

# Harold Hotelling and the Neoclassical Dream

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*There is no path, there is no path at all,  
Unless perhaps where abstract things have gone  
And precepts rise and meta-physics fall,  
And principles abandoned stumble on.  
No path, but as it were a river in spate  
Where drowning forms, down-swept, gesticulate.*

*Malcolm Lowry*

### **1. Down to the Field of Dreams in Our Time Machine**

There is a daydream that many intellectual historians seem to share, indulging in it at the slightest provocation. In this dream, they climb into a time machine and are transported back to the classical origins of their doctrine or discipline. The time machine apparently has no difficulties in locating this omphalos, since it merely has to retrace (in reverse) the single lineage which stretches in a straight line from Now to Then.

We would like to invite the reader into our own version of an intellectual time machine. In particular we would like to transport the reader back to the time when the "theory of demand" was little more than the suggestion that a stable relationship existed between the price of a good and the quantity of the good that would be sold at that price. But we caution our fellow-travelers in this virtual dreamtime that they may be in for a rather bumpy ride, primarily because the destination lacks determinate coordinates. By this we mean that our trip will be quite different from the trip provided by those such as Creedy (1992) or Stigler (1965) who would whisk us back three centuries to the so-called "King-Davenant Law" or something of that sort (perhaps that story should be reserved for Back to the Future: Part 7). Rather, we will set the controls for a

more unsettled time, a time when there was little agreement about "demand" being anything other than a phenomenal regularity, and yet at the same time it was widely recognized that a mere phenomenological relation in price-quantity space was just not good enough (there were too many intervening variables such as income, composition of the target population, interactions with other commodity demands, problems of noise and errors, vicissitudes of the passage of time, and the like). But while our destination is a tumultuous time, we will not be able to consider all of the different individuals who contributed to the intellectual fervor. We will restrict ourselves to the class of economists who would acknowledge that they operated within the same rough doctrinal school or foundational orientation; so for instance, although some contemporary American Institutionalists like Frederick Mills were also equally absorbed by the empirical implications of price-quantity interactions, we shall arbitrarily omit him from our itinerary. Hence, the place where we should like to transport the reader is to the time just before the neoclassical orthodoxy (or what some modern textbooks -- Mas-Colell, Whinston, and Green, 1995, for instance -- call "classical consumer theory") coalesced around the apparatus of income and substitution effects, Slutsky symmetry conditions, parametric prices, and the tools of simultaneous equation econometrics. Our destination thus lies somewhere between Alfred Marshall and the Cowles Commission.

But why should the reader join us in our little outing? Well, for one thing, we promise a much higher quality ride than what has previously been available in the literature. The conventional straight line from Marshall to Slutsky to Hicks/Allen to Samuelson badly misrepresents the situation on the ground from both a methodological and historiographical standpoint. A wide range of actors whom we believe were important (both locally and in retrospect) have been shamefully neglected by historians of economic thought: here we shall

focus upon Henry Schultz and Harold Hotelling, although the list might justly be expanded to include Henry Ludwell Moore, Edwin Bidwell Wilson, Sewall Wright, Nicholas Georgescu-Roegen, Charles Roos, Jacob Marschak, Griffith Evans, Milton Friedman, Holbrook Working, and a whole host of others. Also, we shall restrict ourselves to events in the United States, partly for convenience, although also partly upon principle: national context and culture matters for the story we wish to tell. Thus our coordinates grow narrower and more precise, our destination more tangible.

Yet the more tangible the destination, the less our readers might like to come along for the ride. "Who cares about Schultz and Hotelling?" they might object. "What are the dreams of an obscure but respected man like Hotelling to me, someone interested in the progressive character of a powerful empirical science like neoclassical economics?" Therein lies our tale. Our major thesis is that even within the arbitrarily narrow parameters we have just set, there is not, nor has there ever been, a single discrete neoclassical demand theory, and that this may have something to do with its demonstrable power. While historians might feel comfortable with such arguments (though we must confess we have yet to find such a person in the community of historians of economic thought), methodologists would normally quail at such a prospect.

Our contention is that one of the ways to understand the development of a scientific discipline is to focus on its hopeful failures. By hopeful failures, we mean programs or approaches that seem to provide solutions to a number of the discipline's stated problems, and yet do not get chosen to become a part of established science. An examination of why such programs do not get chosen should help us to better understand the various forces and interests that are at work in determining which things do get chosen as established science.

Such rejected knowledge does not get much attention in "Whig" histories

of science. According to such histories, science exhibits consistent and systematic progress; those views that are rejected by the scientific community are, for the most part, rejected for the right reasons. Whig histories are often tied to a particular normative philosophy of science. The claim is that there exists a proper (and relatively simple) "scientific method" and that such a method is manifest in the history of successful science; what came to be accepted was accepted for essentially the right reasons. Such histories are fundamentally evaluative and the historian's argument for why a particular view gets evaluated as it does is the same as the winner's argument for why they are winners; the values of the winners are deployed in the evaluation of the losers.<sup>1</sup> Recently, more contextual and sociologically-inspired studies in the history of science are less likely to take such a dismissive stance toward rejected knowledge. For example, in Leviathan and the Air Pump (1985), Steve Shapin and Simon Schaffer examine the 1660s natural philosophy controversy between Thomas Hobbes and Robert Boyle. Hobbes was clearly the loser in this debate, and as a result his work was written out of the history of science by the late eighteenth century (this is despite his status and prestige in other areas of philosophy). Unlike the standard "Whig" history of this episode, the narrative presented by Shapin and Schaffer gives an earnest rendering of Hobbes's position and does not seek to evaluate it from the winner's perspective. Their story treats accepted knowledge (Boyle) and rejected knowledge (Hobbes) symmetrically and

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<sup>1</sup> "Here we see the germ of a standard historiographic strategy for . . . handling rejected knowledge in general. We have a dismissal, the rudiments of a causal explanation of the rejected knowledge (which implicitly acts to justify the dismissal), and an asymmetrical handling of rejected and accepted knowledge. First, it is established that the rejected knowledge is not knowledge at all, but error. This the historian accomplishes by taking the side of accepted knowledge and using the victorious party's causal explanation of their adversaries' position as the historian's own. Since the victors have thus disposed of error, so the historian's dismissal is justified" (Shapin and Schaffer, 1985, p. 11).

attempts to understand the various forces that were influential in determining which adjective came to be attached to which knowledge claim. Such stories may involve the traditional philosophical idiom of "empirical facts" and "epistemic virtues," but they do so in a substantially more solicitous and nuanced way than these terms are used in Whig histories.

This paper considers a bit of rejected knowledge from the history of early twentieth century demand theory; we shall maintain that the mistake of previous commentators has been to uncritically regard this incident as minor, or to neglect it altogether. The rejected knowledge we shall highlight is the theory of demand presented in Harold Hotelling's paper on "Edgeworth's Taxation Paradox" in the Journal of Political Economy in 1932. We will examine this paper in detail, and demonstrate that it solves quite a number of the discipline's perceived problems -- problems from the 1930s as well as problems associated with the Arrow-Debreu general equilibrium model in the 1950s and 1960s -- and then to offer a contextually situated discussion of why Hotelling's model nonetheless did not become accepted knowledge: that is, we will explore the question of why Hotelling's view did not subsequently become the standard demand theory within neoclassical economics.

We feel impelled to insert at least three caveats before we climb into our own time machine and embark in search of this neglected creature, the rejected bit of science that might have worked out under different circumstances. First, we would like to make it clear that we do not seek to argue that Harold Hotelling was "right" and Eugen Slutsky was "wrong" about the proper elaboration of neoclassical demand theory. Our story is certainly not a story about what "should" or "should not" have emerged from this tumultuous period; it is not just a new twist on the same old morality play about good and bad economics. Such a normative project would be quixotic and pointless, and

more importantly, it would subvert our stated intention to treat winners and losers in a more symmetric manner.

Second, this account cannot stand alone as a self-sufficient chapter of the history of neoclassical demand theory. We are painfully aware of what it would take to adequately demonstrate the existence of many neoclassical demand theories in all their historical splendor and specificity. After all, it is our position that the conventional boxes in which the story has previously been retailed -- say, the "history of econometric ideas" (Morgan, 1990; Christ, 1985; Epstein, 1987); the "history of empirical studies of consumer behavior" (Stigler, 1965); "the development of utility theory" (any history of economics textbook); "the discovery of the law of demand" (Creedy, 1992); the "history of welfare economics"; the "history of mathematical economics" (Mirowski, 1991); the history of methodological doctrines (Blaug, 1992; De Marchi and Blaug, 1991) and yes, even "the history of energy physics" -- are a big part of the problem. One can only tell the symmetrical story of rejected knowledge which we propose by violating those categories, and bursting all those neatly stacked boxes on the storage shelf of economic knowledge asunder. But that, in turn, would require a thorough rewriting of the received history of neoclassical economics: one in which we are already engaged (Mirowski, 1989; 1990; 1994; Hands, 1993; 1994), and one which when completed will stretch from Francis Edgeworth and Henry Ludwell Moore through to the construction of the Chicago school and the Arrow-Debreu stabilization (Weintraub, 1991; Weintraub & Mirowski, 1994). Yet even while admitting those perhaps vaulting ambitions, it does seem to both of us that the Hotelling/Schultz encounter is one of those pivotal events around which the entire structure of neoclassical demand theory pirouetted, wobbled, but then miraculously recovered its balance. For Henry Schultz was an avowed disciple of Moore at Columbia, whom himself had earlier clashed precipitously

with Edgeworth over the very issue of the viability of empirical demand estimation, and while Schultz was on the faculty at the University of Chicago one of his most illustrious undergraduate students was Paul Samuelson; he also mentored Milton Friedman and encountered George Stigler. Harold Hotelling's contribution, on the other hand, was prompted by Edgeworth's paradoxical paper on taxation: and while teaching mathematical economics and statistics at Columbia, Milton Friedman was one of his students and he supervised the Ph.D. thesis of Kenneth Arrow. Many of the major streams of neoclassical thought about the nature of demand converged with alacrity upon these two men, and out of their struggle with these issues was conceived the three major skeins of 1960s American neoclassical orthodoxy: the MIT revealed preference doctrine, the Chicago Marshallian variant, and the Cowles general equilibrium approach. It's a great story, and there is no way we can tell it all here.

