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CHARLES S. PEIRCE, ECONOMICS, AND SCIENTIFIC METHOD

BY
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CHOPE WORKING PAPER No. 2017-10

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CENTER FOR THE
HISTORY OF POLITICAL ECONOMY
AT DUKE UNIVERSITY

**RICARDIAN INFERENCE:
CHARLES S. PEIRCE, ECONOMICS, AND SCIENTIFIC METHOD**

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Abstract

Standard histories of economics usually treat the “marginal revolution” of the mid-nineteenth century as both supplanting the “classical” economics of Smith and Ricardo and as advancing the idea of economics as a mathematical science. The marginalists – especially Jevons and Walras – viewed Cournot’s (1838) book on mathematical economics as a seminal work on which they could build. Surprisingly, the scientist, philosopher, and logician Charles S. Peirce discovered Cournot before the marginalist economists and possessed a deeper appreciation of his mathematical approach. While Peirce’s contributions to economics are limited, the influence of economics on his philosophy is subtle and not well understood. In a number of fragments, Peirce, who, despite Ricardo’s lack of mathematical form, nonetheless regarded him as a paradigmatic mathematical economist, refers to “Ricardian inference,” as a fundamental contribution to scientific method. Two, perhaps complementary, options are explored as to exactly what Peirce meant by Ricardian inference. On the one hand, he associates Ricardo with the “primipostnumeral syllogism,” which is a sort of generalization to uncountably infinite sets of what Peirce calls Fermatian inference (often referred to as mathematical induction). On the other hand, he holds up Ricardo as an exemplar of the “analytical method,” which is Peirce’s name for a hybrid form connecting analogy, abduction, and induction. On either account, economics plays a larger and more fundamental role in Peirce’s philosophy of science than is generally understood.

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**RICARDIAN INFERENCE:
CHARLES S. PEIRCE, ECONOMICS, AND SCIENTIFIC METHOD**

Charles S. Peirce (1839-1914) was an American original. Perhaps most famous as the founder of the philosophical school of pragmatism, Peirce was a polymath who made signal contributions to mathematics, logic, astronomy, geodesy, psychology, semiotics, probability and statistics, and other fields. It is less well known that Peirce was not only a student of economics, but a widely read and unusually perceptive student. Carolyn Eisele, who deserves the most credit for bringing Peirce’s few, previously unpublished economic writings to light, concluded that “[i]t is apparent that Charles Peirce played a role in the attempt to find new mathematical approaches to the investigation of economic theory” (Eisele’s 1979, p. 253; see also p. 368). Eisele’s judgment is too vague, in that she never clarifies the nature of Peirce’s positive contribution. It is also too generous: Peirce paid close attention to the economists, but there is no evidence that the economists reciprocated, and there is no reason to believe that 19th century economics would have developed differently had Peirce never existed. Nevertheless, Peirce was more than a student; he contributed two notable pieces of applied economic analysis: a paper for the United States Coast Survey on the economy of research in 1876, which was probably the most sophisticated application of mathematical economics to any concrete problem written up to that time and a trenchant economic criticism of the editorial position of *The Nation* on the Spanish Treaty under negotiation in 1884.¹ In contrast to Eisele, we read the relationship between Peirce and economics as importantly running in the other direction: economics contributed to Peirce’s understanding of other matters. The

¹ We are currently writing a book on Peirce’s engagement with economics. One chapter of that book will consider Peirce’s paper on the economy of research in detail, while a companion paper to this one addresses his analysis of the Spanish Treaty (Wible and Hoover 2017).

importance of Peirce's analysis of the economy of research to the philosophy of science came to be widely recognized only in the mid- to late 20th century (see Wible 1998). We suggest that Peirce credited economics with a more fundamental importance in his account of logic and the methodology of science, owing to its role in what he called *Ricardian inference*.

Peirce's life story is fascinating and sad.² Peirce started at the top of the 19th century American intellectual world. His father, Benjamin, was the most distinguished mathematician in the United States and professor of mathematics and astronomy at Harvard University and superintendent of the United States Coast Survey. With distinguished Cambridge colleagues, such as Louis Agassiz, Benjamin Peirce founded the National Academy of Sciences. Charles Peirce was graduated from Harvard College in 1859, taking an M.A. in 1862, and a B.Sc. *summa cum laude* in Chemistry from Harvard's Lawrence Scientific School in 1863. His employment in his early career at the Harvard Observatory and with the Coast Survey was furthered by his father's patronage. His earlier scientific achievements were sufficient for him to be elected to the National Academy of Sciences (1877), nevertheless owing to his personality, social trespasses, and bad luck, his life trajectory can best be described as cycling around a declining trend. In his final, still intellectually productive years, he lacked any regular employment and only alms provided by friends and admirers, organized by the philosopher William James, kept him from literally starving or freezing to death (Ketner and Putnam 1992, Introduction; Brent 1998, pp. 303-306).

² Ketner and Putnam (1992) provide a capsule summary of some of the key points; and Brent (1998) offers a full-scale biography.

The vicissitudes of Peirce’s life resulted in many unfinished projects and a complex mass of partly completed papers and incomplete and disjointed notes.³ Peirce is best known today, perhaps, as a logician and philosopher of science, so that it is striking that Peirce attributed inferential forms that he associated with economics an important place in his logic and philosophy of science. Unfortunately, Ricardian inference suffered even more acutely from the tangles and lacunae of Peirce’s written legacy, and significant puzzles remain as to exactly what Peirce understood it to be and how and why he regarded it as so important. Our goal is to uncover as much as possible of Peirce’s understanding of Ricardian inference and to articulate one dimension in which Peirce found the study of economics to contribute to his wider philosophical projects.

1. Cournot and Ricardo

Economics today is highly mathematized – indeed, it is the most mathematical of the social sciences. It was not always so. Commercial arithmetic played a role in many early tracts in political economy. Various attempts were made to render economic ideas using geometry or elementary algebra. But economics came late to mathematically sophisticated analytical tools. The decisive shift occurred with the so-called “marginal revolution” of William Stanley Jevons in England, Leon Walras in France, and Carl Menger in Austria, which overthrew the “classical economics” of Adam Smith and David Ricardo.⁴ The key planks of marginalism were, first, casting the decisions of workers

³ This by no means suggests that Peirce was an unsystematic or disorderly thinker. Rather his circumstances prevented him from starting the core work on a number planned projects or finishing some once they were underway. Unfortunately, practices on the part of the editors of the *Collected Papers of Charles Sanders Peirce* ((1931-1958) have made Peirce appear more disorderly than he actually was. The editors of the chronological *Writings of Charles S. Peirce* (1982-2010) have gone a long way to set the record straight.

⁴ On the marginal revolution, see Schumpeter (1954), ch. 5 or Blaug (1997), ch. 8.

and consumers as individualistic, Benthamite exercises in utility maximization and, second and perhaps more importantly, framing those decisions, as well as the production and sales decisions of firms, in terms of the differential calculus. From the marginal revolution on, economics was indisputably a mathematical discipline – readily incorporating new mathematical techniques and, sometime – as, for example, in the cases of game theory and operations research – proving instrumental in their creation.

