

Narrative Inference with and without Statistics: Making Sense of Economic Cycles with Malthus and Kondratiev

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1. Drawing Inferences from Statistics: A Very Brief History

Statistical inference of the “classical” kind is bound by strict rules. It relies not just on numerical information but also on the fact that the set of observations is known to follow certain kinds of rules in terms of “sampling” and that the observations have come from a specific type of “population.” Having such knowledge, or reason to believe it, was taken to support inferences to answer *what* questions in the nineteenth century, such as to provide the true measure of the path of a heavenly body (Carl Friedrich Gauss’s question and the development of the law of error and least squares measure) or to find the average and distribution of deviancy in a population (Adolphe Quetelet’s questions and the transfer of the error law from natural to human and social sciences). Thenceforth, such statistical thinking reframed the natural and human/social error curve into the biological “normal distribution” and measures of “co-relation” (to answer Francis

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Galton's / Karl Pearson's biometric questions with a measure of covariance).¹ The development, in the twentieth century, of statistical inference rules of hypothesis testing involved formalizing scientific questions in ways that extended the inference domain to go one step beyond correlation to imply (possibly) causal relationships, and so potentially to answer *why* questions about the relationships between things.

These final moves to probability-based inference were understood in the early twentieth century to be fraught with difficulties for the social sciences, particularly economics, given both the nonsample nature of much economic data and the peculiar difficulties of time-series data. For economics, Trygve Haavelmo is credited as the source of a probability revolution prompting the gradual introduction of formal statistical inference methods and rules into economics.²

This short account raises the question: what was inference like before the rule-bound, probability-based, statistical inference of the twentieth century? One relevant element that has been well researched in the history of science is the attention paid in the early modern period to careful observation, often in extended scales over time, space, or institutions, in order to collect information *en masse* (see Pomata 2011). These observational activities in medicine, meteorology, commercial currents, and other phenomena of the natural and human worlds created pages, folios, and archives of "evidence." While recording such "observations" was often thought sufficient in itself, speculation about their meaning was not always considered proper until the eighteenth century, when observation and conjecture became viable partners in the tool box of natural philosophers (see Daston 2011).

There remained the problem of how such an observer/collector might figure out ways to abstract, generalize, or extract knowledge from such a mass of observations, that is, how to learn from, or draw inferences from, those observations. In one of the few explicit historical treatments of this question for economics, Harro Maas (2011) outlines how political economists of the late eighteenth to the late nineteenth century made arguments from observations. I take his account to suggest three modes: *generalizing*

1. These developments are broadly interrogated in two separate monographs (Porter 1986; Stigler 1986) and in the two volumes that came out of "the Probabilistic Revolution project" (see Krüger, Daston, and Heidelberger 1987; Krüger, Gigerenzer, and Morgan 1987). These writings offered new starting points following a more dispersed earlier literature on the history of statistics. This short survey history of the problem leaves aside issues of classical probability vs. Bayes's alternative, the history of the latter going back into the eighteenth century (see Daston 1988).

2. See MacKenzie 1981 on social science vs. biometrics and Morgan 1997 on correlation; on the introduction of probability thinking into economics, see Morgan 1987, 1990; Biddle 2017; and the editors' introduction to this volume.

from many observations (William Stanley Jevons following John Stuart Mill's argument about induction); *synthesizing* heterogeneous observations (taking John Elliott Cairnes as the exemplar); or *picking out particulars* for individual attention (with Richard Whately as a relevant figure). These three modes of learning from observations suggest that the nineteenth-century inference menu offered a variety of flexible practices according to the questions and materials at issue.³ This article suggests a fourth mode of inferential reasoning from observations based on *narrative making*.

2. Narrative Inference?

I use the term *narrative inference* to label an inference from some empirical materials that involves the construction of a narrative to fit the observations together *and/or* an inferential statement that draws on narrative. First, it is important that the word *narrative* here does not refer just to the use of words but involves a much stronger claim. There are many definitions of narrative available, and some of them focus on its structure, such as a narrative account involves a “beginning-middle-end” or a “change of state between beginning and end.” It is difficult to pin down a well-honed definition of narrative that works for all sites of inferential argument in the sciences. But there is an umbrella notion that works across a range of scientific contexts in which narrative is used in scientific argument, reasoning, inference claims, descriptions of relationships, and so forth. This is that: narrative functions to order and relate materials together, possibly very heterogeneous materials (rather than the similar ones of statistical inference). Narrative accounts, in both deductive and inductive modes, are particularly found in scientists' account of relations and events that occur through time. My use of the label points to a wider and generic reliance on, and usage of, narrative, not just in economics but in many fields of science, and where connections are spatial or causal, as well as those made across time.⁴

3. Maas in a number of papers (e.g., 2021) also implies that this nineteenth-century flexibility also exhibited a mash-up of deductive and inductive reasoning—a point that comes back later in my considerations of Malthus's reasoning.

4. Time sequencing of the elements is not necessary for an encompassing definition of narrative for the sciences: the events could be spatially or conceptually sequenced. The critical difference is that a *narrative* is about the connections in the sequence, in contrast to a *chronicle*—whose events are sequenced in time but are otherwise unconnected. Cycle and developmental accounts are only two kinds of typical circumstances in the sciences where narrative thrives, and this is not a claim that narrative will thrive in all sites in all sciences (see Morgan 2017; Morgan and Wise 2017; and Morgan, Hayek, and Berry forthcoming).

Nevertheless, it is evident that narratives often do play an important role in two domains of inferential reasoning in sciences precisely where the nature of the subjects under study involved changes over time and where the kind of time-connected evidence they produce both lend themselves to narrative modes of reasoning. One is the natural historical sciences, and the other is sciences that study the development of phenomena. The former—geology, paleontology, evolutionary biology, and so forth—are sciences that depend on traces of evidence that need to be time ordered and put into relation with each other to provide explanatory accounts. Narrative functions almost “naturally” in these contexts, just as for the historical phenomena of social sciences (including development economics). Narrative also functions in the same self-evident way when scientists are dealing with dynamic phenomena where the events (such as, development stages) follow one another in regular sequences; where the causal links between events may be ambiguous or irregular as well as clear and regular; where the time-dynamic relation between the elements of the sequence are difficult to discern; or where those relations may be complex or interactive. Thus the circadian rhythm, the developmental cycles found in the animal kingdom, the circulation of blood, the seasonal patterns of life, and so forth—just as cycles in economic life—all rely on dynamic processes that can well be narrated and might even be best explained in a narrative account of the internal relations of a phenomenon to suggest why and how it appears repetitive, rather than because it is a simple pattern of output response that can be matched to an input.