Thirdly, and equally humbling, is the fact that we do not feel we can draw any simple methodological rules out of our present narrative -- unlike, say Green and Moss (1994) or chapter 6 of Blaug (1992) -- basically because we believe that no small set of methodological rules can adequately account for the fact that one variant was chosen and stabilized, eventually coming to dominate another; our story is no more a meta-lesson for philosophers than it is a parable for economists. This is a thesis we can only hope to illustrate here, postponing for a later venue the extended argument. However, it will not escape the devotee of recent developments in science studies that we are favorably inclined towards the position of the Social Studies of Knowledge (SSK) that science is the complex product of interests, communities, unanticipated consequences and cultural formations. What happened in economics was as much the product of the Great Depression and World War II or contemporary upheavals in physics as



it was of perspicacious farsighted theorists and empiricists subjecting their ideas to astringent tests; nothing less in the way of narrative accounts will do, as we here attempt to illustrate. Nevertheless, the absence of any clean "scientific method" does not imply randomness, whimsy or bald power plays were the predominant order of the day.

The remainder of this paper is organized along the following lines. The next section provides a relatively abstract statement of Hotelling's proposed solution to the problem of grounding empirical demand theory in a coherent foundational theory of the maximization of utility. We have dubbed this solution "Hotelling's Dream," not only because it would serve his own specific purposes, but also because it would solve five generic methodological problems of the nascent neoclassical program, as well as conform to a major heuristic which we proceed to identify in section 3. Section 4 demonstrates in a telegraphed manner just how Hotelling's version of demand theory could have suitably fulfilled those goals, if only it had become the orthodoxy. Section 5 is intended to buttress the preceding account with the story of the actual Schultz/Hotelling encounter, just the sort of thickly textured narrative calculated to gladden the heart of an intellectual historian, based upon both published and archival sources. Precisely because it is real history, the story becomes appreciably more complex in this section than in our preceding outlines. The final section of the paper provides a brief sketch of what actually happened to orthodox neoclassical demand theory after the 1930s, when Hotelling's Dream was downgraded to Hotelling's Lemma, while the rest of his version of neoclassicism was relegated to the realm of the unthinkable after both our protagonists had abandoned the field. Some major players, such as Paul Samuelson, can be held relatively responsible; but historical accident also played a significant part. As is our wont, we conclude by entertaining the unthinkable,

offering a set of possible explanations of this phenomenon, and drawing some cautionary consequences for the economic methodologist.

## **2. Hotelling's Dream**

The original motivation for Hotelling's (1932a) paper was the so-called "Edgeworth taxation paradox": the rather counterintuitive argument that a tax imposed on one good may actually lower the price of that good as well as the price of certain related goods. Edgeworth first published the result in Italian in 1897, but it was accessible to Hotelling in Edgeworth's Papers Relating to Political Economy (1925, I, 132, 143ff; II, 401). Edgeworth's original example was based on a profit maximizing monopoly railway supplying two classes of passenger rail service. Hotelling's 1932 paper employed an early example of the optimization-based differential comparative statics that subsequently became standard in post-Foundations (Samuelson, 1947) economic analysis, and demonstrated that in the case of interrelated commodities it was possible for a tax on one good to cause a reduction in the prices of both goods, even when they were sold under conditions of "free" competition. Edgeworth's result did not depend on monopoly, Hotelling insisted, but rather was an implication of the interrelatedness of commodity demands.

Hotelling's characterization of demand in this paper was quite different from the consumer choice characterization that is now standard in microeconomics textbooks; the modern view descended more directly from Fisher (1892), Pareto (1927), and Slutsky (1915) than Hotelling. While the standard textbook version was also present, and perhaps even the dominant, during the 1930s -- particularly after Hicks and Allen (1934) -- the word "dominant" should be used rather gingerly. The entire program of what is

now called neoclassical microeconomics was still quite inchoate during the 1930s and that instability would continue for the next thirty years or so; "dominant view" in this context only means that it was the characterization of consumer choice that seemed to be most accepted by the specific coterie of economists who were concerned with the theoretical grounding of demand curves under conditions of interdependent multicommodity utility maximization.

Before returning our attention to Hotelling's approach, it may be useful for comparison purposes to quickly review the "standard" textbook neoclassical approach to the consumer choice problem. In the standard n-good consumer choice problem the consumer maximizes utility subject to a budget constraint. The utility function  $U(x)$  is a real valued function of the quantities of the n goods given by the vector  $x=(x_1, x_2, \dots, x_n)$ . The budget constraint simply says that the money value of the goods purchased at the price vector  $p=(p_1, p_2, \dots, p_n)$  can not exceed the money income M. The standard consumer choice problem can thus be written as,

$$\begin{aligned} & \text{Max } U(x) \\ \text{Subject to: } & \sum p_i x_i = M \end{aligned}$$

where the utility function has sufficient structure to satisfy the second order conditions for this constrained optimization problem (it is strictly quasi-concave).

The Lagrangian function for this constrained problem is,

with  $\lambda$  as the Lagrangian multiplier. There are n+1 first order conditions for this problem; they are given by,

$$\partial L / \partial x_i = \partial U / \partial x_i - \lambda p_i = 0 \quad \text{for all } i=1, 2, \dots, n,$$

$$\text{and } \partial L / \partial \lambda = \sum_{i=1}^n p_i x_i - M = 0.$$

Solving these  $n+1$  first order conditions determines the  $n$  demand functions  $x(p, M)$  and the optimal value of the Lagrange multiplier  $\lambda(p, M)$ .

Unlike the standard consumer choice model, Hotelling's characterization of demand in the 1932 paper was based on the decision-making of an "entrepreneur," someone who is purchasing goods in order to resell them. The focus on such "entrepreneurial" demand functions meant that Hotelling was "restricting attention to those cases in which money is spent, as the saying is, to make money" (1932a, p. 592). Hotelling did not believe this case to be very restrictive or empirically unusual. He spent a fairly large portion of the paper (particularly pp. 592-94) trying to convince the reader that his case of entrepreneurial demand actually represented a significant portion of the actual consumer choice problems; he claimed that the only difference between his view and the more standard view of utility maximization subject to a linear budget constraint was the Giffen good "case of a rising demand curve for bread sometimes supposed to occur" (1932a, p. 593). Hotelling made similar arguments about the empirical relevance of entrepreneurial demand in unpublished correspondence with Henry Schultz, as we shall discover below in Section 5. Schultz had read an early version of the paper and responded that similar demands could be derived from the standard model by assuming that the marginal utility of money income ( $\lambda$ ) was constant with respect to (independent of) prices.<sup>2</sup> Hotelling agreed with Schultz on this point, but claimed that it was just another reason for employing the entrepreneurial approach.

Hotelling started with the sales or total revenue function of the

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<sup>2</sup> The argument will be presented below.

entrepreneur. This function is given by,<sup>3</sup>

$$U(x) = U(x_1, x_2, \dots, x_n), \quad (1)$$

where  $x=(x_1, x_2, \dots, x_n)$  is the quantity vector of the  $n$  commodities. With  $U(x)$  as total revenue, the entrepreneur's objective function of net revenue (or profit) of the is given by,

$$(2)$$

where  $p=(p_1, p_2, \dots, p_n)$  is the vector of commodity prices.

The first order conditions for this (unconstrained) maximization of the objective function in (2) are given by,

$$\partial U / \partial x_i = p_i \quad \text{for all } i=1, 2, \dots, n. \quad (3)$$

These  $n$  first order conditions can be solved for the  $n$  (inverse) demand functions:

$$p_i(x) = p_i(x_1, x_2, \dots, x_n) \quad \text{for all } i=1, 2, \dots, n. \quad (4)$$

Hotelling assumed these could be inverted to yield the demand functions,

$$x_i(p) = x_i(p_1, p_2, \dots, p_n) \quad \text{for all } i=1, 2, \dots, n. \quad (5)$$

Differentiation of the first order conditions in (3) gives,

$$\partial^2 U / \partial x_i \partial x_j = \partial p_i / \partial x_j = \partial p_j / \partial x_i = \partial^2 U / \partial x_j \partial x_i \quad \text{for all } i \text{ and } j, \quad (6)$$

which implies that the (inverse) demand functions satisfy the following reciprocity (symmetry) conditions,

$$\partial p_i / \partial x_j = \partial p_j / \partial x_i \quad \text{for all } i \neq j. \quad (7)$$

Hotelling calls these symmetry conditions the "integrability conditions" (1932a, p. 591) for entrepreneurial demand functions. These conditions are much stronger<sup>4</sup> than the (Slutsky) symmetry conditions for the standard consumer

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<sup>3</sup> In general we will not adhere to the symbolism used in the papers we discuss. For example Hotelling used  $q$ , not  $x$ , for the quantities of commodities. In this case though, it should be noted that Hotelling did use U for total revenue: not TR, but U. The hint of utility maximization is not very subtle.

choice problem. Hotelling clearly recognized that these conditions are quite restrictive and admitted that they "may not hold accurately" for the case where "the money expenditure is absolutely fixed" (1932a, p. 592).

The symmetry (or immediate integrability) conditions in (7) are written for the inverse demand functions  $p_i(x)$ , but since  $[Jp]=[Jx]^{-1}$  these conditions also imply the symmetry of the demand functions  $x_i(p)$ .<sup>5</sup> Thus the conditions,

$$\partial x_i / \partial p_j = \partial x_j / \partial p_i \quad \text{for all } i \neq j, \quad (8)$$

also hold for Hotelling's demand functions. The particular way the choice problem is specified guarantees the symmetry of both the inverse and regular demand functions.

As Henry Schultz pointed out in his 1933 paper on "Interrelations of Demand," Hotelling's symmetry conditions (7) can be derived as a special case of the standard consumer choice problem. If the choice problem is characterized in the standard way, but the additional (Marshallian) assumption is added that the marginal utility of money income ( $\lambda$ ) is constant, then the symmetry conditions given in (7) follow immediately. To see this result recall the first order conditions for the standard consumer choice problem,

$$\partial L / \partial x_i = \partial U / \partial x_i - \lambda p_i = 0 \quad \text{for all } i=1, 2, \dots, n$$

<sup>4</sup> Empirically these conditions imply that a change in the quantity of apples consumed will have the same impact on what the consumer is willing to pay for oranges, as a change in the quantity of oranges consumed will have on what the consumer is willing to pay for apples: the same in both direction and magnitude.

