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Retrospectives
Edgeworth's Hedonimeter and the Quest to Measure Utility

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This feature addresses the history of economic terms and ideas. The hope is to deepen the workaday dialogue of economists, while perhaps also casting new light on ongoing questions. If you have suggestions for future topics or authors, please write to Joseph Persky, c/o *Journal of Economic Perspectives,* Department of Economics (M/C 144), University of Illinois at Chicago, 601 South Morgan Street, Room 2103, Chicago, Illinois 60607-7121.

Introduction

Economic thought progresses in fits and starts as promising avenues of research are explored, and then backed away from as researchers change their focus to other, seemingly more promising avenues of research. Then, as still other developments raise questions about these new avenues or as technological developments occur, the older avenues of research become more attractive and are reexplored with new understanding and new knowledge. That pattern of ebb and flow seems to be happening with economists' study of the foundations of utility theory and measurement, which increasingly is attracting the attention of modern researchers (Easterlin, 2002; Kahneman, Diener, and Schwartz, 1999; Layard, 2005; Camerer, Lowenstein, and Prelec, 2005; Kahneman and Krueger, 2006; Reder, 2006; Bruni and Porta, 2005).

In this article, I discuss some earlier debates about the foundations of utility and its measurement, focusing on the contributions of Francis Y. Edgeworth (1845–1926), a famous British economist who was a leader in the development of

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a more mathematically structured economics in the late 1800s, and Irving Fisher (1867–1947), one of the first quantitative U.S. economists, best-known today for his work on the quantity theory and interest rate theory. Edgeworth argued that utility was directly measurable and that new developments in "physio-psychology" would make it possible to develop a "hedonimeter" that would allow economists to develop a firm physiological underpinning of utility. Fisher, while agreeing with Edgeworth that it was important to have a workable measure of utility, disagreed with Edgeworth about the possibility of doing so with a hedonimeter and, hence, of having any physiological underpinnings of utility. He argued that instead of searching for physiological underpinnings of utility, economists should instead rely upon backward induction from observed behavior to measured utility. Neither of these views about the possibility of utility measurement carried through, and attempts to measure utility were abandoned in the 1930s, when utility measurement and happiness considerations were determined to be outside the purview of economics.

There are, of course, many aspects of the debate about the dimensionality and measurability of utility. In this paper, I focus on the historical debate over whether economists should be searching for a direct measurement of utility or an indirect measurement of utility. I do not enter the historical debates about ordinal and cardinal dimensions of measurement, the additive aspects of utility functions, or the issues surrounding indifference curve analysis. Discussions of these topics can be found in history-of-thought books, such as Blaug (1996), and in Stigler's (1950) classic discussion of the history of utility theory.

Edgeworth's Hedonimeter

In the 1870s, economics was moving away from classical theories of value based on labor or cost, and moving toward neoclassical theories, which were associated with a mathematization and formalization of economics, a process that gave greater focus to utilitarian foundations of value. In this context, the measurability of utility became a central topic.

Edgeworth entered the discussion when in *Mathematical Psychics* (1881 [1961]), his most famous book, he responded to some pessimistic views about the measurability of utility that had recently been expressed by William Stanley Jevons (1835–1882).[1] Edgeworth posited the development of a hedonimeter that would measure utility, which he felt was necessary for the extension of utility analysis to the real world. To posit this hedonimeter, he first had to develop precisely what was meant by a unit of pleasure, which he does in Appendix III of *Mathematical Psychics.* Edgeworth admits (p. 98) that there is "much difficulty here" and that "hedonism

[1] It is often very difficult in the history of thought to specify an author's views unambiguously, both because those views change and because their views can be interpreted differently in different contexts. As Stigler (1950) points out, stating precisely what Jevons believed about measuring utility is very difficult, since his writings present a number of contradictory views.

may still be in the state of heat or electricity before they became exact sciences, as described by Professor Jevons"; but Edgeworth finds that it is nonetheless useful to "endeavour to gain as clear a view as may be," and he discusses the quantitative estimates that a person can make of his own pleasure or the pleasure of others. Edgeworth (p. 101; italics in the original) describes in practical terms how a hedonimeter would work. He writes:

> To precise [*sic*] the ideas, let there be granted to the science of pleasure what is granted to the science of energy; to imagine an ideally perfect instrument, a psychophysical machine, continually registering the height of pleasure experienced by an individual, exactly according to the verdict of consciousness, or rather diverging therefrom according to a *law of errors.* From moment to moment the hedonimeter varies; the delicate index now flickering with the flutter of the passions, now steadied by intellectual activity, low sunk whole hours in the neighbourhood of zero, or momentarily springing up towards infinity. The continually indicated height is registered by photographic or other frictionless apparatus upon a uniformly moving vertical plane. Then the quantity of happiness between two epochs is represented by the area contained between the zero-line, perpendiculars thereto at the points corresponding to the epochs, and the curve traced by the index; or, if the correction suggested in the last paragraph be admitted, another dimension will be required for the representation. The integration must be extended from the present to the infinitely future time to constitute the end of pure egoism.

Edgeworth (1881 [1961], p. 102) then extends his reasoning to all individuals: "We have only to add another dimension expressing the number of sentients, and to integrate through all time and over all sentience, to constitute the end of pure utilitarianism." Edgeworth recognized the practical problems of aggregating across individual utilities, but he argued (p. 102) that the "greater uncertainty of hedonimetry in the case of others' pleasures may be compensated by the greater number of measurements, a wider average; just as, according to the theory of probabilities, greater accuracy may be attained by more numerous observations with a less perfect instrument." He further argued that this machine would, in principle, offer reconciliation between egoism and altruism, since the pleasure that people take in the happiness of others would be measurable, or as Edgeworth (p. 104) put it, the hedonimeter would integrate into utility "the psychical side of a physical change in what may be dimly discerned as a sort of hedonico-magnetic field."

Edgeworth's discussion of hedonic measurement was based on developments in psychology, where "psychophysics" was seen as cutting-edge work at the time. Psychophysicists were interested in developing a sensory theory that related external stimuli to individual feelings. Psychophysics was built on work by Ernst Weber (1795–1878), a professor of physiology at the University of Leipzig who conducted experiments that explored the relationships between physical sensory sensations

and external stimuli. For example, Weber blindfolded people who were holding weights, and added additional weights in small increments. He had the individuals indicate when they sensed the increased weight. In doing so, he found that people's sense of additional weight was proportional to the relative, not the absolute, increase in weight, so that when the weight the blindfolded person was holding was doubled, the threshold for sensing the increased weight also doubled. He found that that this general relationship held not only for perceptions of weight, but also for perceptions of visions and sound.

Weber's work (1834 [1978]) was publicized and extended by G. T. Fechner (1801–1887) in *Elements of Psychophysics* (1860 [1966]). Fechner suggested a theoretical explanation for Weber's observations and put the relationships in functional form, which he called Weber's Law, but which later psychologists called the Weber–Fechner Law. In mathematical form that law is

$$p = k \cdot \ln(S/S_0),$$

where p is the individual's perception of a change, S is the added stimulus, S_0 is the beginning stimulus, and k is the relationship parameter.[2] Thus, for example, if one were initially holding a large weight, one would need a larger weight added for one to perceive the difference than one would if one were initially holding a smaller weight.

Fechner (1860 [1966], p. 7) waxed eloquent about psychophysics, arguing that it can be seen as "an exact theory of the functionally dependent relations of body and soul." In attempting to develop a theoretical foundation of the relationship, Fechner referred back to Rene Descartes, who saw the pineal gland as the place where body and soul met. (Recent work in science has found a link between the pineal gland and individuals' perception of wellbeing, in the sense that the pineal gland is by far the richest site of the neurotransmitter serotonin in the brain.) Fechner believed that he had discovered more than simply an empirical relationship; he believed that he had discovered a fundamental insight into our understanding of the mind.