Narrative forms used at such sites provide not just a means or method of communication like a piece of rhetoric, any more than the use of metaphors or analogies in the sciences are just rhetorical in function. Just as analogical reasoning in sciences has proved critical to scientists’ understanding of a phenomenon, so narrative reasoning and narrative inference have proved important to explanatory accounts of certain kinds of phenomena (see Morgan forthcoming; Morgan and Wise 2017). Both forms of argument, analogy and narrative, may be deeply associated with the scientific accounts of the identity and behavior of phenomena, rather than simply glosses on their expression. But to argue that these means of expression are not merely rhetorical does not interrogate what epistemic work they do in the sciences and how they do that work. For the use of narratives in economics, this work is not self-evident.

It may help to unpack the history of economists’ use of narrative in their arguments in order to understand what is involved at different sites and see how the narrative form has fulfilled different roles. Narrative arguments in

twentieth-century economics are sometimes quasi-theoretical or deductive arguments.⁵ One kind of example is found in the imagined scenarios that accompany particular games in game theory, such as the narrative that defines the rules of the game for the two prisoners in their dilemma game—where, without the narrative element, the rules remain unspecified, and so the relative numbers in the game matrix remain unexplained and unconstrained. These narratives are important inputs into setting up the problem, without which the deductive apparatus cannot work. Another kind of deductive usage is found in the narratives that accompany model simulations, epitomized in Paul Samuelson's (1938) first ever numerical simulation of a Keynesian macro-model. Choosing different parameter values in the model created different outcome cycles (explosive or damped) over the simulated time horizon. Each of these provided a narrative of what would happen in this little model world, and Samuelson used these to explore the possible event sequences that might come from government actions in such a model. In both cases, but in different ways, the narratives formed part of the identity of the model—the kind of Keynesian stories consistent with Samuelson's model and the kinds of outcomes consistent with the narrativized rules of the game situation.

The term *narrative inference*, however, points to economists' empirical arguments. It is in relation to empirical elements, not just data series, but numerical or qualitative characteristics more generally, where the terminology of *narrative inference* may prove apt. And we might expect such usage to be most evident in dealing with cyclical and developmental phenomena. Exploring this usage lies behind the choice for this article of two key cases where economists used statistics in accounts of cyclical phenomena in the economy before statistical inference became formalized: Thomas Robert Malthus (1766–1834), writing at the turn of the eighteenth and nineteenth centuries; and the Russian economist Nikolai Kondratiev (also Kondratieff, Kondrat'ev; 1892–1938), working in the early twentieth century. These exemplary cases both employed narratives but in very different ways.

Setting the narrower historical agenda is important. Malthus and Kondratiev both sit within the mainstream history of economists' efforts to understand economic cycles, in which statistical inference has appeared in a variety of forms, only some of which have needed, or used, narrative.⁶ Thus, Jevons's late nineteenth-century sunspot theory was initially supported

5. Morgan 2012 (chaps. 9 and 6, respectively) explores the following two examples mentioned in the text here.

6. The following examples all come from an earlier study, Morgan 1990.

by a numerical inference based on matching the periodicity of the two statistical series on trade and sunspots. It was only subsequently filled in (because of the shakiness of numerical matches in cycle lengths) with his narrative inferential account that linked data on sunspots, via agricultural output in India, with data on India's trade with England, to indicate a causal mechanism behind the trade cycle. Warren M. Persons (1919: 8) had seen the statistics of business cycles that he developed at Harvard as representing a "confused conglomerate growing out of numerous causes which overlie and obscure one another," and so forbore to explain or narrate them. In the interwar period, Ernst Wagemann at the Berlin business-cycle institute—as others at other national institutes such as Jan Tinbergen at the Dutch institute—developed Persons's methods to create appropriate national "barometers." These nation-based statistical packets of information were designed to give relational content to predictive inferences through specifying—in narrative terms rather than in formal inference terms—the causal connections to be made between the sectors of the economy.⁷ Tinbergen (1939) used both theorizing narratives and empirical narratives in the first macroeconomic modeling; significantly, both of these were represented not in words but in visual form. For the theory narratives, he used small, time-labeled, causal-arrow diagrams (visual narratives indicating the dynamic structure of the theoretical model); for the empirical narratives, he used graphic techniques of regression that pinpointed critical causal factors (in a visual narrative account of the empirical record of the 1920s through to the Great Crash). Even when these economists used formal numerical or statistical inference procedures, they often relied on narrative forms of reasoning in pinpointing causal connections between the data series under discussion. Here, narrative reasoning was not just a substitute for lack of technical apparatus but a necessary complement, for the simple correlation or periodic-based analysis of these days from Jevons to Tinbergen did not offer insight into the causal interconnections between elements in the economy (see Morgan 1997).

This all changed after the 1940s probabilistic revolution, and the establishment of clear criteria to ensure the reliability and integrity of the statistical inference process was associated with that change. And, then, from the late twentieth century, inference became mechanized, that is, inference statements about the value of a parameter and the statistical test of its significance typically became the output of a computerized statisti-

7. See Lenel, this volume.

cal package. And when data came ready-prepared (downloaded from some source or other), this smoothed over, indeed often hid, important questions about the observations (the difficult practices of collecting, collating, cleaning, and abstracting data). All this reduced the need to tease out the relevant inferential questions (about what was to be aligned with what, and how, in statistical models). Modern practices often lose these significant parts of the sequence of drawing inferences from evidence.⁸ In addition, as Deirdre McCloskey warned long ago, such inferential output statements report only statistical significance, not economic significance. Narrative, usually only present in the seminar room and not in journal publications, came to play only a post hoc role in exploring the economic significance of that statistical inference statement with respect to questions of interest about the phenomena. This narrative moment became a narrative exploring what the statistical inference answer meant, not a narrative of how that answer had been gained; it became a narrative about the possible ontology of the world, not a narrative engagement in the epistemology of investigation.