Mathematically these conditions imply that the differential form,

$$p_i$$

is immediately integrable. We follow Afriat (1980) in making the distinction between an "integrable" differential form -- one where an integrating factor exists -- and an "immediately differential form" -- one where the integrating factor is 1.

<sup>5</sup> Throughout the paper we will use the symbol  $[Jf]$  for the Jacobian matrix of a function  $f:R^n \rightarrow R^n$  with representative element  $[\partial f_i / \partial x_j]$ , and the symbol  $[Hf]$  for the Hessian matrix of a function  $f:R^n \rightarrow R$  with representative element  $[\partial^2 f / \partial x_i \partial x_j]$ ,

$$\text{and } \partial L / \partial \lambda = \sum_{i=1}^n p_i \quad .$$

Solving this system for the optimal values not only gives the demand functions  $x(p, M)$ , or the inverse functions  $p(x, M)$ , but it also gives the optimal value of the Lagrange multiplier  $\lambda^*$  which in general will also be a function of the parameters ( $p$  and  $M$ , or  $x$  and  $M$ ). Now suppose that the Lagrange multiplier  $\lambda$  is a constant (not a function of the parameters of the problem).<sup>6</sup> In this case differentiation of the above first order conditions with respect to some arbitrary  $x_j$  gives,

$$\partial^2 U / \partial x_i \partial x_j = \lambda \partial p_i / \partial x_j \quad \text{for all } i \quad \text{since} \quad p_i \partial \lambda / \partial x_j = 0,$$

and this in turn generates the symmetry conditions in (7) in exactly the same way that they were generated by the equalities in (6). Of course this does not say that the standard demand functions produced from a particular utility function and the added assumption of the constancy of the marginal utility of money income, will be exactly the same as the Hotelling demand functions produced by using that same utility function as the total revenue function in Hotelling's problem, but it does say that the addition of the assumption of the constancy of the marginal utility of money income to the standard consumer choice problem will produce demand functions that have symmetric cross-partial derivatives, i.e. satisfy condition (8).<sup>7</sup>

Regardless of how they are derived, the symmetry conditions in (8) imply

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<sup>6</sup> As Samuelson (1942b) demonstrated, it is not possible for  $\lambda(p, M)$ , or  $\lambda(x, M)$ , to be independent of all the parameters in the problem, but it is possible to have  $\partial \lambda / \partial p_i = 0$  for all  $i$  [or  $\partial \lambda / \partial x_i = 0$  for all  $i$ ]. The relationship between Hotelling's integrability conditions and the standard Slutsky conditions are discussed in Pfouts (1995).

<sup>7</sup> Samuelson (1950, p. 357, n. 3) discusses a special case of the standard problem that generates demand functions that have the same properties as the demands generated by Hotelling's problem. It is the case of quasilinear preferences where the utility function has the form,  $U(x) = x_1 + V(x_2, x_3, \dots, x_n)$ , and good 1 is chosen as numeraire.

that a potential function exists for the vector field defined by the demand functions  $x_i(p)$ .<sup>8</sup> Hotelling calls this potential function the "price potential."

Just as we have a utility (or profit) function  $U$  of the quantities consumed whose derivatives are the prices, there is, dually, a function of the prices whose derivatives are the quantities consumed. The existence of such a function, which heretofore does not seem to have been noticed, is assured by (7). On the basis of physical analogies we may call this the "price potential" (1932a, p. 594).

In this paper, as well as in his 1938 paper, the symmetry conditions (7) and (8) were used primarily for exercises in welfare economics (or more properly, for exercises in the kind of economic analysis that eventually came to be called welfare economics). As Hotelling says in his 1938 paper:

In the paper on "Edgeworth's Taxation Paradox"... I have shown that there is a good reason to expect these integrability conditions to be satisfied, at least to a close approximation, in an extensive class of cases. If they are satisfied, the surpluses arising from different persons, may be added to give a meaningful measure of social value. (1938a, p. 247)

If we focus exclusively on the demand side of the market (thus neglecting producer's surplus) then Hotelling's social welfare argument can be interpreted in the following way. When the symmetry condition (7) holds, the (inverse) demand functions  $p(x)$  define an exact differential. This means that the Welfare line integral ( $W$ ), defined by,

$$\int_C \sum_i p_i dx_i \quad (9)$$

is independent of path (it has the same value for any two endpoints regardless of the path that is taken between those two endpoints).<sup>9</sup> In addition, if the path is

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<sup>8</sup> The conditions in (8) imply that there exists a function  $F(p)$  such that  $x(p)=\nabla F$  (i.e.  $x_i(p)=\partial F/\partial x_i$  for all  $i$ ). The function  $F$  is called the potential function for the vector field  $x$ .



a closed curve -- if it begins and ends at the same point -- then the value of the welfare integral is zero; the demand functions  $p(x)$  generate a "conservative" vector field. The welfare measure  $W$  is the natural general equilibrium extension of Marshall's notion of total social benefit: the concept of total social benefit that forms the basis of consumer's surplus calculations. Consumer's surplus and the related concept of dead weight loss continues to be the theoretical mainstay for most applied welfare economics and cost-benefit analysis;<sup>10</sup> Hotelling's demand functions guarantee that such measures are appropriate tools for the evaluation of social utility in the multiple (and interrelated) market context.

While we have focused on the one particular aspect of Hotelling's paper that did not become part of standard neoclassical theory -- his demand functions -- it is important to note that the paper has generally been quite influential and that many of its other theoretical innovations have been integrated into mainstream neoclassical theory. For example, when the symmetry conditions in (8) are applied to the factor demand functions of a perfectly competitive

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<sup>9</sup> Here, as elsewhere in this paper, we focus exclusively on demand theory and neglect the supply side of the story. Hotelling's actual welfare measure was,

$$S = \int_0^x p(x) dx - \sum_i s_i(x)$$

where  $s_i(x)$  is the supply price of good  $i$  at the quantity vector  $x$  (marginal cost of good  $i$ ).

We neglect production and supply in part because the paper is "about" demand theory (not all of neoclassical microeconomics), but we also neglect it because the issues we are concerned with are less apparent in production theory. For example, while Hotelling's symmetry conditions (8) are not part of modern demand theory, they are standard in modern production theory. This is certainly not to suggest that modern production theory is devoid of problems; it is only to assert that its particular problems are not the topic of this paper. See chapter 6 of Mirowski (1989) for a discussion of some of the difficulties of neoclassical production theory.

<sup>10</sup> See Harberger (1971), or the discussion of "Hotelling-Harberger" welfare measures in a recent cost-benefit textbook (Zerbe and Dively, 1994, pp. 480-84, for instance). The necessity of neglecting income effects in Consumer's surplus-based welfare economics is emphasized in chapter 10 of Mas-Colell, Whinston, and Green (1995).

firm, they become "Hotelling's Lemma," a result that is enshrined in the production chapter of almost every modern textbook. Hotelling's paper also played a prominent role in Samuelson's discussion of "maximum principles" in his Nobel Lecture (1971); out of the myriad of applications of maximum principles in economics that Samuelson might have chosen to discuss, he opted for precisely those offered in Hotelling's 1932 paper.<sup>11</sup> Finally, as suggested above, Hotelling's papers had an important impact on applied welfare economics and cost-benefit analysis. An example of this impact is provided by John Chipman when he cites "the authority and influence of Hotelling" (1982, p. 55) as one of the main reasons why Samuelson's work did not have a greater influence on cost-benefit analysis.

### **3. Back to the Future, with Alarm Clock, Part 1**

By the 1960s, the heyday of Walrasian general equilibrium theory, most of the economists participating in the Arrow-Debreu program seemed to have reached a consensus regarding the project's basic theoretical "goals." Of course not all of these goals were of equal interest to every participant, nor were these theoretical goals entirely isolated from the other interests of those involved in the Walrasian program. The theoretical goals of this community, like those of any scientific community, were deeply intertwined with the other goals -- practical, personal, pragmatic, political, cognitive, etc. -- of the program's participants. What a particular scientific community is trying to achieve theoretically, the things that would count as a theoretical "success," depends fundamentally on the

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<sup>11</sup> "One of the pleasing things about science is that we do all climb towards the heavens on the shoulders of our predecessors. Economics, like physics has its heroes, and the letter 'H' that I used in my mathematical equations was not there to honor Sir William Hamilton, but rather Harold Hotelling" (Samuelson, 1971, p. 7).

other goals, the other shared values, of the members of that community.

Despite the interdependency of the various goals and values of the scientific community, it is possible to isolate the main theoretical goals of the Arrow-Debreu general equilibrium program. Although we will discover that many of these theoretical goals were at odds with some of the other values shared by the program's participants, these goals can nonetheless be isolated and examined. At this particular point we will not ask why these theoretical goals were sought; we will only focus on specifying them.

Since most of the theoretical discourse regarding the Arrow-Debreu general equilibrium model has taken place within the context of a relatively formalized mathematical structure, introducing some of that structure seems to be useful in characterizing the goals of the Walrasian program. The easiest way to proceed is to specify one particular generic version of the Arrow-Debreu model and then to use that version as a general framework for introducing both the program's goals in this section, and also returning to Hotelling (1932a) in the following section. Of course the model we will present is just one particular version of a very general class of models; not every general equilibrium model of the period had exactly this structure or employed this particular symbolism. Since we are primarily concerned with demand theory -- in particular the relationship between Hotelling's characterization of the consumer and the view that became the standard view of consumer choice -- we will neglect production and focus exclusively on a pure exchange economy. We will also restrict ourselves to a differential version of the model; in addition to its mathematical convenience, this will also make the connection to the neoclassical economics of the 1930s as clear as possible.<sup>12</sup>

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<sup>12</sup> This means that the Arrow-Debreu model we will be considering will look more like Arrow and Hahn (1971) or Quirk and Saposnik (1968), than Debreu (1959).

Let the economy have  $H$  individual traders indexed by  $h=1, 2, \dots, H$  and  $n+1$  goods with unit of account prices given by  $p=(p_1, p_2, \dots, p_{n+1})$  with  $p_i > 0$  for all  $i=1, 2, \dots, n+1$ . Each individual  $h$  has an endowment vector  $\omega^h$  and maximizes a strictly quasi-concave utility function subject to the income constraint  $M^h = \sum p_i x_i^h$ .