Not all psychologists of the day accepted Fechner's interpretation of the law as a description of utility or happiness; some suspected a more primitive mental connection. For example, the pioneering Harvard psychologist William James (1890, ch. 13) saw Fechner's work as more a generalization about frictions in neural machinery. He wrote: "If our feelings resulted from a condition of the nerve-molecules which it grew ever more difficult for the stimulus to increase, our feelings would naturally grow at a slower rate than the stimulus itself. An ever larger part of the latter's work would go to overcoming the resistances, and an ever smaller part to the realization of the feeling-bringing state."

[2] In psychology, the Weber–Fechner Law has been superseded by the Stevens' Power Law (1957), which specifies the relationship between sensation as $S = kI^a$ where S is the intensity of sensation, I is the magnitude of physical stimulus, and the exponent a is empirically estimated and differs among senses.

However, Fechner's work was influential with a number of economists at the time, and his interpretation of the theory dovetails with Edgeworth's description of the hypothesized hedonimeter. Some economists even believed that Fechner's work provided a scientific psychological foundation for the then-developing law of diminishing marginal utility, which implied that changes in income at high levels of income would be less perceptible than changes in income at low levels of income. The foundation for the law was important politically because within a utilitarian framework, which was accepted by many economists of the time, the goal of public policy was to maximize some measure of aggregate utility. Within that framework, diminishing marginal utility seemed to imply scientific support for a progressive income tax.

Fisher's Alternative Approach to Measuring Utility

The beliefs that there could be a sensory measurement of utility, and that such a sensory measurement was necessary for economics to be a precise science, were not universally accepted by economists of the time. In Irving Fisher's Ph.D. dissertation (written in 1891; published in 1892 and 1925), Fisher attempted to provide a mathematical foundation for economics. In it, he took a quite different approach to measuring utility than Edgeworth had. Fisher attempted to provide a rigorous, quantitative, definition of utility; a proof that utility could be measured; and a method of measuring utility that did not rely on physiological measurements. Fisher argued that economics should not rely on direct measurement of utility, but instead rely on an indirect approach that worked backwards from individual choices to measures of utility. He was not interested in building measurements of utility on psychological foundations, but rather in making judgments about utility based on people's behavior.

In the preface to his dissertation, Fisher discusses the difference between his approach and Edgeworth's. He writes that he first saw Edgeworth's book three days after Part II of the dissertation was finished. As Fisher sees it, his approach is similar to Edgeworth's in that they both recognize that utility is a function of all commodities, not separable by commodities as had been assumed by Jevons. This insight had significant implications for relating marginal utility to an aggregate measure of utility, and to consumer surplus. However, Fisher also notes the major difference between their approaches—Fisher does not base his measurement of utility on Fechner's work, while Edgeworth does.

Fisher argued that economics does not need a psychophysical foundation for utility—individuals reveal their utility through their actions. Fisher (1892 [1925], p. vi) specifically ruled out basing utility on physiological measurements: "Edgeworth, following the Physiological Psychologist Fechner, answers: 'Just perceivable increments of pleasure are equatable' (p. 99) [the page reference is to Edgeworth's *Mathematical Psychics*]. I have always felt that utility must be capable of a definition which shall connect it with its positive or objective commodity relations. A physicist

would certainly err who defined the unit of force as the minimum sensible of muscular sensation." Fisher (p. vi, vii; italics in the original) continues the argument: "This foisting of Psychology on Economics seems to me *in*appropriate and vicious.[3] Fisher (p. 11, italics in the original) then continues the argument, writing, "To fix the idea of utility the economist should go no farther than is serviceable in explaining *economic* facts. It is not his province to build a theory of psychology." Fisher argues that economists should content themselves with what he calls a "simple psychoeconomic postulate: *Each individual acts as he desires.*"[4]

While compared to Edgeworth, Fisher was much more closely tied to what became the mainstream view of utility, his approach significantly differed from what became standard theory. The view that became prevalent after the 1940s was that interpersonal utility comparisons were unachievable and unnecessary for economics; in contrast, Fisher held the position that for utility theory to be useful, it had to be measurable. Thus, Fisher's difference with Edgeworth did not concern the measurability and comparability of utility; it concerned whether that measurement should be direct or indirect.