This foray into historical and current practices of economists points to the problematic at issue in this article: What was involved in the earlier informal processes of inference that economists relied on? How were inferences drawn from a mass of empirical materials? As noted earlier, Maas suggested three modes of working from observations: by generalizing, synthesizing, and picking out particular cases. These processes did not need to involve narrative or that the final outcome statement provide a narrative, though narrative elements might accompany any of those three modes. But (as suggested above), for any phenomenon involving a time process such as cyclical and developmental processes, the narrative form has obvious benefits for expressing and for explaining the outcome and so may well play a dominant role in inference. And while narrative as a noun may work well as the format for an outcome account (i.e., expressing the cyclical pattern, or giving an account of the development process), narrative needs to take on verblike qualities to play a role in the activity of making the inference, that is, in the process of drawing an inference. Thus I propose we need something like *narrative making* to express the way a

8. Of course, there are always matters of judgment, alignment, and so forth where subject matter expertise is critical to drawing a statistical inference: our two special issue editors, Jeff Biddle and Marcel Boumans, have opened up this agenda in their past writings (see their introduction, this volume, for references).

scientist pulls together and combines the elements involved to make sense of and construct an account of their phenomena.⁹

Once we recognize that this narrative-making process occurs, we also see why it has not just been associated with time-based phenomena. We can see this narrative making in habitual form in, for example, the way economists use the terms *principles* or *laws* of supply and demand in their empirical work, where those laws are made relevant for, or *applied to*, different kinds of markets with different interactive processes or different degrees of competition in order to give adequate accounts of the phenomena. We see such narrative making both in Alfred Marshall's arguments about supply and demand in interaction with his observations about specific or particular markets and in late nineteenth-century attempts by US economists to make sensible analyses of railways and other natural monopolies and so design their regulation. We can also see narrative making in the slightly later (but still prestatistical inference) econometric work of the 1920s on agricultural markets, where econometricians used narrative forms to make sensible inferences from the empirical cobweb diagrams found for specific markets. But caution is needed: narratives at such sites may not all be cases of *narrative inference*. In some cases narrative making involves reasoning that enables inferences for specific empirical sites about the actions of the laws—thus *narrative inference*. Other times, the narrative element serves as something more like an illustration—critical in making the point but not an inference from statistical observations, as seems to be so in the case of Adam Smith's many well-known micro-narratives that attend his account of the division of labor.

There is one other important historical point about inferences from *statistics* that is relevant for economics from at least the eighteenth century into the mid-nineteenth century—that the original root notion of the label *statistics* is information on the *state*, not necessarily quantified.¹⁰ Two elements are important here. First, the cut between numerical information and other observable or well-attested information held less power in the late eighteenth century, in the time of Malthus, than it came to in the early twentieth century, the time of Kondratiev. Thus for economists of the past such as Malthus, state information in both modes—numerical and qualitative—offered equal grist for their inferential mill. Second, the

9. See Morgan 2017, and forthcoming, for my account of what is involved in this process, which I express as one of *colligation*.

10. With particular relevance for the history of economics, see Nikolow 2001.

state refers not to the government but to the kingdom and its many descriptions and characterizations.

Given this broad terrain of what constitutes statistics, and so statistical inference, how do both these economists' reasonings look in hindsight? For both Malthus and Kondratiev, statistics mattered to establish the phenomena of oscillations. Both used narrative reasoning in their studies of cycles, but as I show, at different points and in different ways. This article thus expands Maas's already generous account of the preformal epistemological practices of arguing from observations to include narrative resources and narrative reasoning.

3. Malthus on Economic-Demographic Oscillations

Malthus's writings on population have proved to be some of the most broadly influential works in the history of political economy. His argument that populations will always grow faster than the supply of food over the long term, exposing humanity to the dual "checks of misery and vice," led Thomas Carlyle to label Malthus's account "the dismal science," a moniker that attached itself to classical economics through the nineteenth century. The epithet referred not only to the grip that Malthus's predictions gained over the popular imagination (prompting nineteenth-century cartoons of a Malthusian monster devouring children) but also to the miserable prognostications of later classical economics, especially the gloom of "the stationary state"—when economic growth ran dry.

Though Malthus lived well into that period, his tract "An Essay on the Principle of Population" (1798) was written in the late eighteenth century, at a time recognized by fellow intellectuals as one of scientific discovery, utopian dreams about the social system, and romantic ideas about the future of humanity. He argued against these roseate views of the future, not because he wanted to be gloomy, or because he was not forward-looking, but because he just could not see a way out of the population problem revealed by his knowledge of the statistics of the day and of his analysis. He shared, however, the vigorous mode of argument of that late eighteenth century and purposefully used it not just to celebrate "the blazing comet" of the French revolution but to make fun of, and undermine, the speculative reasoning of his day. By contrast to those revolutionary and utopian writings, his own political economy was couched in the language of "disinterestedness" (Keynes 1933), surely designed to provide credibility to his

account. Here is how Malthus ([1798] 1976: 15–16) posed his question of interest and his commitment to reason with evidence:

It is an obvious truth, which has been taken notice of by many writers, that population must always be kept down to the level of the means of subsistence; but no writer that the Author recollects has inquired particularly into the means by which this level is effected: and it is the view of these means which forms, to his mind, the strongest obstacle in the way to any great future improvement of society. . . . He professes to have read some of the speculations on the future improvement of society in a temper very different from a wish to find them visionary, but he had not acquired that command over his understanding which would enable him to believe what he wishes, without evidence, or to refuse his assent to what might be displeasing, when accompanied by evidence.