The demand for good  $i$  by individual  $h$  is given by the function  $x_i^h(p)$ . The aggregate excess demand for good  $i$  at price vector  $p$  will be given by  $Z_i(p)$  where,<sup>13</sup>

$$Z_i(p) = \sum_{h=1}^H x_i^h(p) - \omega_i^h$$

The most common assumptions on the aggregate excess demand function  $Z(p)=[Z_1(p), Z_2(p), \dots, Z_{n+1}(p)]$  were Walras's Law (W) and zero degree homogeneity (H) at all prices. Thus,

$$\sum_{i=1}^{n+1} p_i Z_i(p) = 0 \quad (W)$$

The aggregate excess demand function  $Z(p)=[Z_1(p), Z_2(p), \dots, Z_{n+1}(p)]$  with restrictions (W) and (H) became the core theoretical concept for the Arrow-Debreu Walrasian general equilibrium model. There were many special cases with additional structure, but this remained the basic model. Under (H) prices can be normalized. One common way to normalize prices was to let good  $n+1$  be the numeraire; this means that  $p_{n+1}=1$  and the prices of the  $n$  other goods are given as relative prices in terms of good  $n+1$ . We will continue to use the same symbolism  $(p)$  for normalized or non-normalized prices.

We can now proceed to identify five theoretical goals of Arrow-Debreu

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<sup>13</sup> In general we will use lower case letters for individual demand ( $x$ ) and excess demand ( $z$ ), and upper case letters for aggregate (market) demand ( $X$ ) and excess demand ( $Z$ ). The symbol  $x(p)$  was used for the special case of the individual Hotelling demand in the previous section to distinguish it from the standard neoclassical demand function  $x^h(p)$  in this section.

general equilibrium theory, as well as what we consider a governing heuristic. The first three of these goals should be uncontroversial; they are the goals clearly identified by the program's participants and/or the authors of the canonical texts, such as Arrow and Hahn (1971). The next two goals on our list may be slightly more controversial, but we doubt they will raise much of a fuss. These two goals were also clearly identified in the canonical texts; what makes them slightly different from the first three is that there was less agreement about what it would take to "satisfy" these goals. The first three goals were generally accepted by the program's participants and clearly defined; the next two were generally accepted by the program's participants but much less clearly defined.<sup>14</sup> Our heuristic, by contrast, is not one that has been uniformly identified or much discussed by the authors of the canonical texts or by subsequent commentators, although one can find it in the margins of many of their texts, as we shall demonstrate. This heuristic connects the Arrow-Debreu program with the neoclassicism of the late nineteenth century via the argument presented in Mirowski (1989).

i) The first goal is existence. The existence question is the issue of whether the Arrow-Debreu model has an equilibrium price vector. Informally it answers the question: Does there exist an price vector  $p^*$  that simultaneously clears all markets? A bit more formally (while still restricting ourselves to strictly positive prices) the existence question is whether the equilibrium price set

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<sup>14</sup> Arrow and Hahn (1971) clearly documents the importance of these six goals. Chapters 2 and 5 of Arrow and Hahn are dedicated to the first goal on our list. Their Chapter 9 focuses exclusively on our second goal, while Chapters 11 and 12 are dedicated to the third. Finally, their Chapter 10 focuses on our fourth goal, and chapter 4 is dedicated to our number five. The importance of these goals is equally apparent in other texts of the period, for example Quirk and Saposnik (1968) or Takayama (1974). It is also quite clear from the only history of modern general equilibrium theory, Ingrao and Israel (1990), where the last three chapters of their book are dedicated respectively to our goals one, two and three.

defined by

$E = \{p^* \mid Z_i(p^*)=0 \text{ and } p_i^*>0 \text{ for all } i\}$  is nonempty.

ii) The second theoretical issue is uniqueness. Is the equilibrium price vector  $p^*$  unique (i.e. does there exist only one such vector)? Since excess demand functions are homogeneous of degree zero (H), if any nonnormalized price vector  $p^*$  is an equilibrium price vector, then  $\lambda p^*$  is also an equilibrium price vector for all  $\lambda > 0$ . When prices are normalized then uniqueness literally means uniqueness, i.e. the set  $E$  contains only one element.

iii) The third goal is the stability of the equilibrium price vector. The intuition behind the concept of stability is that when the system is not in equilibrium, prices should adjust on the basis of the "law of supply and demand" (Gale, 1955); prices should increase when the quantity demanded is greater than the quantity supplied, and prices should decrease when the quantity supplied is greater than the quantity demanded. There are a number of different ways to formalize the idea of a price adjustment process based on the "law of supply and demand", but the standard approach has been the so-called Walrasian tâtonnement adjustment mechanism. The simplest version of this mechanism is given by the following system of ordinary differential equations:

$$dp_i/dt = Z_i[p(t)] \quad \text{for all } i. \quad (T)$$

Following Samuelson (1941, 1942a, and 1944), the equilibrium price vector  $p^*$  is said to be locally stable if the price path  $p(t)$  generated by the system of differential equations (T) converges to  $p^*$  for any initial price vector within some positive  $\lambda$  of  $p^*$ . Thus  $p^*$  is locally stable if,

$\lim_{t \rightarrow \infty} p(t) = p^*$  for all  $p$  within some

Alternatively, the equilibrium price vector  $p^*$  is globally stable if the price path  $p(t)$  generated by (T) converges to  $p^*$  for any initial price vector. Early contributions to the literature on the global stability were Arrow and Hurwicz (1959) and Arrow, Block, and Hurwicz (1959); the most influential survey of the early stability literature was Negishi (1962).<sup>15</sup>

iv) The fourth theoretical goal is general equilibrium comparative statics by means of the "Correspondence Principle." In his 1942 stability paper Samuelson coined the term the "Correspondence Principle" for the relationship between the dynamic stability (in his case, local stability) of an economic model and the comparative statics properties of the model. During the 1960s and 1970s a number of attempts were made to apply the correspondence principle to the Walrasian general equilibrium model. The class of results that were desired would have shown that determinate comparative statics were always implied by the stability of a general equilibrium system. Unlike the cases of existence, uniqueness and stability, there was far less professional agreement about what exactly would be a reasonable thing to expect from the successful application of the correspondence principle. Signs of all possible comparative statics results? Some signs? Own effects but not cross effects? Signs for specific special cases?

v) The fifth theoretical issue concerns welfare economics: welfare economics in the sense that many economists were searching for an economic theory that would provide the ability to pass normative judgment upon various policy proposals and evaluate the desirability of various possible configurations of economic institutions. Demonstrating that a competitive market economy is

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<sup>15</sup> Weintraub (1991) gives a detailed and context-sensitive history of the literature on the stability of general equilibrium.

"optimal" or "efficient" (or the related project of showing why it might not be optimal or efficient) had clearly been a long-standing goal of Walrasian general equilibrium theory (and neoclassical economics more generally). But the question of how precisely this should be done -- that is, what exactly should be the proper approach to questions of optimality and efficiency -- is a topic where there has been much less consensus. If one takes a very weak optimality notion like Pareto optimality, then one can prove some general theorems about the desirability of competitive institutional arrangements. On the other hand, if one takes a stronger notion of optimality, say one involving the gain or loss in consumer's or producers surplus, then one may be able to make more specific policy recommendations. Neoclassical economists have traditionally desired welfare results, but there has been some ambiguity regarding the exact properties those results should have.

In addition to these five goals we would also like to consider one of the guiding heuristics of Walrasian general equilibrium theory (and all of neoclassical economics). This guiding heuristic is clearly not one that the program's participants have openly acknowledged (or one they would uniformly agree to if it were pointed out to them); it is the accommodation of the energy metaphor found in classical physics. In Mirowski (1989) and related work, one of the authors of this paper has mounted a systematic and sustained argument regarding one, and perhaps the most important, of the intellectual origins of and guiding heuristics for what eventually became neoclassical economics. It is the appropriation by various economists of the mathematics and structure of the energy concept from mid-nineteenth century physics.<sup>16</sup>

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<sup>16</sup> De Marchi (1993) and the March 1992 issue of Philosophy of the Social Sciences contains a number of critical commentaries on Mirowski (1989). The author's responses are given in Mirowski (1992) and (1993).



The "marginalist revolution" can be understood as little more than the replacement of "energy" in the mathematical framework of energy physics with the "utility" of neoclassical economics. This appropriation of the energy metaphor not only shaped neoclassicism from the very beginning, but contrary to those who insist upon the irrelevance of origins, it has continued to play an active role in what is and is not accepted as legitimate neoclassical theorizing ever since. We hereby claim that accommodating the original energy metaphor was an important theme, or implicit guiding principle, of neoclassical theorizing in the 1930s and 1940s as well as later general equilibrium theory.

We will call the simultaneous achievement of all five of these goals plus satisfaction of the physics heuristic The Neoclassical Dream. This is not to suggest that these six things were the only theoretical goals of Walrasian general equilibrium theory -- indeed, the reason we are engaged in writing a history as opposed to a "rational reconstruction" is because there clearly were other identifiable goals -- and it is most certainly not to say that "theoretical" goals (these or others) were the only factors that determined the kind of theoretical discourse the profession ultimately produced. There existed simultaneously goals and interests of other types, as well as various constraints and impediments, at work in the determination of the various theoretical strategies that ultimately came to dominate the discourse of general equilibrium theorists. But in recounting the interests which appear to govern a scientific community at a given historical nexus, one must start somewhere. In the weighting of goals and interests we find ourselves in much the same predicament as were the actors themselves: by merely bringing objectives to light we are attempting to enroll allies, block opponents and redirect the existing research program. Constitution of goals and evaluation of findings interact and change through time, depending upon the locations of the protagonists (Pickering, 1995a); this is

one of the primary reasons why a serious historiography cannot presume an unchanging entity like "neoclassical price theory."

We will demonstrate in the next section that a general equilibrium theory based on the rejected knowledge of Hotelling's demand functions would have fulfilled all five goals of the Neoclassical Dream by following Hotelling's heuristic. The remaining sections of the paper will carefully examine the history of American neoclassical economics in the 1930s/40s in order to address the question of why it was, given its potential fecundity, that Hotelling's approach did not become the standard view of demand. But before turning to these issues, let us briefly set the coordinates of our time machine for a quick trip back to the present, in order to check just how well the program that managed to become the High Church orthodox view -- namely, Arrow-Debreu general equilibrium theory -- succeeded in achieving these five goals. (Some comments on the Low Church sects will be postponed to the end of the paper.)