Important as the marginal revolution was to the history of economics, few historical developments are truly unprecedented, and Walras and Jevons both acknowledge Antoine Augustin Cournot (1801-1877) as having stood in the forefront of mathematical economics and, to some extent, as having anticipated marginalism. Cournot was a French mathematician, astronomer, physicist, probability theorist, and historian of science. In 1838, he published *Recherches sur les Principes Mathématiques de la Théorie des Richesses*, in which he systematically applied the differential calculus to economic analysis. Cournot can be thought of as a proto-marginalist.⁵ Where the marginalists derived a demand function from the maximization of utility, Cournot simply took the inverse relationship of demand to price as a primitive and focused instead on supply. Applying the tools of the calculus, Cournot mathematized Ricardo’s system and went substantially further by investigating what is now called the theory of the firm, showing sequentially how a monopolist, a duopolist, and higher order oligopolists would set prices and output. He showed how in the limit increasing the number of producers of a particular product would generate what is now termed “perfect competition” – the market organization when each producer can take prices as beyond their own control and

⁵ Wible and Hoover (2015) provide a detailed account of the core of Cournot’s analysis and Peirce’s reaction to it.

simply decide how much to supply given the market price. While the names came later, Cournot was the first to draw those mainstays of economic analysis the supply and demand curves.

Although the *Recherches* was the most advanced economics text of its day, it, like Hume’s *Treatise of Human Nature*, fell stillborn from the press. Virtually, no one took any notice of it. The first reviews appeared only nineteen and twenty-six years after it was published (Cherriman 1857 (see also Dimand 1995); de Fontenay 1864). And, again like Hume, Cournot later felt compelled to publish two reworked versions of his neglected masterpiece – leaving out the mathematics in the hopes of attracting a wider audience. Jevons and Walras discovered it only after their own work was well advanced; and, though they admired Cournot, their work does not really build on him. Only after marginalism was well established did Cournot receive serious consideration, ultimately contributing key ideas that have been incorporated into modern economics.

One reason for Jevons and Walras not making more substantial use Cournot’s work is simply that, even though they themselves advocated mathematical economics, they were not up to Cournot’s standard. The historian of economics Mark Blaug notes that “Walras had only the instincts and none of the techniques of a mathematician” (1997, p. 279). Jevons himself notes in the preface to the second edition of his *Theory of Political Economy* that “[e]ven now I have by no means mastered all parts of [the *Recherches*], my mathematical power being insufficient to enable me to follow Cournot in all parts of his analysis” (Jevons 1965, p. xxx).

Unnoticed at the time, however, Cournot did have one group of mathematically competent readers – the members of the Scientific Club of Cambridge, Massachusetts, a

group that included both Benjamin and Charles Peirce, as well other members of Harvard intellectual elite. In December 1871 – three years before Walras first mentions him in print and a year before Jevons had even obtained a copy of the *Recherches* (Jevons 1879, p. xxx) – the Scientific Club met to study him together. Charles Peirce, who appeared to have missed the meeting, remaining at his job with the U.S. Coast Survey in Washington, D.C., nonetheless corresponded with his father and with the astronomer, mathematician, and sometimes economist Simon Newcomb about Cournot. In these letters, as well as in a later letter to an upstate New York lawyer named Abraham Conyer and notes preserved among his papers, Peirce displays a mastery of Cournot’s economics, offering careful exposition, as well as critical analysis of key parts of the book. Peirce’s reading of Cournot is the beginning of a longer engagement with economics that is reflected, among other ways, in his frequent use of economic examples to illustrate philosophical points.

If Cournot represented the *avant garde* of economics early in Peirce’s career, why does Peirce cast David Ricardo the eponym of the particularly vital form of inference? In his *Principles of Political Economy* (1821[1951]), Ricardo, a rich, retired stockbroker and member of the British Parliament, provided the canonical statement of the classical political economy that dominated British economics before the marginal revolution. To Jevons, the essence of progress was to supplant Ricardo by embracing Cournot, “for that able but wrong-headed man, David Ricardo, shunted the car of economic science on to a wrong line . . .” (1965, p. li). Jevons opposed Ricardo’s focus on the labor theory of value and the emphasis on supply over demand, as well as regarding him as a nonmathematical economist. Cournot, on the other hand, saw himself as building on, but not replacing, classical economics; and, while aware of Ricardo’s lack of mathematical

form, nonetheless regarded him as a kindred mathematical spirit. “There are authors,” Cournot writes,

like Smith and Say, who when writing on Political Economy, have preserved all the beauties of a purely literary style; but there are others, like Ricardo, who, when treating the most abstract questions, or when seeking great accuracy, have not been able to avoid algebra, and have only disguised it under arithmetical calculations of tiresome length. [Cournot 1838[1927], p. 3]

Peirce, as we will document presently, fundamentally agrees with Cournot, but is willing to go further. Ricardo, in his view, is a fundamentally mathematical economist. The details of Peirce’s economics trace mainly to Cournot, but the spirit is Ricardo. Peirce appears to view Ricardo’s most important contribution to be Ricardian inference, but at the same time leaving considerable ambiguity about the nature of that contribution. What exactly is Ricardian inference? Our goal is to answer that question. While the answer –given the complexity and incompleteness of Peirce’s professional papers – cannot be univocal, we believe that it nonetheless casts significant light on Peirce’s philosophy of science and on the relationship of his understanding of economics to it.

2. Ricardo’s Theory of Rent

In his “Sketch of a New Philosophy” (1890) under the heading “Development of the Method,” Peirce refers to “[t]he peculiar reasoning of political economy; the Ricardian inference” (WP 8.21). In an entry for the *Century Dictionary* (1889, p. 3081) Peirce glosses “Ricardian inference” as “the mode of inference employed by Ricardo to establish his theory of rent” (WP 8.365; Ricardo 1821[1951], ch. 2). As is well known to historians of economics, Ricardo acknowledged that the theory of rent, developed in his *Principles of Political Economy and Taxation*, was originally due to Malthus (Ricardo

1821[1951], , esp. p. 5).⁶ The attribution to Ricardo, in which Peirce is not alone, is in part a recognition of the central role that the theory of rent plays in Ricardo's systematic analysis of income distribution and growth for a whole economy (Blaug 1997, ch. 4; Eltis 1984, ch. 6). Peirce also sees Ricardo as essentially the founder of mathematical economics. Ricardo

may be said to have rediscovered the reasoning of the differential calculus and applied it to the theory of wealth. All the so-called mathematical economists have done nothing which was not quite obvious after Ricardo's examples of analysis [Peirce quoted in Eisele 1979, p. 369]

Peirce explains:

The reasoning of Ricardo about rent is this. When competition is unrestrained by combination, producers will carry production to the limit at which it ceases to be profitable. Thus, a man will put fertilizers on his land, until the point is reached where, were he to add the least bit more, his little increased production would no more than just pay the increased expense. Every piece of land will be treated in this way, and every grade of land will be used down to the limit of the land upon which the product can just barely pay. [CP 4.115]

Oddly, Peirce's explanation of rent in this passage never explicitly mentions rent. Rent for Ricardo is the excess product of the more productive land relative to the land at the margin – that is, to the land that is just productive enough to pay the wages and normal profits necessary to bring it into production.⁷ All land generates the same wages per bushel of corn and the same profits per pound sterling of capital laid out, but the most productive land pays the highest rent and the least productive (the marginal) land pays no rent.