It is worth pausing on this issue of speculation and the rhetorical contrast that Malthus uses, for rhetoric is never *mere*, as he well recognized.¹¹ Malthus wanted to distance his own scientific arguments—reasoning with evidence—from the characteristics of argument made by those “speculative philosophers . . . who preach up ardent benevolence and draw captivating pictures of a happier state of society . . . or as wild and madheaded enthusiasts whose silly speculations and absurd paradoxes are not worth the attention of any reasonable man” (17). Here is Malthus, the reasoning scientist, complaining about such speculative philosophers:

A writer may tell me that he thinks man will ultimately become an ostrich. I cannot properly contradict him. But before he can expect to bring any reasonable person over to his opinion, he ought to shew that the necks of mankind have been gradually elongating, that the lips have grown harder and more prominent, that the legs and feet are daily altering their shape, and that the hair is beginning to change into stubs of feathers. And till the probability of so wonderful a conversion can be shewn, it is surely lost time and lost eloquence to expatiate on the happiness of man in such a state; to describe his powers, both of running and flying, to paint him in a condition where all narrow luxuries would be contemned, where he would be employed only in collecting the necessities of life, and where, consequently, each man's share of labour would be light, and his portion of leisure ample. (19)

11. Malthus's words become a salient resource for later evolutionary theorists, with Darwin picking up his phrase “the struggle for existence.”

This passage against speculation immediately preceded his own population account, which served to show by contrast what a carefully reasoned and reasonable account based on evidence (as he sought to provide) should be.

Malthus is well aware of the difficulties in making a good scientific argument, and there are two elements in his argument: laws and evidence. The laws, he understood, must be reasonable conjectures about the world as it is—not utopian speculations about progress or utopias. He believed his two laws could hardly be argued with:

I think I may fairly make two postulata.

First, That food is necessary to the existence of man.

Secondly, That the passion between the sexes is necessary and will remain nearly in its present state. (19–20)

But he then continues with two further *factual* or *empirical* laws that he later sought to justify with evidential claims and by careful reasoning that explored the implications of these two in creating checks to population growth:

Assuming then, my postulata as granted, I say that the power of population is indefinitely greater than the power in the earth to produce subsistence for man.

Population, when unchecked, increases in a geometrical ratio. Subsistence increases only in an arithmetical ratio. A slight acquaintance with numbers will shew the immensity of the first power in comparison of the second.

By that law of our nature which makes food necessary to the life of man, the effects of these two unequal powers must be kept equal.

This implies a strong and constantly operating check on population from the difficulty of subsistence. This difficulty must fall somewhere and must necessarily be severely felt by a large portion of mankind.

While the two earlier laws of nature seemed unassailable, Malthus sought to support the two latter ones of the inequality in population growth and the power of the earth to feed them by “experience, the true source and foundation of all knowledge, invariably confirms its truth” (21).¹² Malthus thus turned his account away from the deductive mode toward a more

12. The terminology of experience becomes closely aligned with that of experiment and features strongly in the empirical and inductive work of gathering information from observations in the early modern period into the eighteenth century (see introductory essays by Pomata and Daston in Daston and Lunbeck 2011).

inductive mode in his chapter 2, and here is where we find his arguments meeting up with evidential claims in statistics as information about the state of the kingdom, rather than statistics as numbers. And “the test of experience” (16), which he relies on, is shorthand for *evidence*: knowledge of particulars from varied sources, which are relevant for understanding the demography and economy of his day and their histories.

It may be thought that Malthus picked out of thin air his claim that population—unchecked—grows in geometric ratio. But he justifies this by referring to the experiences of a population least checked by means of subsistence, that is, to certain contemporary populations in the United States. Such evidence suggested that population can double every fifteen years, but he takes a somewhat milder rate (and thus more conservative outcome) of doubling in twenty-five years, that is, a “geometric ratio.” On the other side, the arithmetic growth of food output is less obviously sourced, but Malthus was living through the agricultural revolution in Britain, with experimental farming investigating the efficacy of fertilizer application, and “spade husbandry” to increase output per acre, as well as extending cultivation into unused lands. Some of these changes reduced usable food supply (e.g., when enclosure replaced arable-crop lands with animal pastures). More important, he understood that these changes had limits. These changes, and their effects, were hotly debated in the political arithmetic discussions of the day, not least in the letters between David Ricardo and Malthus.¹³

Such attested knowledge buttressed a nicely framed arithmetic argument to prove the greatness of the problem, and thus how the *law of necessity* (the requirement for food) is found reflected in an *empirical law* or knowledge from “experience,” which reflects these checks to population growth. Even though Malthus and his work are well known to historians of economics, I quote his discussion at length, both so that we can see the reasonableness of his argument chain, and how his competent use of numbers fed into what might be counted as factual, and what inferences followed from his numerical arguments and evidence. The reasoning or argument takes a narrative form that traces out, from the combined assumptions and evidence, the implied sequence of events over future years:

13. See chapter 2 in Morgan 2012, both for the narrower discussions between the two and the wider discussions and evidence used in political economy of the time.

In the United States of America, where the means of subsistence have been more ample, the manners of the people more pure, and consequently the checks to early marriages fewer than in any of the modern states of Europe, the population has been found to double itself in twenty-five years. . . . a geometrical ratio.

. . . Let us now take any spot of earth, this Island for instance, and see in what ratio the subsistence it affords can be supposed to increase. We will begin with it under its present state of cultivation.

If I allow that by the best possible policy, by breaking up more land and by great encouragements to agriculture, the produce of this Island may be doubled in the first twenty-five years, I think it will be allowing as much as any person can well demand.

In the next twenty-five years, it is impossible to suppose that the produce could be quadrupled. It would be contrary to all our knowledge of the qualities of land. The very utmost that we can conceive is that the increase in the second twenty-five years might equal the present produce. Let us then take this for our rule. . . . The most enthusiastic speculator cannot suppose a greater increase than this. In a few centuries it would make every acre of land in the Island like a garden.

Yet this ratio of increase is evidently arithmetical.

Let us now bring the effects of these two ratios together.