Our time machine lands us truly at an impasse: The short answer is that the standard theory did not succeed very well at all. Out of the five recognized goals, only the first, existence, has an unequivocally satisfactory solution. The main existence result of Arrow and Debreu (1954) has stood the test of time and continues to be accepted as a definitive answer to the existence question; this one goal has actually been achieved. Unfortunately this is not the case for the other four of the stated goals. Uniqueness is not a general result; it holds in a number of restrictive special cases, like gross substitutes or a dominant diagonal on the aggregate excess demand Jacobian, but there are no general results. As Ingrao and Israel say in their history of general equilibrium theory; "It is quite clear that uniqueness theorems can only be obtained on assumptions so restrictive as to appear unacceptable" (1990, p. 360). Similar things can be said about stability, where the results are "unquestionably negative" (ibid, p. 361). The

counterexamples in the papers by Gale (1965) and Scarf (1960) initiated a flood of similar counterexamples, and more recently the Sonnenschein-Mantel-Debreu (SMD) results on aggregate excess demand functions have made it even easier to produce examples of Walrasian economies that lack stability.<sup>17</sup> Stability, like uniqueness, is only available for a few, quite restrictive, special cases and the desired general results have not been achieved. So too with the Correspondence Principle, which Arrow and Hahn declared "isn't" (1971, p. 321). The absence of adequate stability results has certainly infected the comparative statics that we are trying to derive from them.<sup>18</sup> Finally there is the issue of welfare economics. There has been some limited success on this fifth goal; the first and second fundamental theorems of welfare economics relating the set of Pareto optimal allocations to the competitive equilibrium (or equilibria) are important results that are displayed prominently in every modern microeconomics textbook. It is also true that "applied" neoclassical economists regularly use cost-benefit analysis to evaluate the economic feasibility of various projects and microeconomic policy changes. The problem is that there is no clear and systematic linkage between the criterion used in these "applied" welfare evaluations and the first and second fundamental theorems of abstract theory. While there has undoubtedly been some success with the goals of welfare economics, even here things are not entirely as the profession would

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<sup>17</sup> The Gale and Scarf counterexamples are discussed in Hands (1984). The SMD results are contained in Debreu (1974), Mantel (1977), McFadden, Mas-Colell, Mantel, and Richter (1974), and Sonnenschein (1972, 1973). Shafer and Sonnenschein (1982) provide a survey and some of the implications are discussed in Kirman (1992).

<sup>18</sup> "In my view, the correspondence principle was always a nonstarter. It was motivated by the right reasons and it was in some sense natural, but it never had a chance of bearing much fruit for the simple reason that economic theory when the principle was first stated, just as now, has no accepted propositions on disequilibrium dynamics and thus no necessary stability conditions of equilibrium. Less important, it is also true that if our ad hoc dynamic adjustment model is of any order higher than two, the necessary conditions for (local) stability are not powerful enough to deliver the desired comparative statics predictions" (Hahn, 1983, p. 35).

apparently like.

And the "failure" of the physics heuristic is most ironic of all, because it was less a matter of a yawning gap between desire and realization than it was one of failed renunciation. In the 1940s, representatives of the program such as Gerard Debreu and Tjalling Koopmans suggested that the importation of new mathematical tools and techniques had effectively liberated neoclassicism from its earlier acknowledged dependence upon physical metaphors, freeing it to explore the pure economic logic of the Walrasian system.<sup>19</sup> These assertions have led otherwise perceptive historians such as (Ingrao and Israel, 1990) astray in detecting a major shift in inspiration for the neoclassical program in the 1930s. Our narrative suggests a different reading: the Cowles program of Arrow, Debreu, Koopmans and others did indeed set out to explore one possible logical framework which diverged in certain specific ways from the physics framework, the heuristic which stood much closer to Hotelling's model, and which they had rejected. But in the process of exploring the logic of their own preferred variant, they eventually discovered that it was disappointing: as noted above, they only got one-fifth of what they wanted, and worse, Cowles lost faith in its own program of structural econometric estimation. Now, if the Hotelling/physics version would have been more successful in crucial respects than their own innovations, then one implication is: their very attempt to free themselves from

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<sup>19</sup> For instance, in his extremely influential Three Essays, Koopmans noted that, "A utility function of a consumer looks quite similar to a potential function in the theory of gravitation..." (1957, p.176), but this is deemed irrelevant since with the advent of formalist axiomatization, "the absence of any natural meaning of mathematical symbols, other than the meaning given to them by postulate or definition, prevents the associations clinging to words from intruding upon the reasoning process" (1957, pp. 172-3). Likewise, Debreu has admitted that, "theoretical physics had been an inaccessible ideal towards which economic theory sometimes strove...The benefits of that special relationship were large for both fields; but physics did not completely surrender to the embrace of mathematics and to its inherent compulsion towards logical rigor... in these directions, economic theory could not follow the role model offered by physical theory..." (1991, p.1). The claim that this Bourbakist movement had cut itself loose from its physical moorings is critically examined in (Weintraub & Mirowski, 1994).

their origins in the energy metaphor was responsible for the frustration of their other objectives. It was in this curiously inverted sense that the goal of the imitation of energy physics failed.

#### **4. The Dream Fulfilled, If Only Virtually**

In this section we will show how a general equilibrium model with Hotelling's demand functions would fulfill the Neoclassical Dream. Before addressing each of the goals it is useful to derive a few mathematical results for the an economy with Hotelling's demands.

Recall the first order conditions for the entrepreneurial demands from above in (3),

$$\lambda U / \lambda x_i = p_i \quad \text{for all } i=1, 2, \dots, n.$$

The second order conditions for this (unconstrained) maximization problem require the Hessian matrix [HU] with representative element  $U_{ij} = \partial^2 U / \partial x_i \partial x_j$  to be negative definite. This implies that the matrix has principle minors that alternate in sign starting negatively (i.e. each  $i$ th order principle minor has sign  $[-1]^i$ ). Such matrices are often called N-P matrices.

But notice from the first order conditions in (3) that the Hessian matrix of U is the Jacobian matrix of the inverse demand functions  $p(x)$ . We thus have the following result,

$$\partial U / \partial x_i = p_i \text{ for all } i \Rightarrow \partial^2 U / \partial x_i \partial x_j = \partial p_i / \partial x_j \text{ for all } i, j \Rightarrow [HU] = [Jp],$$

which means that [Jp] is also negative definite.

Since the Jacobian of the demand functions  $x(p)$  is the inverse of the Jacobian of the inverse demand functions  $p(x)$ , i.e. since  $[Jp] = [Jx]^{-1}$ , and since a matrix is negative definite if its inverse is negative definite, we have that [Jx] is also a negative definite matrix (and an N-P matrix).

Thus we have that any individual who solves the Hotelling choice problem (2) generates a demand function with a negative definite Jacobian matrix. Since this negative definiteness is preserved under aggregation, the aggregate excess demand Jacobian will be negative definite for a pure exchange economy composed exclusively of traders who solve Hotelling's choice problem (2). If we call a pure exchange economy where the demands of all individuals are generated from Hotelling's choice problem a Hotelling Economy, then the above results can be summarized in the following way,

$$[JZ] \text{ is negative definite for a Hotelling Economy.} \quad (10)$$

Armed with this result let us return to the neoclassical dream.

Very little needs to be said about the first goal of existence. The standard existence results for the Walrasian model should carry over to the Hotelling Economy in a relatively straightforward way.<sup>20</sup>

It is easy to demonstrate that the Hotelling Economy satisfies the second goal; it has a unique equilibrium. Most of the uniqueness results for general equilibrium models involve an application of the Gale and Nikaidô (1965) theorem on the global univalence of mappings. Applied to the case of a

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<sup>20</sup> One potential problem stems from the fact that Hotelling demands are not homogeneous of degree zero (H). A possible solution might be to "price" utility (at say  $r > 0$ ) making the Hotelling objective function,

This objective function has appeared in the "life cycle" literature on labor supply and commodity demands and generates demand functions that are homogeneous of degree zero in the  $n+1$  prices ( $r, p_1, p_2, \dots, p_n$ ). This objective function is called the "consumer's profit function" and it shows "the maximum profit attainable from selling utility (to him or herself) at a price  $r$ , subject to the technology of utility production, i.e., the utility function, and the prices of the inputs" (Browning, Deaton, and Irish, 1985, p. 508). An alternative approach to proving existence for the Hotelling economy would be to exploit the existence results for "compensated equilibria" in Walrasian systems (Arrow and Hahn, 1971, chapter 5; Honkapohja, 1987; Fuchs-Seliger, 1993).

Walrasian general equilibrium model it requires the aggregate excess supply Jacobian matrix  $[-JZ]$  to be a P-matrix (have all positive principal minors).<sup>21</sup> We know from (10) that  $[JZ]$  is negative definite for a Hotelling Economy, which means that  $[JZ]$  it is an N-P matrix, but if  $[JZ]$  is an N-P matrix, then  $[-JZ]$  is a P-matrix and the equilibrium is unique.<sup>22</sup>

The global stability of the Hotelling Economy follows immediately as well. Arrow and Hurwicz (1958, p. 536) prove that if the aggregate excess demand Jacobian matrix is negative definite then the system exhibits global stability of the (unique) general equilibrium price vector  $p^*$ .<sup>23</sup> From (10) a Hotelling economy has such negative definiteness.<sup>24</sup>

<sup>21</sup> See Arrow and Hahn (1971, pp. 208-11).

<sup>22</sup> See Theorem 13 on page 235 of Arrow and Hahn (1971).

<sup>23</sup> The argument is basically as follows. The equilibrium price vector  $p^*$  is globally stable if there exists a real valued Liapunov function  $V(p)$  with the following three properties:

- i)  $V[p(t)] > 0$  for all  $p \notin E$ ,
- ii)  $dV[p(t)]/dt < 0$  for all  $p \notin E$ ,
- iii)  $dV(p^*)/dt = 0$ .