⁶ Ricardo also recognizes the independent discovery of the theory in a pamphlet published in 1815 by an anonymous "Fellow of University College, Oxford," who is now known to be Edward West.

⁷ Following Ricardo, Peirce recognizes two distinct, but logically similar margins. In the quotation immediately above, he refers to what modern economists refer to as the *intensive margin* – that is, to the increasingly smaller additions to output that result from the increasing use of inputs (labor, fertilizer, etc.). Elsewhere he refers to what modern economics the *extensive margin* – that is, to the increasingly smaller output that arises from bringing intrinsically and increasingly worse land into production. (CP 4.115; Ricardo 1821[1951], pp. 70-72).

Despite the agricultural illustration, Peirce has already generalized Ricardo’s message to apply to all forms of production. He reads Ricardo through Cournot’s (or perhaps Marshall’s glasses): production is carried on to the point that the value of the marginal product of a factor of production equals its marginal cost (Wible and Hoover 2015, p. 517). But this is quite in keeping with Peirce’s reading of the history of economics:

Ricardo carried the analysis of political economy to its highest pitch, and Augustin Cournot treated the subject mathematically (as Ricardo did substantially, too) in a book whose mathematical blunders do not really affect its principal conclusions. [Peirce quoted in Eisele 1979, p. 369; see also Wible and Hoover 2015]

Indeed, the passage in which Peirce explains Ricardo’s theory of rent moves on seamlessly to an analysis of the incidence of import duties in which he refers to consumer behavior in a manner than assumes implicitly the existence of demand curves – fundamental elements of Cournot’s analysis, but never mentioned by Ricardo – reaching the conclusions, first, that consumers will not pay the full of the amount of the duty and, second, that duties are most effectively levied on goods on which “our demand is so influential that a small decrease in the demand will cause a relatively large fall in the price” (i.e., on goods that are now referred to as *price-inelastic*) (CP 4.115; Cournot 1838[1927], pp. 46-47).

The theory of rent is simply an illustration for Peirce of something analytically and methodologically more fundamental. But exactly what? The question is hard to answer; for Peirce outlined so many projects in which the methodology of economics would have formed an important part, but which were never completed. We are left with headings and fragments and are forced to conjecture exactly where Peirce would have

ended up.⁸ There are two defensible options for further defining what Peirce means by “Ricardian inference.” On the first, Peirce takes the lesson of Ricardo’s theory of rent to be principally a mathematical one; on the second, he takes it to be a lesson in scientific logic. There is, perhaps, no way to make a certain choice between these options. Peirce himself may have wavered between them, and they may in the end not be mutually exclusive. Either option is consistent with seeing Ricardo’s theory of rent as an important illustration of inference in action.

3. The Primipostnumeral Syllogism

Peirce had a very broad conception of logic as the study of the principles that make our reasoning in all its many forms secure (CP 2.1). The never-completed “Qualitative Logic” (1886) was not devoted to reasoning in this broad sense but only to deductive inference, including mathematical deduction. In its table of content, only two proper names are mentioned: Fermat and Ricardo. “Ricardo’s Inference” (chapter IX) sits between “Fermatian Inference” (VIII) and “Infinity & Continuity (X) (WP 5.323). If Ricardo were one of Peirce’s scientific heroes, Fermat would have been a demigod. Pierre de Fermat (1601-1665), the proof of whose “last theorem” eluded mathematicians for 325 years, was to Peirce undoubtedly an important mathematician but, more than that, as a reasoner [he] cannot possibly be placed lower than second in the whole history of mind, for he invented a form of inference absolutely novel, and besides,

⁸ The fragmentary works in which economics is mentioned include the “Sketch,” a syllabus for sixty lectures on logic intended for, but apparently never delivered as a course at the Johns Hopkins University in 1883 (WP 4.476-489, esp. p. 489; for context, see WP4.1x), in which economics would have been discussed between lectures on the kinetic theory of gases and anthropomorphic science; “Qualitative Logic,” dated 1886 (WP 5.323-371), for which the section on economic inference was never completed; a prospectus for a twelve-volume work on the principles of philosophy, dated 1893 (CP 8, pp. 282-286, esp. p.285), in which mathematical economics would be covered in volume VIII, titled *Continuity in the Psychological and Moral Sciences*, and Walras and Marshall were to be deployed as part of a proof of free will (volume VII would address “evolutionary chemistry” and volume IX a mathematical approach to comparative biography).

discovered the mode of reasoning of the differential calculus, all but its notation . . .
[WP 8.269].⁹

Elsewhere, Peirce refers to Fermatian inference as the “greatest feat of pure intellect ever performed” (Peirce quoted in Eisele 1979, p. 164; cf. Eisele 1979, p. 101; see Eisele 1979, ch. 14 for a more detailed examination of Peirce’s treatment of Fermatian inference). And Ricardo stands shoulder-to-shoulder with Fermat in the plan for the “Qualitative Logic.”

Fermatian inference takes the following form: Suppose that a series of numbers is indexed by $n = 1, 2, 3, \dots$, that they are ordered by a generating relation, and that they are conjectured to display a certain property (CP 4.198). If it can be shown that a) the property holds for some particular element n_0 in the sequence, and b) if it holds for some arbitrary element $k > n_0$, then it holds for the element $k+1$ as well, then the property holds for all elements in the sequence greater or equal to n_0 . The inferential form, “as it is sometimes improperly termed, ‘mathematical induction’,” is a staple technique of number theory (CP 6.116).¹⁰ Without Fermatian inference, which Peirce casts in a syllogistic form, “no progress would ever have been made in the mathematical doctrine of whole numbers. . .” (CP 4.208-210). It “lurks beneath” the reasoning, even where it is not explicitly invoked (CP 4.208).

⁹ Fermat’s last theorem – really his conjecture, since it is believed that, despite a marginal note to the contrary, he did not have a correct proof – states that, for any positive integers a , b , and c , there is no integer $n > 2$ such that $a^n + b^n = c^n$. It was finally proved in 1995 by Andrew Wiles, using mathematical techniques unknown to Fermat.