The population of the Island is computed to be about seven millions, and we will suppose the present produce equal to the support of such a number. In the first twenty-five years the population would be fourteen millions, and the food being also doubled, the means of subsistence would be equal to this increase. In the next twenty-five years the population would be twenty-eight millions, and the means of subsistence only equal to the support of twenty-one millions. In the next period the population would be fifty-six millions, and the means of subsistence just sufficient for half that number. And at the conclusion of the first century the population would be one hundred and twelve millions and the means of subsistence only equal to the support of thirty-five millions, which would leave a population of seventy-seven millions totally unprovided for. (Malthus [1798] 1976: 22–23)

Clearly such a future outcome is not possible; it is no more credible than that man should turn into an ostrich. But this is indeed his point—he does not claim the incredible outcome as a realistic possibility; rather, he uses a narrative to follow the inference chain step by step to show how you get to such an impossible outcome. In later chapters, Malthus uses similar modes

of narrative reasoning to pinpoint the problems in other incredible arguments of his day, particularly by William Godwin, that humankind would become ever more perfect and society attain a state of utopian bliss!

For Malthus, something must be happening that stops the population growing beyond the means of subsistence. And indeed, for Malthus the economist, as opposed to demographer, the clever part is still to come: namely, his explanation of the processes by which the checks to population occur. Again, his explanation takes a narrative form, following the interactions of demographic processes with economic ones. In Malthus's account, the actions of the two empirical laws (population growth and food supply) working together create oscillations between experiences of "severe distress" and "tolerable comfort," as the actual population grows first more, then less, than the output of food:

We will suppose the means of subsistence in any country just equal to the easy support of its inhabitants. The constant effort towards population, which is found to act even in the most vicious societies, increases the number of people before the means of subsistence are increased. The food therefore which before supported seven millions must now be divided among seven millions and a half or eight millions. The poor consequently must live worse, and many of them be reduced to severe distress. The number of labourers also being above the proportion of the work in the market, the price of labour must tend toward a decrease, while the price of provisions would at the same time tend to rise. The labourer must therefore work harder to earn the same as he did before. During this season of distress, the discouragements to marriage and the difficulty of rearing a family are so great that population is at a stand. In the mean time the cheapness of labour, the plenty of labourers, and the necessity of an increased industry amongst them, encourage cultivators to employ more labour upon their land, to turn up fresh soil, and to manure and improve more completely what is already in tillage, till ultimately the means of subsistence become in the same proportion to the population as at the period from which we set out. The situation of the labourer being then again tolerably comfortable, the restraints to population are in some degree loosened, and the same retrograde and progressive movements with respect to happiness are repeated. (24–25)¹⁴

14. Malthus concedes that the Marquis de Condorcet has foreseen such oscillations between "good and evil," but Condorcet was convinced such an outcome was so far in the future it was not of serious concern; he did not, however, fill in the details of the demographic-economic interactions in the way Malthus was able to do.

Revealing the interactive sequences between demographic and economic processes required Malthus to make a *narrative argument* of the kind that economists love to make, just as much then at the turn of the nineteenth century as they do now in the early twenty-first century. Such arguments were used then to show how their laws work out in the activities of the world just as economists now use narrative arguments with their models to explore how that mini economic world might work. And these narrative economic arguments—then as now—may be applied to the real world in the current time, to the counterfactual world for the past, or the imagined world for the future just as Malthus did.¹⁵

It is especially worth noting that Malthus's narrative was—in a particular sense—not about the observed world but about the nature and processes of economic life that sat behind the observations, a narrative about what was really happening, not what was on the surface, creating a mode of argument that we can recognize as habitual in the way economists still grapple with the empirics of our aggregate world. As Malthus remarked: “This sort of oscillation will not be remarked by superficial observers, and it may be difficult even for the most penetrating mind to calculate its periods” (25). We do not *see* these oscillations directly in our histories, he argued, first because our histories are histories of the rich, not the poor where these checks will be at work, and extremely patient observation would be needed to discern them. Second, the oscillations would be irregular because of many other relevant but “interrupting causes” (such as new manufactures, agricultural improvements, wars, plagues, institutional changes) and “so much friction, and minute circumstances occur in practice” that it is impossible to see even what has stood “the test of experience” (18). (This argument parallels Mill's later claims about our histories not revealing our economic realities because of disturbing causes at work.) Malthus's third reason, and his most important economic insight, relied on recognizing “particularly, the difference between nominal and real prices of labour, a circumstance which has perhaps more than any other contributed to conceal this oscillation from common view” (25). For “common view,” we can understand that this hides the oscillations not only for experienced observers but even to those laborers in the economy whose experiences this account narrates.

We might see and assess more clearly what narrative is doing in Malthus's reasoning by drawing the contrast between his treatment and

15. I analyze the way economists use model narratives in counterfactual and imagined world cases and in historical/empirical cases in Morgan 2014 and 2017.

discussion of the same problem by Adam Smith. Rather than advance any explicit “laws” of demography (in *Wealth of Nations*, bk. 1, chap. 8), Smith draws on anecdotal kinds of evidence (“I have frequently been told”) and generality claims, whose observational status is unknown (“barrenness, so frequent among ladies of fashion, is very rare among those of inferior station”). In contrast, Malthus referred to the facts reported from specific times and places. On the basis of his more cursory observations, Smith advances an explanation of the checks to population based on a class division. In his demographic account, the rich who have the means but have small families experience no checks, while the poor produce large numbers of children but the check of poverty keeps their families small. His subsequent economic argument is not an inferential process based on evidence but a simple deductive one using the laws of supply and demand: labor is in a market, just like “any other commodity,” with periods of over- (and under-) supply. And the possibility of oscillations in the demography is not explicitly discussed as a phenomenon of this market, while any more serious interaction of the demographics and the labor market are hardly explicated. Smith’s text lacks both the analytic subtlety of Malthus’s reasoning and the observational credibility the latter brought to bear on the topic. Smith’s text cannot be analyzed so easily into evidence claims and law claims and thus not easily open to argument either!