Another way in which Fisher and other mathematically oriented economists attempted to separate mathematical economics from these sensual foundations can be seen in the debate about what to call utility. Most mathematical economists wanted a separate name given to utility, to separate their analysis from the conception of utility having a physiological or welfare basis. They argued that the term "utility" was associated with pleasure and welfare, which implies a sensual foundation, whereas all an economist could actually observe were desires. Fisher (1892/1925, p. 11, italics in the original) wrote: "The plane of contact between psychology and economics is *desire.*" Among the alternate names, Vilfredo Pareto (1909 [1971]) favored the term "ophlelimity"; Fisher (1918) favored "wantab"; and Arthur C. Pigou (1920 [2002]) used the term "desirability."

[3] While Fisher objected to physiological psychology serving as a foundation for economic theory, he did not rule out a connection between economics and psychology. In fact, even as Fisher justified his statistical approach to measuring utility, he lauded the behavioralist approach in psychology. Fisher (1892, p. 180) writes, "So economists cannot afford to be too academic and shirk the great practical problems pressing upon them merely because these happen to touch on unsolved, perhaps insoluble, philosophical problems. The psychologist has set the example by becoming a 'behaviorist.' He can thereby deal practically with phenomena the essential nature of which he confesses he cannot fathom."

[4] One reason why Fisher took this stand against a physiological approach was to insulate the mathematical approach, which both he and Edgeworth were advocating, from the challenge that the mathematical approach was simply a metaphysical psychophysical approach—an attack to which he felt some advocates of the new approach were vulnerable. (By "metaphysical" they meant that it implied discussions of immeasurable, and hence unscientific, concepts.) Fisher (1892 [1925], p. vii) notes that others besides Edgeworth were making a connection between utility and sensory foundations, specifically stating that "Gossen and Jevons appeared to regard the 'calculus of Pleasure and Pain' as part of the profundity of their theory." He continues: "They doubtless saw no escape from its use. The result has been that 'mathematics' has been blamed for 'restoring the metaphysical entities previously discarded.'" The last quotation to which Fisher refers is referenced as coming from Dr. Ingram, which most likely refers to John Ingram, the author of a history of political economy book (Ingram, 1888[1907]), which according to Richard Howey (1985) was the only history of political economy book of the time that referred to marginal utility.

Fisher's arguments against a physiological foundation for utility theory were accepted by most of the profession. In the early 1900s, a number of economists distanced themselves from any physiological connection between economics and psychology. For example, Friedrich von Wieser (1924 [1927]), an important economist working in the Austrian tradition, wrote in his second edition of *Social Economics* that even though the method he uses has been recently designated as the "psychological" method, it does not have any connection to the physiological foundations of utility. He stated (p. 3) "that designation is 'not a fortunate one'" and that it does not start from "scientific psychology." Von Wieser specifically separates himself and Hermann Gossen, a German economist who was involved in the neoclassical revolution, from any association with Weber's Law. He further states that his and Gossen's approach has nothing to do with physiology, writing (p. 2), "Gossen's law of satiable wants, the foundation of the modern theory of value, has nothing to do with Weber's law. Economic theory would be benefited, had scientific psychology advanced further beyond its beginnings; but our discipline does not seek and could not find direct aid from this source. The tasks of the two branches of knowledge are entirely distinct."