¹⁰ Fermat himself referred to the method as “indefinite descent” (CP 4.110). A very simple example of the Fermatian inference: *Theorem*: For all $n \geq 1$, $3^{n+1} - 1$ is even. *Proof*: i) Let $n = n_0 = 1$, then $3^{n_0+1} - 1 = 3^{1+1} - 1 = 3^2 - 1 = 8$, which is even. ii) Suppose that the conjecture holds for some element $k > 1$, so that $3^k - 1$ is even. Consider element $k + 1$, $3^{k+1} - 1$, which can be written $3^{k+1} - 1 = 3(3^k - 1) + 2$. The term in parentheses is even by assumption, and any integer multiple of an even number is even, and adding any even number to an even number results in an even number. Thus, iii) if element k is even, then element $k + 1$ is even; and for any k whatsoever, a chain can be constructed from $n = 1$ to $n = k$ in which the successive elements of the series can be shown to be even. *QED*

Fermatian inference applies to infinite sets, but only to countably infinite sets, which Peirce and some modern mathematicians refer to as *denumerable* (CP 4.188). A set is *countable* when its elements can be placed in a one-to-one correspondence with the natural numbers (i.e., with the positive (or sometimes nonnegative) integers). In an early paper, the German mathematician Georg Cantor (1874) showed that some infinite sets cannot be placed into one-to-one correspondence with the natural numbers, so that some infinities are in fact larger (i.e., have a higher cardinality) than others. Peirce was converted to Cantor’s analysis of infinities at a time when it was still deeply controversial among mathematicians. Peirce referred to the smallest *abnumerable* (i.e., uncountable or nondenumerable) set as the *primipostnumerable multitude* (CP 4.200). Mathematicians, including Peirce, indicate the cardinality of infinite sets by a sequence designated by the Hebrew letter aleph (CP 4.203). The cardinality of the countably infinite is \aleph_0 , while that of the primipostnumeral set – that is, the first uncountable set is \aleph_1 .¹¹

Fermatian inference does not work for uncountably infinite sets. The problem is that the primipostnumeral set cannot be constructed with the Fermatian generating function. He illustrates the problem with the famous “paradox,” in which in a footrace Achilles can never catch a slower moving tortoise since every time he gets to where the tortoise was, it has moved on, and there remains a gap yet to close.

¹¹ The conjecture that the real numbers are \aleph_1 is known as the *continuum hypothesis*. Like Cantor, Peirce presumed that it was true. In fact, its truth remains an open question. Gödel (1940) showed that the continuum hypothesis could not be disproved within Zermelo-Frankel set theory supplemented by the axiom of choice. Cohen (1963) showed that it could not be proved in the same context. Thus, it is independent of Zermelo-Frankel set theory plus the axiom of choice. Both Gödel’s and Cohen’s proofs take the consistency of Zermelo-Frankel set theory as given, but though this is widely believed, it too has not been proven. (See Cohen’s 1966 for a further discussion.) The real numbers are, therefore, known to have a higher cardinality than the integers; yet they may not have the cardinality \aleph_1 .

We can reconstruct Peirce’s analysis (CP 4.202). Let Achilles start at position A_1 and the Tortoise some distance ahead at T_1 . Achilles starts behind the Tortoise and runs faster. Peirce characterizes the position of the Tortoise according to a generating relation in which any point T_{k+1} is the point that the Tortoise has reached when Achilles reaches the Tortoise’s previous location (i.e., when $A_k = T_{k-1}$. The Fermatian syllogism would then run as follows:

- | | |
|--|------------------------|
| i) $A_k = T_{k-1}$, for all integers $k > 1$; | (generating relation) |
| ii) if $A_k < T_k$, then $A_{k+1} < T_{k+1}$; | (conditional property) |
| iii) $A_1 < T_1$; | (particular property) |
| <hr/> | |
| \therefore iv) for all integers $n \geq 1$, $A_n < T_n$. | Q.E.D. |

The argument is correct: there is no point in the denumerably infinite set defined by the generating function (what Peirce refers to as points in their *primal* arrangement (CP 4.198)) at which Achilles is ahead of the Tortoise.

But does the Fermatian syllogism show that Achilles can never catch the Tortoise? Not at all, says Peirce. The point at which Achilles catches the Tortoise – a point that is easily computed based on the differences between their two starting points and between their speeds – simply is not one of the points in the denumerable set employed in the Fermatian inference. Were we to add the catch-up point to the infinite sequence, we would still have a denumerably infinite set; yet that set would no longer be in its primal arrangement – that is, the generating relation could not generate every point in the set. “The only reason,” Peirce writes, “that the inference does not hold is that the [points] are no longer in their primal arrangement” (CP 4.202). The essential problem is that the generating relation does not exhaust the points at which Achilles and the Tortoise

can be located which are nondenumerably infinite: “the denumerable collection in its primal order leads to no way of constructing or of conceiving of a primipostnumeral collection. . . . The primal arrangement of the denumerable collection affords no definite places nor approximations to the places for the primipostnumeral collection” (CP 4.202). In effect, Peirce argues that Fermatian inference restricts us to a subset of all possible numbers, and if that subset is chosen (i.e., if a generating relation is) such that it is defined to be those points at which the Tortoise is ahead of Achilles, then necessarily the point at which Achilles catches the Tortoise cannot lie in that subset. That says something about the nature of the subset; it says nothing about whether Achilles can actually catch the Tortoise.¹²

Peirce develops a close analogue to Fermatian inference, defined in terms of sets, which he calls “collections” and “packs,” that reinstates a clear successor relationship. This is his *primipostnumeral syllogism* (CP 4.209). He argues that the primipostnumeral syllogism is as central to the theory of real quantities as the Fermatian inference is to the theory of numbers; and, even when it is not explicitly invoked, it is a foundational element of the differential calculus. In Peirce’s hands, it is essential to the foundations of an approach to calculus grounded in infinitesimals as actual numbers. Infinitesimals already had a poor reputation in Peirce’s day, having been pushed out of calculus by analysis in terms of limits. Peirce has “nothing against [the doctrine of limits], except its timidity or inability to see the logic of the simpler way” (CP 4.152). “[T]he hypothesis of

¹² The cited texts are from 1897. Peirce continued to try to understand how anyone could regard Achilles and the Tortoise as a paradox rather than a straightforward fallacy, taking up the topic in three letters to William James in 1903 (Peirce 1976, pp. 801-808). As well as laying out why the typical argument fails even to articulate the claim that it is meant to support, Peirce notes that for the paradox mongers (“the philosophers” (p. 808)) “[t]here is a supposed contradiction of logic. *Whose* logic? If it were mine that was in so flagrant violation of fact I would change it for one that died [*sic*] not lead from a true premise to a false conclusion [*viz.*, that Achilles fails to catch up to the Tortoise]” (p. 803).

infinite and infinitesimal quantities is consistent and can be reasoned about mathematically . . ." (CP 4.118, fn. 1; see also CP 3, ch. XVIII; and Peirce 1976, pp. 121-127).¹³

Peirce does not claim to be original:

I do not mean to say that the primipostnumeral syllogism is altogether unknown in mathematics; for the reasoning of Ricardo in his theory of rent, reasoning which is of fundamental importance to political economy, as well as much of the elementary reasoning of the differential calculus, is of that nature. But these are only exceptions which prove the rule. . . . [CP 4.210]

Does Peirce identify Ricardian inference with the primipostnumeral syllogism?