This very brief comparison of Malthus with Smith suggests that the credibility of Malthus’s arguments rests not just on matters of rhetoric but on a much closer and clever argument that brings inductive statistical (broad notion) and deductive modes of reasoning together into a narrative that traces the dynamic interaction between demography and economy and, from that, of what we should reasonably expect to see and equally reasonably why we cannot do so. It is evidently difficult to make any clear distinctions between induction and deduction in the economic argumentation of Malthus. Smith and Malthus argued about the topic in very different ways: whereas casual observation and short deductive sequences are clear in Smith, what is happening in Malthus is much less clear, for his inferences based on his empirical claims and his arguments from laws are not so much integrated but bleed like textile dyes into each other. While it would be awkward to claim Malthus’s argument from evidence as *statistical inference* in our formal modernist terms, the way he dealt with the problem, and the way he used narrative in drawing inferences from empirical evidence, shows a considerable degree of sophistication, which prompts further analysis.

As we see with Malthus’s arguments: his imperative is for careful reasoning in process and a reasonableness of outcome. The latter is defined

as consistent with our knowledge, thus his appeal to, and then contrast, throughout his arguments in his book, between the knowledge from *experience* and the utopian outcomes found from *speculation*. These give him criteria for his inferential accounts but provide neither recipes nor rules for drawing inferences. Narrative making provides these means: *narrative making takes him from the evidence to the expression*.¹⁶ What this entails is a bit tricky, but it is important to keep in mind the difference between the inferential process and inference outcome.

Malthus's account of the interaction between demography and economy with respect to labor and poverty has a narrative form—events follow one another in interaction, and the account is explanatory, for it joins up the elements rather than just recognizing a sequence, and it melds them into an account of an oscillatory phenomenon. It is necessarily complicated because the elements interact with each other: the sequence is neither linear nor simple. It relies also on the evidence of activities we could not easily see, hidden, but revealed by economists' superior analytic insight (for example, into the difference between the nominal and the real wage rate), to discern the causal chain in events, and their implications wrapped into the narrative account. Much is involved here. First, the narrative Malthus tells does more work than just expressing the evidence in a narrative in the sense that Malthus must first construct his narrative in such a way that it gives a joined-up account of the evidence; this narrative making marks the process of making his inference. Second, the narrative he tells is not just an outcome expression of the evidence. Rather, in his narrative-making process, Malthus has integrated the lawlike postulates along with the statistical evidence to give an account of the interactive process between demography and economy. This is perhaps a true form of inference—inference is drawing an outcome from the evidence with respect to a claim, lawlike or one-off, about something. It not just that the claim and evidence are not separated out—as later rules require—into a hypothesis about the claim and an inference outcome.¹⁷

16. Thanks to Tom Stapleford, who suggested at the workshop that “narrative takes us from evidence to expression”—but note that in my rendering of this, the important words are verbs so that my preferred version of the maxim is “making the narrative takes us from evidence to expression,” i.e., the process takes us to the outcome, where that process involves narrative making.

17. The more recent version of statistical inference might be well be expressed using the metaphor that Aashish Velkar suggested at the workshop, namely, “that narrative functions as a bridge that takes you from evidence to outcome.” If the process of making the narrative is a process of linking theories and empirics, then it does provide a means of bridging, but since in Malthus's work, the narrative also operates as a process *integrating* the two sides, the bridge analogy remains a little problematic. However, it may well be that for other economists, who keep their empirics and theories/laws/models rather separate, narrative does work as a bridge.

Rather, Malthus's narrative making does take us from evidence to expression, but what is expressed is not a simple outcome statement but an integrated process account showing how the world works.

This account of Malthus's inference process may make more sense when contrasted with Kondratiev's use of narrative in conjunction with long-wave cycle accounts, for there we are unambiguously into inductions based on numerical evidence. And when induction refers to inference from observable information (on the state), such statistical inference can quickly find its place on a continuum from older forms of informal inference from evidence as in Malthus, to numerical claims in Kondratiev, and later to formal statistical inference. At the same time, the Kondratiev case makes the problem more complicated because of the need to pin down claims with respect to numerical observations in a way that Malthus, with his limited numerical evidence, avoided.

4. Kondratiev on Long Waves

It is well recognized that economists across the international community became obsessed with the study of economic cycles in the late nineteenth and early twentieth centuries—short ones for stock market activity (“Kitchins”), middle-length ones for the “business cycle” (associated particularly with W. C. Mitchell),¹⁸ the investment cycle (“Kuznets”), and “long waves” (“Kondratievs”). These studies were instrumental in developing generic methods of studying time series statistical data in order to *pin down* these phenomena. Compared with the late eighteenth-century account Malthus gave of his demographic-economic oscillations, this was extremely numerically data-heavy work. For some of those involved, this was an entirely empiricist project, inference from statistical data to establish patterns, but not formal statistical inference according to rules. For others, such as Kondratiev, it involved arguments about the dynamic mechanisms involved in long waves, though he stopped short of claiming “a theory of long cycles.”

Kondratiev has his name attached to these long fifty-year cycles, for though there were others speculating about their existence before him, he was the first to conduct sustained empirical data collection, data preparation, and data analysis in order to establish them as a valid phenomenon of the economy. Kondratiev was the first director of the Conjecture Insti-

18. See Stapleford, this volume.

tute in Moscow, one of a group of international cycle research institutes in the 1920s. These economists followed one another's work closely, their papers were quickly "abstracted" into other language publications across the community, and they, including Kondratiev, traveled abroad and met each other. Kondratiev's findings were published first in Russian in 1924, prompting detailed discussions among his Russian contemporaries (see Makasheva, Samuels, and Barnett 1998), and thence translated into German in 1926, and selected parts into English in 1935.¹⁹

Kondratiev's two main problems were, first, establishing the economic phenomenon, that is, that long waves existed; and second, giving an account of the phenomenon, which he presumed was one element in the complicated "dynamics of economic life" (made up of all these different kinds of cycles). Here is how he introduced his study in the first English translation:

The idea that the dynamics of economic life in the capitalistic social order is not of a simple and linear but rather of a complex and cyclical character is nowadays generally recognized. Science, however, has fallen far short of clarifying the nature and the types of these cyclical, wave-like movements.