Let the following function be a candidate Liapunov function,

The time derivative of  $V$  is given by,

$$\frac{dV[p(t)]}{dt} = -Z^T(p)Z(p)$$

which, given (T) becomes,

$$\frac{dV[p(t)]}{dt} = -Z^T(p)Z(p)$$

where  $Z$  is the aggregate excess demand vector. This function satisfies (by construction) conditions (i) and (iii) of the definition of a Liapunov function. If the Jacobian  $JZ(p)$  is negative definite then  $dV/dt < 0$  for all  $p \notin E$  and condition (ii) is met as well. Thus, if the aggregate excess demand Jacobian is negative definite everywhere, then the system exhibits global stability.

<sup>24</sup> This result has been anticipated in a number of ways by a number of different authors. An early case is Court (1941) where it was proven that if demand functions (and supply functions in a production model) were based of Hotelling's "entrepreneurial" maximization, then the general equilibrium would exhibit Hicksian perfect stability (local stability as characterized by Hicks 1939).

There are some comparative statics results available for a Hotelling economy. Since the excess demand Jacobian of a Hotelling economy is an N-P matrix the following result holds: "if there is a shift in tastes away from the numeraire commodity to commodity  $i$ , all other excess demand functions being unchanged, the equilibrium value of the  $i$ th price increases" (Quirk and Saposnik, 1968, p. 210). This is certainly not a very strong result, but given the general paucity of such comparative statics information, it represents a significant step beyond what is available for the standard (even stable) Walrasian model.

The implications of a Hotelling Economy for welfare economics are much stronger. As demonstrated in the next section, Hotelling's overarching motivation was the construction of the rigorous foundation for a welfare economics. His symmetry condition (7) guarantees that the welfare line integral defined in (9) measures social welfare. In a world composed exclusively of Hotelling demand functions the concepts of consumer's surplus and dead weight loss that are commonly used in "applied" economics, would be entirely appropriate as measures of social utility from the viewpoint of formal general equilibrium theory. If Hotelling's symmetry condition is added to the standard consumer choice problem -- if we start not with Hotelling's problem, but with the standard problem and add the additional restriction of the

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Another case is Samuelson's discussion of "the stability of a stationary position which is also a maximum" (1942a, pp. 17-8; and 1947, pp. 301-02). This Samuelson case is discussed in detail by Weintraub (1991) and Hands (1994).

Varian (1982, pp. 103-06) proves the result directly (although without reference to Hotelling); first he proves stability for a gradient system, which a Hotelling Economy clearly is, and then he considers a particular special case of an economy where the utility function of all traders is linear in money. Varian's example of utility being linear in money is the same utility function that Samuelson used to generate Hotelling's demands as a special case of standard demand theory (see note 7). The paper by Keenan (1985) draws heavily on Varian's result. The Arrow and Hurwicz (1962) paper in the Hotelling Festschrift volume (Pfouts, 1962) also exploits the fact that Hotelling demands generate a gradient system.



symmetry condition (8) -- the result is a model with "homothetic preferences."<sup>25</sup> As Chipman states, "Thus, within the context of the neoclassical theory of consumer behavior, Hotelling's 'integrability conditions' are equivalent to the assumption that preferences are homothetic" (1982, p. 56).

Finally, it is quite clear that the Hotelling system completes the metaphor of utility as potential energy much more cleanly and much more obviously than the standard approach to neoclassical demand theory. In the standard theory the translation of energy into utility is there but it is submerged, hidden in the Slutsky symmetry condition; the translation is not hidden, nor is it denied in Hotelling. To see this recall the argument for the energy metaphor.

The key to understanding neoclassical economics is to realize that prices constitute a conservative vector field... such that, given a scalar field of utility  $U(x,y,z)$ , the price vector field may be deduced from it. (Mirowski, 1989, p. 223)

In physics, there is no such thing as the deployment of a variational principle without the postulation of a corresponding conservation principle. Neoclassicals cannot have their maximum principles without it. If they leave things where Fisher left them their mathematics says that money and utility are effectively ontologically identical, because they may be extensively added together and conserved in the process. One merely is transformed at determinate ratios into the other, just as potential energy

<sup>25</sup> If  $x^h(p, I)$  is the n-dimensional demand function (standard model) then the Slutsky equation is given by:

$$\frac{\partial x_i^h}{\partial p_j} + S_{ij} = -x_j^h \quad (S)$$

where the Slutsky terms ( $S_{ij}$ ) are symmetric so:  $S_{ij} = S_{ji}$  for all  $i \neq j$ . Now the Slutsky equation and the symmetry condition (8) together imply that,

$$\frac{\partial x_i^h}{\partial p_j} = -x_j^h \quad \frac{\partial x_j^h}{\partial p_i} = -x_i^h$$

i.e. all goods have the same income elasticity. But if all goods have the same income elasticity for a consumer who satisfies the standard budget constraint, then the income elasticities must all be unity. This implies that preferences are homothetic.

becomes kinetic, and vice versa. The fundamental mathematical identity between utility and money income thus provides the logical metric for value in the proto-energetics model. These implications of the model had to be repressed, however, because they contradicted the entire science-based project of grounding the economy in a "natural" principle beyond money. (Mirowski, 1989, p. 231)

First notice that in the Hotelling system prices clearly constitute a conservative vector field; this is an immediate result of the symmetry condition (7). It is also the case that "given a scalar field of utility . . . the price vector field may be deduced from it" (Mirowski, 1989, p. 223). In fact, since the derivatives (gradient) defines a scalar field, the first order condition,  $\partial U/\partial x_i = p_i$  for all  $i$ , not only says that prices can be deduced from such a field, it says that prices are identical to the field.

Also notice that in the Hotelling Economy, in contrast to the standard neoclassical model, the "ontological identity" of money and utility is not repressed; it is entirely explicit. Recall that in Hotelling's objective function: total revenue (utility) and income ( $\sum p_i x_i$ ) are literally added together. Since the symmetry conditions imply that prices  $p(x)$  form a conservative vector field, the Hotelling model is quite literally a case where "money and utility are effectively ontologically identical, because they may be extensively added together and conserved in the process" (ibid.). Hotelling wanted economics to become a science on a par with physics, and what that meant for him was postulation of certain crucial invariants patterned upon similar invariants in physics. These invariants, in turn, would serve to underwrite a legitimate economic empiricism, as well as to promote a situation where political interventions in the system would exhibit certain predictable regularities. That which has been repressed, or at least hidden, in much of neoclassical economics in the interim is worn proudly on the face of the Hotelling model. It clearly fulfills the neoclassical

theory dream.

## 5. Henry and Harold Go Snark Hunting

*Beware the Jabberwock, my son!  
The jaws that bite, the claws that catch!  
Beware the Jubjub bird, and shun  
The frumious Bandersnatch!*

If possible, let us now reactivate once more our time machine in order to escape all these bloodless ideas clashing noiselessly in hyperspace, and return to a dreamtime where a few devoted flesh-and-blood people worked long and hard over their Hollerith calculators to elevate economics to a plane where sensible ideas with clear foundations could be used to guide activity in the world. It is a place where a lone economist of middling reputation and modest means could set out upon a European grand tour and personally consult every economist of moment to his intellectual project.<sup>26</sup> It was a fabled time where, even though the community of mathematical economics was small and dispersed, one committed researcher could seek out another with whom he shared so much common ground that they would spontaneously write letters to

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<sup>26</sup> Here we refer to the diary kept by Henry Schultz of his European trip from 26 August 1933 to 12 August 1934, a copy of which can be found in the archives of the Cowles Foundation, Yale University. Quotes from a typed transcript of this handwritten diary in the Cowles archives will hereafter be cited as [HSDY, page]. The stellar parade of notables with whom Schultz shared dinner and conversation on short notice is enough to make a modern historian swoon: Roy Harrod, Ragnar Frisch, Michael Kalecki, Jacob Marschak, Lionel Robbins, John Hicks, Karl Pearson, R.A. Fisher, F.A. Hayek, Piero Sraffa, Mary Marshall, Joan Robinson, Maynard Keynes, R. Benini, Bernadetto Croce, Francois Divisia, Rene Roy, Etienne Antonelli, Leon Walras' daughter Aline Walras, Maurice Frechet, Emile Borel, Trygve Haavelmo, Gunnar Myrdal, and Eric Lindahl. Only Ludwig Wittgenstein refused to see him. In other words, this was a time when someone like Schultz could be in personal contact with everyone who bore any relevance to his economic concerns.

each other every couple of days for weeks at a stretch about the thorny technical and philosophical issues which absorbed their professional lives.<sup>27</sup> Yet it was also a decade when unemployment was so rampant that it mocked the pretensions to expertise of each and every economist, and incubated ideologies so virulent that belief in rational choice seemed more a leap of faith than any religious commitment.<sup>28</sup>

What was it that drew Henry Schultz, Polish immigrant, and Harold Hotelling, West Coast mathematician, together? Clearly their primary concern was neoclassical price theory, but dominating even that theme was their shared appreciation for an understanding of science which overrode their substantial differences. Both passionately believed that science was transparently mathematical, that it was firmly based upon the collection and statistical processing of empirical data, and that physics was the embodiment of the success of the scientific method. Hotelling, as we shall see shortly, had a very advanced grasp of physical theory, whereas Schultz's appreciation tended more towards the philosophical. In economics, they both adopted as first principles the idea that demand functions, i.e. some sort of price-quantity function, really did exist out there in the world: it was an objective relationship, and all other economic relationships were derivative from it. However, unlike the British Marshallians, they also subscribed to the basic creed that demand functions were

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<sup>27</sup> This refers to the extensive correspondence between Schultz and Hotelling about the issues covered in this paper, stretching from 1932 until Schultz's death in November 1938. This correspondence can be found in the Harold Hotelling papers, Special Collections, Columbia University Library. Citations to letters from this archive will be indicated by [HHC:date].