His juxtaposition of Ricardian inference with Fermatian inference, the close cousin of the primipostnumeral syllogism, might point that way. And it is striking that of all the possible examples that might illustrate the syllogism, Peirce chose Ricardo's theory of rent. It is believable that Ricardo's vision of land taken into production sequentially in order of productivity could be captured in a generating relation suitably adapted to continuous quantities and the last land taken into production (i.e., the land that pays no rent) could be treated as an infinitesimal in Peirce's sense. Yet, in the passages in which he analyzes the primipostnumeral syllogism most carefully, Peirce never uses the term "Ricardian inference." Rather he offers the theory of rent merely as an example. And in his exposition of the theory of rent, he neither mentions the primipostnumeral syllogism nor explicates the theory in a way that would highlight its operation. Instead, he provides

¹³ Eisele (1979, p. 165) claims that "Peirce was one of the last stalwarts to defend the use of the infinitesimal in the structure of the calculus" (see also Eisele 1979, p. 101, and Heron 1997). Murphey (1993, pp. 119-120), originally published in 1961, notes that modern mathematicians reject infinitesimals in favor of limits. Nonetheless, just a few years later, Peirce's claims for the consistency and tractability of infinitesimals would, however, seem to be vindicated by what is now termed "nonstandard analysis." Robinson (1966) demonstrated the logical consistency of a mathematics that includes infinitesimals (cf. Eisele 1979, pp. 246-248; Parker 1998, pp. 93-99, esp. pp. 98-99). Ketner and Putnam (1992, pp. 37-54, esp. p. 48) provide a detailed discussion of Peirce's ideas of the continuum and infinitesimals, noting both the similarity to modern nonstandard analysis and some key differences.

an argument, which could easily have been taken from Cournot, drawing on ideas of the differential calculus that certainly predated Ricardo. To the degree that the primipostnumeral syllogism is foundational for the calculus, it is certainly involved in the theory of rent, but it is not clear that such involvement would warrant attaching Ricardo’s name to the inferential form. Nonetheless, it illustrates the important point about Peirce’s understanding of economics: it is essential to economics that it taps the deepest roots of science – its fundamental forms of inference.

4. Analogical Inference

A second possibility for what Peirce meant by “Ricardian inference” is drawn from his broader conception of the logic of science. Nature is complex. Rather than attack it head-on, a strategy that has little hope of success, Peirce suggest that science ought to – and, in fact, does – address problems indirectly. The strategy is

to substitute for those problems others much simpler, much more abstract, of which there is a good prospect of finding probable solutions. Then, the reasonably certain solutions of these last problems will throw a light more or less clear upon more concrete problems which are in certain respects more interesting.

This method of procedure is that Analytic Method to which modern physics owes all its triumphs. It has also been applied with great success in psychical sciences also. (Thus, the classical political economists, especially Ricardo, pursued this method.)¹⁴ [CP 1.63-64]

The context of Peirce’s methodological praise of Ricardo is a discussion of the history of science and, in particular, its methodological lessons, in which Peirce frames the analytical method in the context of the logic of science (CP 1, ch. 1; especially

¹⁴ Weighing into contemporary debates among political economists over *Historicism* versus *a priori* methods (and anticipating debates between Institutionalists and neoclassical economists), Peirce goes on: the Analytical Method “is reprobated by the whole Hegelian army, who think it ought to be replaced by the ‘Historic Method,’ which studies complex problems in all their complexity, but which cannot boast any distinguished successes” (CP 1.64; cf. Keynes 1917, pp. 314-327; Robbins 1935, pp. 79-83, 1998, Lectures 25 and 26; Schumpeter 1954, ch. 4, part 2).

sections 8-14). Peirce famously and repeatedly throughout his career offers a tripartite classification of the fundamental types of logical inference (e.g., CP 1, section 10). He divides inference into, on the one hand, *explicative*, *analytical*, or *deductive* inference and, on the other hand, *ampliative* or *synthetic* inference (what is "loosely speaking" called *inductive* reasoning) (CP 2.680; see also CP 2, book III, part B). Deductive inference itself brings no new facts into our reasoning, but works out the necessary consequences of facts already presumed to be true. Ampliative reasoning is divided into *induction* proper and *abduction*. ("Abduction" is Peirce's most common term for the latter form of inference, but at various times he preferred the terms *hypothesis*, *retroduction*, or, more rarely, *presumption*; see CP 1.65, 1.68, 2.5152.774, 2.791, and WP 4.419)

In an early exposition of the distinctions (1878), Pierce relates the types of inference to the syllogism, starting with the classic form of *Barbara* (CP 2.623):

DEDUCTION

Rule. – All beans from this bag are white.
Case. – These beans are from this bag.
∴Result. – These beans are white.

INDUCTION

Case. – These beans are from this bag.
Result. – These beans are white.
∴Rule. – All beans from this bag are white.

HYPOTHESIS [ABDUCTION]

Rule. – All beans from this bag are white.
Result. – These beans are white.
∴Case. – These beans are from this bag.

Only the first inference is necessary: if its conclusion is false, one or more of its premises must be false. The second inference is not necessary – we may simply have failed to

sample enough beans to find one of the black ones – but it offers us new knowledge, a *universal* conclusion about the contents of the bag. The third inference is also not necessary – the white beans could have come from some other bag – but it also offers us something new, an *explanation*. Much of Peirce’s work as a logician was devoted to exploring the conditions under which the two unnecessary and, therefore, uncertain inferential forms would be substantively useful.

Peirce sometimes associates induction exclusively with statistical procedures with the determination of the ratio of predesignated quantities (CP 1.67). For example, that the beans are either black or white predesignates the categories in which our results are to be classified, and the induction itself merely counts. The validity of the induction arises from a) *predesignation* – that is, the maintenance of the categories during the process of collecting and classifying the data; and b) *random sampling* – that is, from following a procedure designed to make any group of elements of the population and any ordering of those elements as likely to be sampled as any other of the same size.¹⁵ Without predesignation, there is a vicious circularity: we learn nothing new if we adjust the categories to fit the previously known data.

The essential advantage of random sampling is this: we may be mistaken with respect to the actual ratios or statistically determined values of a parameter, but if we persist we will either exhaust the population and, therefore, find the true value, or, in an open-ended population, converge on the true value according to known statistical laws (which are deductive truths) in a way that lets us set ever narrower bounds on the truth. It

¹⁵ These criteria, along with other aspects of ampliative reasoning are set out extensively in “A Theory of Probable Inference,” dated 1883 (WP. 408-450, esp. 410-413 and 427-433. See also CP 1.67, 1.93, 1.192-197, 2.775, 2.788-790, 2.193, 7.209-211; EP 2.101).

is a method, which if persisted in, is inevitably self-correcting (CP 2.769, 2.781, 5.170; WP 4.429). Still, inductive inference is not necessary. For any finite sample, we cannot rule out that we got a bad draw, and our best estimate is far from the truth. It is just that the probability of such a draw gets smaller as we proceed. What is more, inductive inference is conditional: the inevitable convergence to the true value occurs only if the framework of our predesignated categories is correct and the steps that we have taken to ensure random sampling are successful. The latter may be monitored statistically, but in both cases nothing rules out surprise: long after a stable ratio has been established between black and white beans, we may draw a yellow one, calling into question the predesignated categories and indicating that a new hypothesis is in order.