When in economics we speak of cycles, we generally mean seven to eleven year business cycles. But these seven to eleven year movements are obviously not the only type of economic cycles. The dynamics of economic life is in reality more complicated. In addition to the above-mentioned cycles, which we shall agree to call "intermediate," the existence of still shorter waves of about three and one-half years' length has recently been shown to be probable.

But that is not all. There is, indeed, reason to assume the existence of long waves of an average length of about 50 years in the capitalistic economy, a fact which still further complicates the problem of economic dynamics. (Kondratiev 1935: 105)

As might be expected, establishing that long waves occurred in the economy was not so easy. The availability of data was relatively more restricted than it is now, and Kondratiev had to rely on the kinds of public data created by markets or governments (financial, foreign trade, output

19. There are several contemporary versions of Kondratiev's long waves (see Makasheva, Samuels, and Barnett 1998). The quotations here come from a 1935 partial translation by Wolfgang Stolper of Kondratiev's 1926 German paper. On Kondratiev, see Barnett 1995, 1998.

figures) and by some economists who were by then collecting long-period data and creating index numbers for certain economic characteristics (such as wages and prices). The second problem was that these data had limited past existence, and if long waves were a phenomenon characteristic of capitalism as he thought, then there was hardly, or perhaps just enough—as he showed—for 2.5 waves in full. He relied on data on France and England (as they were the most complete), and the United States, and made claims that similar timed waves were evident in other “European capitalist countries” (111).

A comparison with Malthus’s situation is useful here. First, Malthus’s arguments were about some variables that had clear economic content and meaning, even though the data were not available (his demographic ones would have been available by Kondratiev’s time, but the economic labor market ones only by the mid-twentieth century). So while Malthus could conceptualize the problem he examined, he did not have raw data, just some statistical (i.e., of the state) characteristics that he believed to be true: the arithmetic versus geometric growth rates. But it was less clear what Kondratiev was trying to get at with his data sets: what was *the economy* that was being represented in the long wave? The economy was not yet conceptualized as a macroeconomy. But the many different cycles that were being characterized and labeled in contemporary statistical work with time-series data might now be best understood under the term *economic activity*, or as Kondratiev said, “economic and social life as a whole” (111). Such life had its own multiple kinds of dynamics, and it was one of these dynamics (or cycles) that he sought. He had data on lots of different economic series, presented in graphs and tables, and they were prepared to represent the dynamics of his cycles. That is, they were turned from raw numbers into data series relevant to represent the phenomena using the by then standard Persons/Harvard techniques of secular trend removal, normalizing on population size where needed, and using moving averages.²⁰ Some of the data series can be understood as direct measurements of economic activity, or to use Kondratiev’s language, “natural elements” (production, consumption). Others that he labeled “financial” (interest rates, wages, bank deposits) we might better understand as indirect “indicators” of economic activity, using a label that was just then coming into widespread usage among those economists studying cycles. Still others he regarded as mixtures of the “natural” and “financial” (e.g., foreign trade). These different kinds of variables were used in his many tables and charts

20. Again, see Lenel, this volume.

to establish his long waves: this was statistical inference—without probability reasoning and without narrative.

Establishing the long waves was, for Kondratiev, rather separate from characterizing them, but here, too, his focus was on the empirical. Here is how he introduced and summarized the “Empirical Characteristics” of his long waves on the basis of his statistical work:

- (1) The long waves belong really to the same complex dynamic process in which the intermediate cycles of the capitalistic economy with their principal phases of upswing and depression run their course.
 - (2) During the recession of the long waves, agriculture, as a rule, suffers an especially pronounced and long depression. This was what happened after the Napoleonic Wars; it happened again from the beginning of the 1870’s onward; and the same can be observed in the years after the World War.
 - (3) During the recession of the long waves, an especially large number of important discoveries and inventions in the technique of production and communication are made, which, however, are usually applied on a large scale only at the beginning of the next long upswing.
 - (4) At the beginning of a long upswing, gold production increases as a rule, and the world market [for goods] is generally enlarged by the assimilation of new and especially of colonial countries.
 - (5) It is during the period of the rise of the long waves, i.e., during the period of high tension in the expansion of economic forces, that, as a rule, the most disastrous and extensive wars and revolutions occur.
- (Kondratiev 1935: 111)

It is to be emphasized that we attribute to these recurring relationships an empirical character only, and that we do not by any means hold that they contain the explanation of the long waves.

As with Malthus, it is in wrestling with this second problem—giving an account of the dynamic elements in his long waves—that we find narrative at work. Kondratiev’s text positioned the latter four empirical characteristics (2–5)—treated generically—into particular phases within the long waves. Thus, the timing with which these types of events occur is associated with particular phases of the long waves. So, as we can see in the quoted passage, this is a commentary on the timing of such events within the long cycles: the inference is about *what* happens, and *when*—not about *why*.

In further discussing their place in the dynamics of long waves, Kondratiev rejects each of the characteristics (2–5) as causes of his waves. Indeed, he is convinced that this has the argument backward: these factors

are *consequences*, not *causes*. They are factors that occur in a particular historical time but whose effects only come into play when the long wave brings them in, so he tells a *consequential* narrative for each factor to bring them into his account of long waves. Thus the inventions of the scientific revolution are not accidental, but they come into widespread economic activity only during the long wave of the industrial revolution; new countries or new gold fields are discovered but are pulled into the international trade and economic monetary system in a major way only because of the long wave; wars and revolutions occur because of the pressure of long-wave activities. In effect, each generic factor has its own generic narrative that knits the factor into the long-wave pattern at a particular generic phase of the wave, not as a cause of the wave but as gaining power as a consequence of the dynamics within the wave. They are part of the dynamics of the long waves but not the source of the long waves. Kondratiev's view of these empirical characteristic elements places them into his narratives as add-ons—they do not explain the long waves, they are not needed to make sense of the long waves, but they are salient characteristics of economic life that need to be connected into this major dynamic of the capitalist system.