Although Fisher did not base his theory on psychophysical laws, he believed that he could statistically measure utility and "prove" the law of diminishing marginal utility. In a later essay, Fisher (1927) further developed his reasoning and proposed a statistical method for measuring utility.[5] Fisher quotes a question from Jevons on measurability: "'But where,' the reader will perhaps ask, 'are your numerical data for estimating pleasures and pains in Political Economy?' I answer, that my numerical data are more abundant and precise than those possessed by any other science, but that we have not yet known how to employ them. . . . But it is chiefly a want of method and completeness in this vast mass of information which prevents our employing it in the scientific investigations of the natural laws of Economics" (Fisher, pp. 158–59). He further argues that "such records . . . ought, on proper analysis, to yield a real statistical measurement of this most elusive of magnitudes." Having made that bold statement, Fisher then backtracks and argues that even if it might not be possible to measure utility for an individual, it is for the aggregate. He writes: "We can often gauge a mass effect when we cannot gauge its constituent parts," and in justifying his argument, he quotes the famous physicist,

[5] This essay was actually written and circulated in mimeograph form earlier, but was published only in 1927. In his discussion of Fisher, Stigler (1950, p. 379) distinguishes Fisher's (1892) early views on utility, which Stigler presents as emphasizing the difficulties of constructing a utility function, from Fisher's later writings (1927) in which Fisher discusses his method for measuring marginal utility. In contrast, I see much more connection between Fisher's early and later writings. For example, in the preface to the reprint of his dissertation, Fisher (1925, p. iv) states: "The suggestion in this booklet that so-called 'marginal utility' may be measured is now being followed up." The reason Fisher did not develop the statistical measurement work in his dissertation was not that he did not believe that it was important, but rather that the dissertation focused on theoretical and mathematical issues only, which is why Fisher (1925, p. v, italics in the original) describes his dissertation as "*a study by mathematical methods of the determination of value and prices.*"

Willard Gibbs, who was one of his dissertation advisors: "The whole is simpler than its parts" (p. 159).

Fisher's views on the measurability of utility may seem strange to modern economists who have been brought up on the assumption of making no interpersonal welfare comparisons. However, they were not strange in the early 1900s when, in applied policy analysis, economists believed that interpersonal comparisons had to be made to have a practical measure of welfare to use in policy analysis. The best of these economists, such as Fisher, fully understood the analytic difficulties and the assumptions one would have to make to move from the theoretical concepts to the empirically measured concepts. Fisher (and Ragnar Frisch, who carried on with Fisher's approach) carefully specified those theoretical difficulties and assumptions necessary for their method of drawing inferences about utility.

Fisher's (1927, pp. 179–180) justification for the interpersonal comparisons of wants, which his statistical method required, was pragmatism. He posed the rhetorical question whether the necessary assumptions can be used, and answers: "To all these questions I would answer 'yes'—approximately at least. But the only, or only important, reason I can give for this answer is that, in actual practical human life, we do proceed on just such assumptions." He continues: "Philosophical doubt is right and proper, but the problems of life cannot, and do not, wait."

Fisher (1927) claimed that he had applied his method to certain available statistics of the U.S. Bureau of Labor Statistics. He wrote (p. 193): "The results confirm the common idea that progressive rather than regressive taxation of incomes is justified." However, Fisher does not present the statistics in the essay, explaining that the data "need to be 'smoothed' and subjected to critical analysis for varying size of family and other complications before the results can be considered even roughly accurate."

While Fisher did not further develop his measurement, Ragnar Frisch, a Norwegian economist who visited Yale in the late 1920s and worked with Fisher, did. Specifically, Frisch (1932 [1978]) expanded on Fisher's work, and both further developed an axiomatic foundation to utility measurement that became indifference curve analysis and provided some estimates of statistical measurements of utility at different income levels. Following up Fisher's ideas, Frisch collected data on spending patterns for groups of people in different cities where real income varied over time. He argued that it was reasonable to assume that the average utility functions of these broad groups were comparable, and that, by measuring how spending patterns changed over time as real income in these areas fluctuated, he could determine what he called a "flexibility curve." He further argued that this flexibility curve could be integrated to arrive at an ordinal measure of utility at different levels of income. Fisher (1927) and Frisch (1932 [1978]) offer a detailed discussion of the approach. Frisch then used those statistical measurements to argue for the justice of a progressive income tax.