Peirce has no interest in ruling out surprise, so long as it is not so frequent as to strip induction of its usefulness, since surprise is the main source of new knowledge. Nature teaches us by a series of “practical jokes, mostly cruel” (CP 5.51). The possibility of a surprise of the yellow-bean variety also shows that aside from the establishing of a ratio, induction sometimes provides the opportunity simply to test an hypothesis that has a yes or no, true or false answer. In fact, in some accounts of ampliative inference, Peirce includes under the heading “induction” the testing of hypotheses (the consequences of abductions), even when the hypotheses do not admit of a statistical characterization (WP 4.439-441; CP 2.759, 5.145, 5.280).

The theory of abduction is often seen as one of Peirce’s most innovative contributions to logic.

Retroduction [*abduction*] is the provisional adoption of a hypothesis, because every possible consequence of it is capable of experimental verification, so that persevering application of the same method may be expected to reveal its disagreement with facts, if it does so disagree. [CP 1.68]

While induction can show us the need for something new, it is abduction that actually introduces it. Abductions take the form:

The surprising fact, C, is observed
But if A were true, C would be a matter of course,
Hence, there is reason to suspect that A is true. [CP 5.188]

Read in a bare way, abduction/induction (testing) seems to presage Karl Popper's conjectures and refutations or inference to the best explanation (Popper 1963, ch. 1; Lipton 2004; see also Töben 1986). Peirce calls abduction "inference," but, in fact, he does not object to calling it "conjecture" (CP 5.188). Still, his view is not Popper's; for Popper famously denies there is any scientifically interesting account of conjectures. Conjectures may spring from accident just as well as from as inspiration; from prejudice as from deep acquaintance; from mundane experience as from metaphysical poetry; from Marley's ghost as from "an undigested bit of beef, a blot of mustard, a crumb of cheese, a fragment of an underdone potato." Peirce accepts that abductive conjecture *could* come from anywhere, provided that they fit the appropriate inferential template. But simply guessing wildly in an infinite space of hypotheses would be a remarkably inefficient way to light on the truth. And he denies that ampliative inference fails, as Popper would later suggest, to have a useful logic.

To give one of Peirce's favorite examples, the game of twenty questions shows that a set of carefully structured hypotheses and tests can rapidly reduce the hypothesis space, so that only the truth remains: "twenty skillful hypotheses will ascertain what two hundred thousand stupid ones might fail to do" (CP 7.220). But twenty questions or any other abductive strategy could not, Peirce believes, reduce the uncountable infinity of alternative hypotheses if people did not have at least a slightly better probability of picking the true hypothesis than mere random guessing (WP 4.445-446; also CP 1.121,

2.753, and Murphey 1993, p. 324 ff.). The bias in favor of truth need not be high, but it must be there. Galileo and others who stood at the great junctures in the history of science relied on *il lume natural*, "an inward power, not sufficient to reach the truth by itself, yet supplying an essential factor to the influences carrying their minds to the truth" (CP 1.80; also CP 1.630). Peirce's only explanation ultimate origins of our inferential good luck is that it is evolutionarily adaptive: survival in the kind of world we live in requires it (WP 4.447-450).

Abduction and induction are cooperative forms of inference, with abduction setting the framework for inductions and inductions evaluating abductions. Peirce goes further and suggests that there is actually a fourth, hybrid form of inference, which he terms "analogy" that combines the character of abduction and induction (CP 1.65). It is analogy that forms the basis of the analytical method to which Peirce attributes so much scientific success in economics, as well as in the physical sciences. Peirce defines *analogy* as "the inference that a not very large collection of objects which agree in various respects may very likely agree in another respect" (CP 1.69).

The definition itself illuminates very little. What Peirce appears to have in mind is something closely akin to the way in which modern economists, as well as scientists in many other disciplines, employ models as inferential tools. Self-consciousness about modeling in any science is largely a post-World War II phenomenon. Earlier, the word "model" in the sciences almost always referred to physical representations such as an orrery or a patent model. Yet there is good reason to think that it is not anachronistic to see the concept of modeling in Peirce's understanding of analogy as a distinct form of inference. Peirce takes *analogy* to be an English translation of Aristotle's *παράδειγμα*,

which is the etymological source of *paradigm*. “Paradigm” is, in the wake of Kuhn’s *Structure of Scientific Revolutions* (1962), widely used, but it was an obscure, largely grammatical term in Peirce’s day, and Peirce would not have invested it with Kuhnian resonances. According to the *Oxford English Dictionary*, the root word” *δειγμα* (*deigma*) designates a “sample [or] pattern,” while the prefix *παρά-* (*para-*) conveys the idea of “analogous or parallel to, but separate from or going beyond” that pattern. The word itself aptly conveys the strategy of analogical reasoning that Peirce attributed both to physics and to classical political economy and maps very nicely onto modern practices in which stripped down or idealized root models are elaborated successively to come closer to empirical observations while maintaining their underlying basic character and tractability.¹⁶

Peirce clarifies analogy as a mixed type of inference with an extended analysis of Kepler’s discovery of his laws of planetary motion: “the greatest piece of Retroductive reasoning ever performed” (CP 1.72-74, especially CP 1.74). Kepler began with Copernicus’s hypothesis of the planets in circular orbits around the sun and Tycho Brahe’s and his own observations. The analogy was, if we can use the terminology anachronistically, between Kepler’s mathematical model, with its precise orbits, and the actual observations. The analogy was not, at first a good one: the Copernican model fit the data rather badly. Out of keeping with Popper’s later methodological pronouncements, Kepler did not simply scrap Copernicus’s model. His procedure was not haphazard, but systematic and conservative, in the sense that at each new abductive step, he tried to preserve his quantitative success hitherto – that is, to stay within the

¹⁶ On the history of models in economics, see Morgan (2012).

bounds of error already achieved – and to use the specific ways in which the hypothesis fell short to suggest the next abductive step. Kepler's own abductive contribution was to consider the dynamical implications of the sun, which he knew to be vastly larger than any of the planets and which he conjectured exercised some vaguely-defined causal power over them. Alternating abductions to introduce modifications and inductions to characterize the nature and degree of the deviations between conjecture and data, Kepler refined the model:

never modifying his theory capriciously, but always with a sound and rational motive for just the modification – of most striking simplicity and rationality – which exactly satisfies the observations, it stands upon a totally different logical footing from what it would if it had been struck out at random, or the reader knows not how, and had been found to satisfy the observation. [CP 1.74]

The analytical method for Peirce is largely the method of refining and precisifying analogies. The Copernican hypothesis of the planets moving in circular orbits about the sun was a piece of cœnoscopic astronomy, relying not on detailed observation but on what economists today refer to as "stylized facts." Kepler modified the analogy, but also particularized and added precision to the facts, so that Kepler's laws are a triumph of idioscopic astronomy.¹⁷