It is perhaps unexpected that these narratives of empirical characteristics are treated generically—that is to say, he does not pinpoint particular gold discoveries, or particular wars, and attach them to particular historical points in his graphs of the 2.5 long waves. This would have seemed the obvious way to go, then as it is now, for we regularly see time-series charts with specific events (the global financial crisis of 2008–9 or the great crash of 1929–32) written on the charts. Yet each of these generic consequences had its own historically specific events that Kondratiev discussed in the text and that could have been pinned on particular time moments in his graphs. Indeed, the 1926 discussion by his Russian colleagues of his original 1924 long-wave research (as reported in Barnett 1995: 431–36) showed much willingness to affix the timing of particular examples of the various historical factors into their relevant long wave.

Kondratiev's overall text narrative did, however, claim that long waves are inherent in the capitalist system. He took seriously, as the likely endogenous cause, the long-run "replacement and increase in the stock of basic capital goods" which "occurs in spurts, and this rhythm is reflected in the long waves of economic life" (Barnett 1995: 435). As Vincent Barnett reports, this critical endogenous cause in the system came from Mikhail Tugan-Baranovsky's (Kondratiev's teacher) theorizing on the matter. Given the context of postrevolutionary Russia, this careful equivocation about

causes might have been politically astute, for it keeps the capitalist dynamics free of these kinds of historical factors as main causes, leaving the waves as endogenous to that system. However, it seems that these consequential narratives about long waves were not so politically problematic as his analysis and advice on how to stimulate economic growth and the role of agriculture in the new plan for the economy. In 1928 he was dismissed, the institute was shut down, and in 1932 he was sent to prison, where he continued with his economics writing until 1938, when he was executed.

Just as Malthus's numerical claims and associated narratives put demographic-economic oscillations into economists' agendas, Kondratiev's statistical work put long waves into the list of empirical economic cycles, and the phenomenon has been a topic for economic research ever since.²¹ Just as attention to Malthusian population crises wax and wane, Kondratiev's questions have continued to reverberate through the decades: Do these long waves exist? And if so, are the specific cycles correctly identified in time? These worries were raised by his Russian colleagues in their 1926 discussions, when they complained about errors in dating the long waves and, at the same time, doubted their existence. Over the last century, economists have focused on similar worries: correcting Kondratiev's timing of the waves; analyzing his claims about their regularity; collecting further data; developing smarter statistical techniques; and so forth. It seems that the jury is still out on their *statistical* existence: they are not yet proved either in an irregular form or as a regular periodic cycle. Equally, economists have been arguing ever since about the dynamics of the causal account—either as causes or as consequences of the inherent system of capitalism, and it has been equally problematic to tell a good generic narrative account that makes sense of the timing of such factors for both Marxist and non-Marxist economists alike. Following Kondratiev's thesis about the overall endogenous dynamics of long waves, much of the discussion has focused on the timing implications of long-term capital replacement and innovations of multipurpose (or general-purpose) technologies (i.e., steam, steel, railways, electricity, computers). In so doing, these arguments essentially turn factor 3, which Kondratiev argued was a *consequence*, into the main *cause* of the phenomenon. Significantly, and perhaps because of the difficulties in reducing these wide and deep innovations into well-defined variables with matching statistical measurements, these explanatory accounts have more often been dominated by “analytic

21. This is a very considerable literature, exemplified in Rosenberg and Frischtak 1984, and surveyed (but only up to 1979) in a “selective annotated bibliography” of nearly forty pages (Barr 1979)!

narratives,” economic explanatory narratives more closely knitted to inference from economic historical evidence than to the more rule-bound regimes of formal statistical inference.²²

5. Conclusion

Using narrative in inference relies, as do other modes of inductive scientific investigation, on beginning with questions about phenomena. Malthus and Kondratiev shared much the same question. Both were interested in probing the relational dynamics of the economic system as it manifested in cycles. For Malthus, these dynamics were given an account in his narrative reasoning; he had not the means to see the arithmetic and geometric laws directly in his evidence and, as I have shown, argued that evidence of their interactions would not be seen. In contrast, Kondratiev could display the long waves in his data graphs yet did not weave the consequences of the system into its causal dynamics, although, just like Malthus, this did not stop him arguing about those elements. Both Malthus and Kondratiev had good questions about economic cycles, and used statistical knowledge, but this proved not enough—narratives were needed to make economic sense out of that statistical information. Whereas Kondratiev added a narrative on top of the statistics to put sense on the numbers, Malthus practiced narrative inference to explain his cycles. For Malthus, the statistical information/claims were an integral part of his narrative, and the narrative was an integral part of his making sense of the numbers.

At the beginning of this article, I remarked that narrative statements serve to relate (possibly disparate) elements, event, and factors together; narrative making offers a way for scientists to explore and explain how such elements might be related together, especially where those events are already time-sequenced. So narrative making is a site where scientists can probe and fashion answers to their questions about the relational nature of events through time: are those events somehow causally related and if so, how? Making sense of statistical evidence by constructing narratives about them offers a way to draw inferences about the relations embedded in such statistical information. Narrative making in this sense offers a kind of mechanism, even an engine, for drawing inferences. Narrative statements may also serve as the mode of expression for an inference outcome statement (in contrast to an outcome expressed as the value of

22. This phrase has a particular meaning in economic history and refers to narrative accounts that meld together both theoretical and historical empirical materials (see Bates et al. 1998).

parameters and their t -statistics from a statistical program). *Narrative inference* thus refers to these situations, namely, where narrative making is a major part of the inferential process and in which narratives are used to explain and justify the inference, not just to state the outcome.²³ Since narratives are fundamentally expressions about the relationships between things, they are associated with economic explanations, rather than economic descriptions, namely, about answering questions about *why* things are the way they are rather than *what* they are: about *why* population does not grow beyond the means of description rather than *what* kinds of waves are found in capitalist economies.

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23. I thank Tom Stapleford for discussions that clarified my claims.

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