*, based on internalized probabilistic models of the social world.

The tendencies of probabilistic orders to ensure their continuous reproduction are sustained by the investment of actors in those very regularities that allow them to predict and anticipate on a moment-by-moment basis. This allows analysts to examine how anticipation and, more specifically, classification, are evidence of probability-in-action, akin to a description loop. The "best classified" of the properties that describe individuals and actions are those that carry the most "infamy, stigmata, especially the names and titles expressing class membership whose intersection defines social identity at a given time—the name of a nation, a region, an ethnic group, a family name, the name of an occupation, an educational qualification, honorific titles and so on." These classifications serve as the basis for "appropriating practices and properties" according to a "probable distribution between groups that are themselves classified." This distribution precedes the appropriation, which does not unfold in random space. The classification is a matter of probability, rather, which means that the appropriation is also a matter of probability.

A probabilistic distribution is appropriated by those who can anticipate how they will be classified by that distribution. But the process in question can be less elaborate because, in a sense, it is constant. Since we are constantly making predictions about the world at multiple timescales, we loop into objective probabilities of all kinds. This includes those involving even repetitive, "pointless" action, like fidgeting, whose only purpose appears to be reminding the brain that it is still part of a body. Thus, we do not need apperceptive self-knowledge of the *Spielraum* we orient to or how we mediate our guesses through it. The point is that our action unfolds only in conjunction with it.

Probabilistic sociology is not functional, but neither does it subscribe to a conflict approach of highly structured interests in all cases. It does not even subscribe to contingency. It subscribes, rather, to the objective existence of *Chance* as understood to have a structured presence in a world of non-arbitrary expectation. As Weber understood, this requires a distinction between adequate cause and chance cause, and which in turn implies some boundary-making around a formation like a probabilistic order that can create and maintain the distinction between the two. Adequate causes, in the Kriesean sense, are always linked to actor expectations, though we must stop ourselves from assuming they are purely subjective guesses—if they were, they could only rarely have practical consequences, and only as a result of a lucky guess (or lucky loop). As emergent "islands of uniformity, stability and predictability," loops feature distributed outcomes that are persistent, learned, and maintained as orientations in action.[43] Hence, whatever else might make certain probabilistic distributions objective, action (as anticipation and expectation) needs to loop into them if they are to be maintained and thus be capable of having the horizontal effects that *we* (the analysts)

[43] We do not continue horizontally with no orientation that we could identify as within an enclosure, structure or modulation. The sequence in question can be situated relative to certain initial conditions, and horizontal action unfolds as a loop; when we act according to certain initial conditions, we do so in a way that presumes they will be present in the next instant.

can predict with any consistency over time.

Conclusion

Probabilism attempts to refocus sociology around the concept of *objective probability*. Lessons about the nature of social groupings as continuous probability distributions over anticipated classifications and appropriations of cultural and social goods at any one point in time, can be extended, without loss, to a continuist way of conceptualizing the main groups, fields, and ecologies more generally as they continuously reproduce themselves through time.[44] But this is also tied to the orientations that actors can have at a given time, and how this itself is distributed. Probabilistic sociology maintains that a probabilistic statistical correlation in the aggregate is produced by social action that is no less probabilistic.

Quantitative measures only attach an explicated meaning to a range of probability without telling us much about the conditions that decide what those probabilities are, how actors make predictions and form expectations according to them, how those actors orient themselves to probability, and how this is maintained in action.[45] The precise nature of this is the most general question, and perplexity, of probabilistic sociology because it integrates an entirely different approach to probability and, with it, prediction. Sociologists do not have to be limited in their understanding of probability by the assumption that any engagement with it that is not statistical or

[44] Peirce, for instance, performed a lengthy study of continuum (against collective) as a potential aggregation of common states ("indeterminate yet capable of determination") (Peirce, 1898/1992, p. 247). Such an approach shares many similarities with multiple correspondence analysis to construct a continuous social space consisting of non-random distributions of probabilities grouped according to "mean points" and fractions within larger clouds (Rouanet et al., 2000, p. 11), but without giving these averages more reality than the surrounding dispersion (as Quetelet and Durkheim did). Individuals are also distributed in this space, and their position consists of a vector of objective probabilities designating their everyday practices, tastes, and position-takings. Like a continuum in Peirce's sense, a point in multidimensional space defines a potential for predictive orderliness because it pursues the incipient tendency of achieving "pair-edness" within a context of dynamic properties.

[45] Weber (1922/2019, p. 88) indicates that only in such instances where there is uncertainty can action be oriented to probability; only then can statistical measures give us any purchase on an adequate explanation. He compares objective probability with biological, geological, or climactic necessity to which probability does not apply in the same way.

data-driven is not empirically valid.

The framework proposed in this article seeks to be as bold as, we believe, the times demand in our field. Data science holds promise for the sociological project, but that project has always been different from data analysis. This challenge forces us to state our differences as unambiguously as possible. Probabilistic sociology is a way the distinctiveness of sociology can be maintained, while appreciating the possibilities opened not only by the promise of data science, but also a different revision of the inherited 19th-century view of probability, namely predictive processing, the current vanguard of probabilistic thinking in the cognitive and neurosciences.

Probabilistic sociology can ensure sociology avoids the limitations of nominalism. The entities that sociologists deal in, speak for, and credit with giving society its texture and structure are ranges of probability to which, in various ways, social action reveals its orientation. Objective probabilities of the kind most familiar to natural scientists do not require our direct orientation to be maintained in the same way the probabilistic social world does. The subjective orientations sociologists glimpse and reveal in social action must find a corresponding objective probability, somewhere—outside of purely pathological cases.

Our methodological toolkit can be calibrated to draw attention to this novel loop and the myriad ways that prediction shows up as an observable part of action. Sociology can find new points of orientation on this emerging horizon, beyond the tired rehearsal of structure, agency, and culture, outside the parameters established by data-analytic techniques that decide by fiat what counts as collective, objective, and probabilistic. We can realign our discipline along emerging fronts concerning how probability is used, discovered, and conceptualized.

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