¹⁷ Perhaps because he counted astronomy among the many sciences to which he made important contributions, Peirce frequently illustrated methodological points with reference to Kepler (CP 1.70-79, 2.96-97, 2.707, 7.180, 7.419)). Peirce's reading of Kepler's accomplishments comports well with modern scholarship. Mill, in Peirce's view, misread Kepler, denying "that there was any reasoning in Kepler's procedure." And characterizing it instead as "merely a description of the facts" (CP 1.71). In a detailed examination of Kepler's work, Voelkel (2001) reaches the same conclusions as Peirce for essentially the same reasons. Voelkel explains that astronomy in Kepler's day referred to the mathematics of celestial observations – that is, to the mere description of the facts – and it was thought to have achieved its ends when it could offer an accurate systematic description of the movements of the heavenly bodies (p. 24). In contrast, cosmography concerned itself with the nature of those bodies and was a branch of *physica* or even metaphysics, as the term was used at the time (p. 26 ff.). Thus, astronomers had no problem with entertaining Copernicus's heliocentric model, so long as it was regarded merely as a way of generating the patterns of observation; while cosmographer, who were concerned about the real composition of the heavens, frequently rejected it as contrary to scripture. Voelkel, like Peirce, reads Kepler as radically rejecting the division between astronomy and cosmography or *physica* in insisting on a causal explanation not only to achieve the ends of cosmography but to perfect astronomy (*inter alia* pp. 28, 37, 127). Voelkel

The root hypotheses of economics, such as the law of supply and demand, are similarly cœnoscopic: “In all economics the laws are ideal formulae from which there are large deviations, even statistically . . . general tendencies to which exceptions are frequent” (CP 7.158). Economic laws, for Peirce, are more true on average than they are for any individual. Economics is thus the natural landscape for statistical refinement, for inductions that raise the precision of the models (Peirce quoted in Eisele 1979, p. 251).

The essential point for Peirce is that analogical reasoning represents not – as Popper would have it – testing leading to relentlessly directing the arrow of *modus tollens* against a horde of wild hypotheses; rather analogical reasoning represents a *constructive* interplay of abduction and induction. Any induction presupposes a prior abduction. A surprise or an inconsistency between the induction and the prior abduction provides the impetus for a new abduction. Such an abduction could be wholly novel or it could be, which is vastly more likely to be fruitful, a carefully selected variation on the original abduction.

At this point, Peirce’s views on the boundaries between abduction and induction become fairly subtle; for he does not count every emendation of a hypotheses as a new abduction. For example, he counts it as an epistemic virtue of a hypothesis “that it may give a good ‘leave,’ as the billiard-players say” (CP 7.221). The idea is that the hypothesis contains some wiggle-room. We may think, for instance, that the data should be described by a quadratic relationship and, yet, we may find it more instructive to fit a linear relationship because the residuals are more readily interpreted. The pattern of their deviations provides some information about how we might wish to adjust our hypothesis.

also notes the manner in which Kepler modeled the astronomical data iteratively in the manner that Peirce emphasizes (p. 205).

In starting with a simple hypothesis and successively modifying it within a class of possible hypotheses that are obvious from the start, we are, Peirce believes, engaged in induction, not abduction: "induction adds nothing. At the very most it corrects the value of a ratio or slightly modifies a hypothesis in a way which had already been contemplated as possible" (CP 7.217). Peirce's idea seems to be that even a fairly specific hypothesis, such as that the data are quadratic, can be treated not as a precise claim but in a manner close to original meaning of "paradigm," as an instance or exemplar that can serve as an index for wider family of precise hypotheses. Thus, the quadratic hypothesis could be taken to be an index of the whole polynomial family; and, just as enumerative induction of a statistical kind results in a narrowing of the bounds on the value that a true ratio could take, inductions of a nonstatistical kind can be taken as a narrowing of the subset of family members compatible with the truth. Idioscopic sciences, physics without a doubt, but possibly economics as well, are rapidly approaching a condition in which "we should no longer look for unexpected additions to our knowledge so much as to narrowing the limits between which it seems likely each truth will ultimately be found to lie" (Peirce quoted in Eisele 1979, p. 371). The true abduction – the inference that might provide "unexpected additions to our knowledge" – is the replacement of one family of hypotheses by another.

The promise of both statistical and nonstatistical induction for Peirce is their property that, if we keep at it, they are bound in the fullness of time to reveal the falsity of our hypothesis, if it is indeed false – even if the falsity appears only beyond some particular limit of precision. There is no certainty of their success even in a negative sense: the falsifying observation may always lie just round the corner. And there is

absolutely no certainty in the positive sense: an observation may tell us that we are entertaining entirely the wrong family of hypotheses; it requires luck or *il lume natural* or a natural affinity for the truth, an affinity which can be grounded only in hope and not in knowledge, to pick the right family (CP 1.121, 7.219).

Peirce holds up Ricardo's theory of rent as a prime example of analytical reasoning, not only for economics but for all science. The manner in which he moves seamlessly in his exposition of the theory from the specific details of rent to the question of tax incidence, which involves demand analysis – quite new since Ricardo – suggests that he regards the theory as a starting point, an initial template, an index of a family of hypotheses whose members differ in their scope and complexity, which is ripe for inductive precisification and which, in the manner of Kepler's introduction of the causal powers of the sun, can be joined to novel abductions to build an idioscopic economic model of real-world phenomena (CP 4.115).¹⁸

5. Ricardian Inference and Economic Science

¹⁸ Peirce's treatment of Ricardo is deeply consistent with that of Morgan's (2012, ch. 2), not only as a generic "modeler" but as one who used the generic model as a template for quantitative refinement. Both Peirce and Morgan stand in stark contrast to the interpretive tradition exemplified by Schumpeter's (1954, p. 473) coinage of the term "Ricardian vice." Where Peirce and Morgan see Ricardo as fundamentally an *analytical* economist – that is, as an economic theorist who provides a basis quantification and inductive precisification, Schumpeter sees him as "not a mind that is primarily interested in either fundamentals or wide generalizations. . . His interest was in the clear-cut result of direct, practical significance" (p. 472). His method was to pile "one simplifying assumption upon another until, having really settled everything by these assumptions, he was left with only a few aggregative variables which, given these assumptions, he set up simple one-way relations so that, in the end, the desired results emerged almost as tautologies" (pp. 472-473). The *Ricardian vice* refers to drawing solutions to practical problems from such reasoning as, and Schumpeter likens it to "implicit theorizing" – a practice that Leontief attributed to, and criticized in the work of, John Maynard Keynes, whom he sees a similarly practically-oriented economist. Schumpeter's complaint would have been seen as barely coherent to Peirce. Far from hiding one's hypotheses, as the charge of implicit theorizing suggests, Ricardo's radical simplification is the essence of analytical inference, and essential to bring mathematics *and* empirical evidence to bear on a scientific question in a systematic way. It is odd to simultaneously task Ricardo to have been dominated by immediate practical concerns and to have adopted a method that is criticized for being inadequate to those very practical concerns.

Peirce found that economic examples were particularly effective when trying to explain various aspects of science and its logic, which lends salience to the question, where does economics itself fit among the sciences? The answer to that question is easier to determine for Peirce than it might be for other philosophers, since he was an inveterate classifier (as well as a prolific and inventive coiner of terminology). Peirce attempted comprehensive classifications of the sciences more than once, but all the attempts bear a close family resemblance. He takes Jeremy Bentham and Auguste Comte as his models and starting point.

Comte classifies the sciences in a hierarchy from simple/general to complex/particular. Peirce cites Comte's metaphor that likens the sciences to "a sort of ladder descending into the well of truth, each one leading on to another, those which are more concrete and special drawing their principles from those which are more abstract and general" (CP 2.119; also CP 1.180 and cf. Comte 1830[1905], p. 52). Again, like Comte, Peirce divides sciences into theoretical and practical, but subdivides the theoretical to give a tripartite classification: sciences of discovery, sciences of review, and practical sciences (CP 1.181, 239).