<sup>28</sup> "Miss Tintner, Coll. Univ. student, related her experiences and those of her group, with matters that are puzzling the youth of today: disillusioned, expecting to find positions, they become 'verbalizers'. They master no field of knowledge, no technique or method of procedure. They begin as skeptics; they end up as tired radicals, or as communists. They take to Communism because it is 'new,' because it offers a promise of a better world. They have their own code of morals. Married couples may have affairs with members of the opposite sex belonging to the same group. But they must not keep it secret!" [HSDY, July 8, p. 60].

obviously interdependent, functionally linked together by rational adjustments to other prices, incomes, as well as various accidents; therefore any simplistic phenomenological price-quantity plot was bound to be misleading. What was sorely needed was a general theory of the constitution of the demand function, the laws behind the law, as it were, in order to sort out the character and extent of the interdependencies; further, only a mathematical theory would do the job. Both turned to the same authors, essentially Walras, Pareto and Edgeworth, to provide the mathematical framework needed to sort through the interdependencies. Yet this set of commitments did not exhaust their common interests. Both started out in the 1920s engaged in extremely pragmatic tasks, estimating demand functions for agricultural staples, in response to the dire problems besetting the American farm community in that decade. Their interest in theory was never predominantly for its own sake, but rather was subordinate to having something convincing to say concerning tariffs, crop subsidies, production quotas, and the like. This background allowed for much greater prior commitment over the relevance and quality of the data than might generally obtain between two such mathematically inclined economists. More unusually, they also shared a very avant garde appreciation for the potential applicability of Fisherian statistical hypothesis testing. Thus, well prior to the crusade of Cowles econometrics, they had glimpsed the potential symbiosis of linear regression analysis and neoclassical demand theory; Schultz (but not Hotelling) was one of the very first to carry out a large scale empirical inquiry informed by both traditions.<sup>29</sup> The extent of their agreement on a wide range of intellectual issues is all the more impressive when one considers that political orientation was not numbered among their shared enthusiasms. Hotelling was a

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<sup>29</sup> Indeed, (Keuzenkamp and Barten, 1995, p.108) trace their brief history of the origins of the "testing" of neoclassical price theory from Schultz's books.

fairly consistent advocate of market socialism throughout his career, whereas Schultz tended to veil his conservative politics behind a stance of technocratic neutrality. Less harmoniously, there was one more experience that they ultimately shared: both were deeply frustrated at the end of the 30s with the way their joint project had panned out.

Much of this may sound so trivial to the reader that he or she may be wondering why it is even necessary to cover this ground. Aren't the prognostications of this duo just another obvious moment in the long sequence of anticipations of modern neoclassical demand theory? Aren't these just minor variations upon a common theme? No, they aren't. Our point is precisely that in America in the 1930s there was no single "theory of demand," no widely shared set of commitments which embodied a consensus neoclassical view of the relationship between the price of good and the quantity of that good that would be sold. The closest thing to what passed for orthodoxy was Marshallian demand theory, which in practice simply denied the importance of the interdependence of demands, worked in terms of separable utility functions, generally appealed to a putatively constant marginal utility of money, and up to that point had encouraged no systematic quantitative empirical research. The two main inspirations of the Schultz/Hotelling inquiry were anti-Marshallian in this specific sense,<sup>30</sup> though historians of economics have not paid much attention to this variation in early neoclassicism. For instance, Henry Ludwell Moore was contemptuous of the Marshallian inability to confront real data; but he also came to believe that Walras and Pareto could not provide the requisite theoretical underpinnings for the demand curves which he never doubted were out there in the real world (Mirowski, 1990). Francis Ysidro Edgeworth, on the

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<sup>30</sup> Though perhaps not in the more general sense in which Hammond (1993) or Hirsch and De Marchi (1990) use the term "Marshallian."

other hand, represented the polar opposite position; he was unshakable in his belief in the palpable reality of individual utility maximization, but thought the direct implication of the theory was that Marshallian demand curves did not, in general, exist (Mirowski, 1994). Thus circa 1930 it was not clear that there was any specific doctrine that qualified one as a neoclassical economist in good standing: demand curves did/didn't exist; demand curves were primary/derived phenomena; individual utility maximization did/didn't exist; mathematical expression was essential/inessential to the theory; demand functions were separable/interdependent; demand theory was restricted to competitive situations or not..... the list goes on and on.

So what was it like to establish oneself as an economist amidst this blooming buzzing confusion?<sup>31</sup> No one seemed particularly paralyzed: they just presumed that everyone else was working on the same rough research project, unless acrimonious controversy convinced them otherwise. If there had been a cavernous split within American economics at this particular juncture, it would have been a split between the Institutionalists and the Marshallians, although even that divide could be easily overstated.<sup>32</sup>

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<sup>31</sup> Sort of like living in the 1990s. Parenthetically, Schultz realized quite early on that Edgeworth was opposed to the existence of demand curves (Schultz, 1928, p.24). This didn't stop him from entertaining Hotelling's desire to use Edgeworth as an entry-point into the questions which concerned them both.

<sup>32</sup> Nevertheless, that is the way that Schultz and Hotelling saw the lineup of the contestants. One can speculate that one of the reasons that Schultz was hired at Chicago was his disaffection from the Institutionalists he had encountered at his alma mater. As he wrote in his (1928, p. 95):

Some economists, among whom are to be included not a few members of the institutionalist school, have, unfortunately, gotten the impression that any attempt to derive a law of demand needs to be based on no better a psychology than that of James Mill. A few of them even go so far as to deny the very existence of a law of demand. What these economists overlook, is the existence of a law of demand is an objective fact, quite independent of one's psychological preconceptions.

Hotelling also did not hold his Institutionalist colleagues at Columbia in high regard. As he wrote in 1962, "I actually did teach economics there, but it was economics so mathematical that no member of the distinguished economics faculty there could understand it" (quoted in Darnell, 1990, p.3). Nonetheless, there is a big difference between the pragmatic intellectual tolerance which characterized the working economist in media res, and the historian's attempt to homogenize everyone into a smoothly progressive narrative after the fact. In the thick of things it is never clear whether some disagreement over "integrability" or the "identification problem" will become central to one's identity as a neoclassical economist, since one's standing in the profession is a function of a wide range of considerations, some of which cannot be readily mapped into abstract doctrinal issues. Only well after the fact does one find a protagonist haplessly pigeonholed into categories and schools that determined his fate, if only in retrospect. Yet it must be stressed that the essence of being a neoclassical economist was exceptionally evanescent in the 1920s and 1930s, and that even restricting ourselves arbitrarily to what became known later as "microeconomics," very little could have been taken for granted. The division of labor between theorists, empiricists and builders of econometric instruments which imposes so much doctrinal uniformity in the modern world had not yet taken hold before World War II.

Henry Schultz's induction into economic theory came at Columbia University under the tutelage of Henry Ludwell Moore, Wesley Clair Mitchell and Edwin Seligman. After a stint in the US Army in WWI, he attended classes at the LSE and Galton Laboratory, where he learned statistics from Arthur Bowley and Karl Pearson. Upon returning to Columbia to pursue his doctorate, he served as statistical expert in an array of governmental agencies, from the Census Bureau to the Department of Labor. His 1925 thesis, written under



Moore, the "Statistical Law of Demand as Illustrated by the Demand for Sugar", was first published in the Journal of Political Economy in 1925, and then expanded into a book-length treatise (1928). Upon the strength of his work on demand, he was appointed to a position in the Economics Department at the University of Chicago in 1926, where he remained until his death in a car accident near San Diego on November 26, 1938. He devoted his entire professional life in his Chicago period to the creation and nurture of his own "laboratory" for the collation of data and the graphing and hand calculation of the least-squares estimates of demand functions for various commodities; it was the innovation of Big Science in economics along the lines pioneered by Mitchell at Columbia. The results of this massive effort were compiled in his book The Theory and Measurement of Demand (1938), which appeared just after his untimely death. As Hotelling reported in his obituary, "He had left Chicago for a semester to teach at UCLA while making a fresh start after the task that had absorbed him for so long. He jestingly remarked after the completion of the book that it was a good time to die" (1939, p. 98).

Since Schultz came to demand estimation somewhat ahead of Hotelling, it will be useful to try and characterize his early orientation towards the project. In both style and substance he openly proclaimed himself a protégé of Moore. In style, he was a master of the relevant literatures in many European languages, as well as maintaining a fair sophistication in mathematical tastes, at least relative to the economic orthodoxy of the day. He also followed Moore in his adherence to Pearsonian statistics and Pearsonian philosophy of science. In substance, he imitated his master in the use of link relatives and trend ratios to pre-process the data and allow for imponderable causes before fitting demand curves. However, quite early on he began to express some reservations about Moore's understanding of the "law of demand." Whereas Moore, having had

his work disparaged by both Marshall and Edgeworth and therefore relatively unwilling to resort to constrained utility maximization to buttress his findings (Mirowski, 1990), resisted most attempts to provide a theoretical grounding for the stability of his curves; Schultz seemed more insistent upon the provision of some underlying rationale for the "laws" of which he was in pursuit. This tendency to look for the laws beneath the laws already shows up in a letter to Moore from December 1924.<sup>33</sup> There he queries Moore as to whether the demand curve derived from the method of link relatives is a static or dynamic law, and what effect the method has on the interpretation of the estimated elasticity of demand. Moore himself did provide an answer, but one which did not make reference either to utility or to the micro-interdependence of markets (Moore 1925; 1926). What is striking about Schultz is that he continued to protest his allegiance to Moore's program till his dying day, but in actual fact started moving towards the Walrasian model and away from Moore as early as 1928.

It may seem incongruous today to think of Walrasian general equilibrium as providing a superior operational framework for empirical microeconomics, but it is important to realize that in the 1920s one had to make some such move in order to extricate one's self from the perceived Marshallian paralysis. Here was how Schultz characterized the horns of the dilemma (NB that he uses "neo-classical" to mean Marshallian):

To derive the ordinary (neo-classical) demand curve as a special case of the general demand function of the mathematical school... is to effect an improvement over the neo-classical approach. The neo-classical economists simply ignored the other variables, without troubling themselves first to introduce them into their demand equation and then to assign them constant values... Though the

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<sup>33</sup> Schultz to Henry Ludwell Moore, 19 December 1924. Henry Ludwell Moore papers, Special Collections, Columbia University Library.

theoretical advantages of the Walras-Pareto over the neo-classical type of demand function are quite apparent, it may be asked whether its very generality renders it useless for practical purposes... How can we deal with such complicated functions in any practical problem? The answer is that, although in theory it is necessary to deal with the demand function in all its complexity in order to show that the price problem is soluble, in practice only a small advantage is gained by considering more than the first few highly associated variables. Which are the most highly associated variables cannot be answered a priori. (Schultz, 1928, pp. 26-7)

If one could summarize the increasing separation between Moore and Schultz over the demand side of the "law," it was that Moore believed that this pragmatic choice of demand variables "external" to own-price and quantity would not gain much in the way of effective guidance from the Walras-Pareto tradition, if only because it's "generality" was empirically intractable on the one hand, and yet hobbled on the other by a litany of patently unrealistic assumptions (perfect competition, static equilibria, no account of nature of disturbances, etc.) (Moore, 1929, p. 53). Schultz, on the contrary, increasingly came to regard Moore's own principles of selection and his various "corrections" to the data as arbitrary, perhaps even to the extent of vitiating his claim to find stable demand elasticities, and therefore sought what he hoped would be a more scientific grounding in Walrasian general equilibrium.