Fisher's approach did not catch on. With the economics profession's acceptance of Lionel Robbins's (1932) argument against using interpersonal welfare comparisons, which were necessary to use the statistical measurement approach,

Fisher's statistical measurement approach was abandoned in scientific economics.[6] If one could not compare utilities among individuals, or at least groups of individuals, then all hope of actually developing a workable measure of utility for applied policy was hopeless. Thus, with the acceptance of Robbins's arguments, all that remained of Fisher's approach was the axiomatic foundation to utility theory. It is that axiomatic approach that shows up in intermediate micro texts.[7]

Conclusion

Both Edgeworth and Fisher knew that their approaches to utility measurement opened up a Pandora's box of problems, most specifically the issue of interpersonal utility comparisons. But they opened that box nonetheless because they felt that theoretical economics had to be relevant to policy, and, to be relevant, theoretical economics had to face those problems. In the 1930s, the economics profession decided to close that box and not to follow up either Edgeworth's direct approach or Fisher's indirect approach to utility measurement. That freed up theoretical work and applied policy work to go their separate paths. Theoretical welfare economics abandoned utility measurement as unnecessary. The problems of that abandonment for theory were noted by Jan de V. Graaff (1959, p. 169), who in his book *Theoretical Welfare Economics* concluded that without making some interpersonal utility comparisons "the possibility of building a useful and interesting theory of welfare economics—i.e. one which consists of something more than the barren formalisms typified by the marginal equivalences of conventional theory—is exceedingly small."

Real-world applied work didn't abandon utility measurement; instead, until recently, it abandoned utility theory, and adopted simplistic measures of utility that didn't take into account the theoretical problems with which Fisher and Edgeworth were struggling. The problems of doing so were noted by Slesnick (1998, p. 2019) who wrote that until recently there has been little cross fertilization between theory and applied policy work, and that the standard applied economist's approach of assuming a representative consumer in applied policy work is "unappealing both because distributional issues are ignored and because much evidence shows that

[6] In Colander (2005), I argue that Robbins's views were misinterpreted, and that in applied work, he believed that interpersonal comparisons were necessary and acceptable. He just did not want that work called science.

[7] The debates on measuring utility continued after the period I have discussed, as economics moved to ordinal measures of utility and Pareto optimality as the central welfare criteria. Stigler (1950) traces that development through Marshall, Pareto, and Slutsky. Ultimately, economics moved away from calculus, which framed these earlier discussions, and developed a set-theoretic axiomatic foundation for utility, thereby restructuring the discussion. In that set-theoretic definition, utility is a formal ranking without any sensory elements. Chipman (1960, p. 221) summarizes the axiomatic definition nicely: "Utility, in its most general form, is a lexicographic ordering, represented by a finite or infinite dimensional vector with real components, unique only up to an isotone (order-preserving) homogeneous transformation."

aggregate demands are inconsistent with the behavior of a single representative agent."

Given the problems created by the separation of theory and applied work, it is a positive sign that modern theoretical economists are returning to utility theory and to utility measurement issues, and are reopening the questions that led economists to set aside large portions of this topic in the 1930s. But the path forward will not be easy. Behavioral economics, by showing that context and framing of decisions matter, is undermining any simply backward induction from agents' choices to agents' welfare, which was the foundation of Fisher's indirect utility measurement approach. If an indirect approach must take into account people's behavioral characteristics, then the measurement issues based on an individual's revealed choices become much more complicated.

Despite the problems which behavioral economics presents for his approach, I suspect that Fisher would have applauded this new work for the same reason that he applauded behavioral psychology earlier—it was dealing with real-world observations. Similarly, I suspect that Edgeworth would have been a strong supporter of neuroeconomics work with brain scans to relate experience to a person's pleasure and pain (Camerer, Lowenstein, and Prelec, 2005). While this work is far from Edgeworth's hedonimeter, its careful consideration of the neurological foundations of pleasure provides insight into how people respond to different events, something that Edgeworth felt was necessary to apply economic theory.

Where this new theoretical and empirical work on measurement issues of happiness and utility will lead is uncertain, but I strongly believe that economics will be better for it. The result will be a richer and more nuanced relationship between economics and policy than currently exists.

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