Peirce divides the sciences of discovery into three: *mathematics* and, drawing on Bentham's terminology, *cænoscopy* (in some places also called *philosophy*), and *idioscopy* (CP 1.183, 239-242; Bentham 1816, pp. 177-179; Bentham 1952, p. 85). All these sciences are observational, though in different senses.

Mathematics studies what is and what is not logically possible, without making itself responsible for its actual existence. Philosophy [cænoscopy] is a *positive science*, in the sense of discovering what is really true; but it limits itself to so much truth as can be inferred from common experience. Idioscopy embraces all the special sciences, which are principally occupied with the accumulation of new facts. [CP 1.184]

Mathematics stands on the top rung of Comte’s ladder – or, at least, on Peirce’s version of it; for Peirce quips that “Comte was too small a mathematician to appreciate Cauchy, Fresnel, Gauss, Laplace, or even Ricardo” (WP 8.268-269).¹⁹ It posits hypotheses and traces out their consequences without reference to facts. It is observational just in the genuine but “very peculiar sense” that it constructs imaginary objects “according to abstract precepts, and then observes these imaginary objects, finding in them relations of parts not specified in the precept of their construction” (CP 1.240). Mathematics serves all sciences (CP 1.133, 1.245). Mathematics is even more basic than logic, which itself is a servant of all other sciences. Mathematics, in Peirce’s view, “is purely hypothetical: it produces nothing but conditional propositions,” whereas logic “is categorical” and “a normative science” (CP 4.240). Logic is a servant of truth in the world; mathematics is a servant of its own precepts. There is a mathematical logic, but mathematics itself has no more need of logic *per se* than the ability to talk has need of a theory of the formation of vowel sounds (CP 4.241-242).

Given his conception of mathematics as purely hypothetical, it is easy to see why Peirce can regard the classical political economist Ricardo as a mathematician – even if a not a deeply trained one – and it is easy to see the way in which he will frame the notion of mathematical economics. For mathematical economics amounts to adopting *conditionally* hypotheses and behavioral precepts that are suitably “economic” and traces out their consequences whether or not they prove to be useful explanations of actual economic facts. Ricardo was, in Peirce’s view, the first – and indeed the finest exemplar

¹⁹ On the central role of mathematics in all areas of Peirce’s thought, see Murphey (1993).

– of the mathematical approach in economics, quite independent of his lack of mastery of mathematical technique (Peirce 1976, p. xxv; also Eisele 1979, p. 369).

Somewhere below the rungs of pure mathematics on Comte’s ladder, Peirce places cœnoscopy (or philosophy), which “contents itself with observations such as come within the range of every man’s normal experience and for the most part in every waking hour of his life” (CP 1.241). Philosophy on this view remains an observational science; yet,

[t]hese observations escape the untrained eye precisely because they permeate our whole lives, just as a man who never takes off his blue spectacles soon ceases to see the blue tinge. . . [N]o microscope or sensitive film would be of the least use in this class. The observation is observation in a peculiar, yet perfectly legitimate sense. [CP 1.241]

The cœnoscopic sciences include for Peirce the physical statics of Lagrange and physical dynamics, excluding the law of the conservation of energy (CP 8.198). Peirce traces the development of an analogous cœnoscopic foundation for the psychical sciences from John Locke to the rest of Europe – especially to Scotland:

The analytical economics of Adam Smith and of Ricardo were examples of it. The whole doctrine in its totality is properly termed the Philosophy of Common Sense, of which analytical mechanics and analytical economics are branches. [CP 8.199]

Peirce rejects the idea that the experiences that warrant even analytical mechanics are true to “microscopical exactitude”; yet they are mainly true and inform further scientific refinements. And what was true of analytical mechanics was even more true of cœnoscopic economics. The classical political economy of Ricardo and others, even though it was unfamiliar with probability and statistics, “relates to a very large collection of individuals whose average character must be much more fixed than those of the single individual” (Peirce quoted in Eisele 1979, p. 251).

To bring precision and refinement to any science was, for Peirce, to move to lower rungs on Comte’s ladder – to idioscopy. Bentham’s idioscopic sciences depend on special observations : for example, field work, special instrumentation, or special training and research methods (CP 1.242). Physics (as opposed to physical statics and dynamics), chemistry, and astronomy are examples of idioscopic physical sciences, whereas psychology, linguistics, sociology, history and, presumably, substantial parts of economics are idioscopic psychical sciences.²⁰ The idioscopic sciences must take account of the relevant conclusions of the cœnoscopic sciences (CP 1.246).

We have explored the evidence for two different interpretations of what Peirce understood Ricardian inference to be. And we have been left with a puzzle: on one interpretation Ricardian inference is related to a sophisticated area of mathematics; while on the other interpretation, it is related to the methodology of empirical investigation; and we have no compelling reason for choosing one interpretation over the other. But perhaps we do not have to choose. Yes, these interpretations are different; yet, given, Peirce’s systematic and hierarchical view of the sciences, they may be more closely related than appears at first blush. Mathematics, for Peirce, is the most fundamental science. And, as we have seen, it bears a dual aspect. On the one hand, it is a self-contained, *a priori* science, in that it investigates the consequences of arbitrary hypotheses, according to rules that are themselves mathematical precepts. Yet, on the

²⁰ In a plan for a scientific dictionary in 1892, Peirce lists *mathematical economics* between *mathematical psychics* and *probabilities* under the general heading *Mathematics*, while he lists *domestic economy* between *our laws and customs* and *education* under the general heading *Sociology* (WP 8.293-295). It is impossible to know for certain what Peirce means by “domestic economy” in this context: is it an element of political economy or what we would today call “home economics”? Indeed, the entry for *economics* in the *Century Dictionary* (p. 1836) gives, as its first definition “The science of household or domestic management. [Obsolete or archaic].” *Oddly*, in an earlier structured list, “The Sciences in Their Order of Generality” written for the *Century Dictionary*, Peirce omits economics altogether (WP 8.275-276).

other hand, it remains, in a special sense, observational and empirical, because mathematical reasoning fundamentally involves the observation of imaginary objects or, what he elsewhere refers to as *diagrams*, where the term is broad enough to include virtually all kinds of mathematical representation (e.g. CP 1.54; also Ketner and Putnam 1992, pp. 2-3.). What Ricardo (and, subsequently, Cournot) showed is that advances in economics used Ricardian inference on the first interpretation and, thus, required an appeal to continuity and higher orders of infinity (Peirce's primipostnumeral multitude). At the same time, to the degree that mathematics truly is observational, not only investigation of actual economies, but also investigations in mathematics itself, including investigation of the transfinite cardinals, involves the sort of analogical reasoning that constitutes Ricardian inference on the second interpretation. We recognize that this speculation cannot be definitely confirmed on the evidential basis that Peirce has bequeathed us. Yet, it does have the interpretive virtue of making the available evidence pull in a consistent direction.

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