There was nothing acrimonious about this difference of opinion between the two men: indeed, their letters reveal a friendly discussion of the issues; the publications of each honor the other with laudatory citations throughout the mid-to late-20s as they struggle with this conundrum; we have found no evidence that Schultz ever objected to Moore's marked Nietzschean philosophical relativism.<sup>34</sup> Yet the ongoing conversation was abruptly cut short

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<sup>34</sup> Apropos the rhetorical figure that runs throughout our paper, the last section of Moore's final book is entitled, "Economic Dreams." We quote the following passage from it (1929, p. 183):

when Moore suffered something akin to a nervous breakdown in November 1927.<sup>35</sup> His illness forced him to retire from Columbia University in April 1929, just prior to the publication of his Synthetic Economics; he never published anything further up to his death in 1958 (Mirowski, 1990, p. 604). After an extensive search for an empiricist of commensurate reputation, Moore was replaced at Columbia by none other than: Harold Hotelling.

The paths of Hotelling and Schultz had already crossed prior to this point.<sup>36</sup> One of the distinguishing characteristics of Hotelling vis-a-vis Schultz was just how very little background and familiarity he had with economics (Darnell, 1990, p. 3). Hotelling had earned a degree in journalism at the University of Washington in 1919, but after some disappointment with that career, rejoined the University to study mathematics in 1920. He wrote his Master's Thesis on "A Mathematical Theory of Migration" in 1921, and it was at this time apparently, that he conceived the guiding principle of his subsequent work:

The combination of science and political economy led to the thought of applying methods proven so useful in the exact sciences to discover new truth in economics and political science... Ideas of flow and diffusion in mathematical physics looked as if they might

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Our economic dreams and fantasies reveal, in part, our will to believe. Throughout a long past our thought has been directed, unconsciously, toward extenuating and fortifying our dominant desires, and our economic fantasies we have called, pretentiously, economic science. Classical political economy... We recognize it now as a rationalization of middle-class will-to-power and we understand why it waned in interest with the coming of political control of the industrial class... Marxian socialism... is a rationalization of lower-class will-to-power.

<sup>35</sup> Moore to Henry Schultz, 23 April 1928, Moore papers, box 3, Special Collections, Columbia University.

<sup>36</sup> Schultz had already commented upon some work of Hotelling and Working on statistical estimation of trends in (1929). The biographical information on Hotelling in the subsequent section is taken from (Darnell, 1990; Arrow, 1987; Smith, 1978; Pfouts & Leadbetter, 1979).

have applications to human migration and to the flow of commodities.<sup>37</sup>

From there he obtained a scholarship to study mathematics at Princeton, writing his Ph.D. on topology under Oswald Veblen in 1924. He claimed in retrospect, "In going to Princeton I had intended to study mathematical economics and statistics; actually I found there was no one there who knew anything about either subject. I therefore studied the topology, differential geometry, analysis, mathematical physics and astrophysics that Princeton then offered, and all of these have to some extent contributed to my later work." (Ibid.) In this he was fortunate, since Princeton was then in the forefront of upgrading American physics into the world-class status it would enjoy in the 1940s. Jobs for pure mathematicians were scarce, so Hotelling accepted a position at the recently-created Stanford Food Research Institute as a mathematical and statistical consultant in 1924. One of his duties at SRI was to aid in the estimation of demand and supply functions for wheat and other crops; it was this, and his close cooperation with Stanford colleague Holbrook Working, that turned Hotelling's attention to problems of demand theory and estimation.

In 1927 Hotelling was appointed to a full-time position in the Stanford mathematics department, where he remained until his recruitment by the Columbia Economics department in 1931. His publications in the Stanford period reveal an ongoing interest in economic questions, but are scattered over subjects somewhat distant from pure Walrasian demand theory: the entire corpus consists of a 1925 paper applying Hamiltonians to the problem of optimal depreciation, the 1929 paper on the spatial distribution of oligopolists, and the 1931 paper on exhaustible resources. What one can glean from these early

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<sup>37</sup> Excerpt from an unpublished typescript by Hotelling entitled "Western Hotelling and Allied Families on Epic of Migration" in the Hotelling papers. This excerpt is quoted in (Darnell, 1990, p. 4).

efforts is Hotelling's clear admiration for Cournot and Edgeworth as economic theorists. His writing on demand theory is essentially bounded by the 1932 article on Edgeworth's taxation paradox -- the centerpiece of our current narrative -- and some responses to critics in 1939; though publishing 40+ further papers in the next two decades, there were no more substantial contributions to economic theory or empirical economics in their number. The coincidence of this lapse into silence with Schultz's death is striking, although Hotelling's work for the Statistical Research Group during WWII and his move to the University of North Carolina in 1946 should also be taken into account. Hotelling's withdrawal from demand theory has not been the subject of any commentary on the part of previous historians; we shall defer our consideration of this incident till the next section of this paper.

Hotelling himself tells us in the 1932 paper that his concerns about demand theory dated back "several years" in discussions with Holbrook Working; it appears he had also been discussing them with Henry Schultz, as a subset of a larger conversation about the future of statistical theory.<sup>38</sup> Schultz was the editor of the Edgeworth paradox paper for the *JPE*, and his attempts to come to grips with the implications of the Hotelling Economy for his own massive project of estimation of demand curves sparked an extensive correspondence on these and related issues. We shall here weave a commentary upon the published text of Hotelling (1932a) with some discussion of the correspondence, since the latter influenced the final shape of the former.

And what a curious shape it is! From the title and the introduction, one

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<sup>38</sup> See, for instance, the letter from Schultz dated 11 May 1932: "It is good to learn that you have become interested in factor analysis. Several years ago I spent lots of time on it myself... but when Spearman, Wilson and Kelley got tangled up in the psychological-mathematical aspects of the problem, I thought it advisable to step out. I am working, on and off, on another aspect of psycho-physics about which I would like to talk to you..." [HHC: Box 1].

might anticipate a straightforward explication of the conditions in orthodox price theory under which one would encounter the Edgeworth phenomenon of a tax on sellers reducing equilibrium prices; but we are warned right off the bat that, "For a random case, the purely deductive reasoning now available fails to tell us definitively whether it is more probable that a tax will increase or decrease the price paid by buyers" (p. 583). Hotelling sketches the reasons for this impotence of the mathematical theory in the broadest possible terms, but in fact he misrepresents the contents of the Pandora's Box which Edgeworth had pried open. For starters, Edgeworth produced his counterintuitive numerical example in order to undermine the whole idea of a stable demand curve, but Hotelling recasts the problem set by Edgeworth as one of finding out the conditions under which demand curves could rule out the appearance of the "paradox." Furthermore, to ask the question presumes the existence of a stable consensus on neoclassical price theory to serve as a point of departure, which we have argued did not then exist outside of Cambridge UK. To top it off, the theory should then indicate a small number of parameters to be estimated, whose magnitudes would dictate the empirical appearance or absence of the paradox; but Hotelling must have known that situation was remote, since there was no reigning agreement as to the correct estimation of parameters -- Schultz being one of the world's authorities on the attendant practical problems. Heedless to all these objections, Hotelling chose to portray the main impediment to an understanding of the Edgeworth paradox as a theoretical comprehension of interdependent demand functions, or as he put it, "It is easy to fall into the error of supposing that demand functions for several commodities need satisfy no condition except the decrease of demand for each commodity when its price increases" (p. 582). Of course, this relegated two-thirds or more of the contemporary economics profession to the status of heretics; but nevertheless,

Schultz certainly recognized a soulmate in the bearer of these cautionary tidings.

Following hard upon the insistence that the solution to the paradox must be theoretical, Hotelling's paper traverses the most amazing set of "digressions," as he calls them; with one exception, most are ignored in subsequent citations of this paper. Section 4 proposes to derive the general form of the demand and supply curves of a single isolated commodity from the cumulative normal distribution function; but in fact it is an extreme special case for the milk market, with the derivation of the supply curve dependent upon some very specific spatial assumptions about production, and then some rather brazen hand-waving to construct the demand curve in a symmetrical fashion. The most noteworthy aspect of this section is that the curves are not derived from the constrained maximization of utility, a fact then blithely ignored in the next section on "Demand and Supply Functions for Several Commodities." This section contains the "Hotelling Economy" which we have outlined above; it is also the section to which Schultz raised objections in the correspondence. In his letter of 14 July 1932, he asked how one should interpret the sum of prices times quantities in the system, as well as the meaning of Hotelling's insistence that the integrating factor  $\lambda$  would only come into play in the presence of an "absolute limitation" of money expenditures. Hotelling's response that, "Perhaps my tendency to neglect  $\lambda$  is due to the feeling which I have more strongly than Walras, Pareto or Fisher, that the bulk of buying and selling is done under circumstances in which the marginal utility of money does not play a very important part" was not entirely satisfying, since it could easily be confused with the standard position of the Marshallians.<sup>39</sup> Schultz subsequently opted to

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<sup>39</sup> Hotelling to Schultz, 16 July 1932; [HHC, Box 1]. These letters also initiate an inquiry into the impact of inverting a demand function and estimating quantity as a function of prices rather than the inverse. This query led to Hotelling's duality results.



regard this line of defense of the "price potential" model as an assertion of the special case of a constant marginal utility of money (as did Samuelson later), but that was not a faithful rendering of Hotelling's intentions. It is unlikely that Hotelling had in mind a particular thesis regarding the marginal utility of money and then wrote down his equation  $U(x) - \lambda px$  in order to give it expression; rather, he wanted to arrive at a price potential model analogous to that found in mechanics, and saw that this was how it could be done. The payoff was a much sleeker and simpler version of neoclassical theory, one which "excludes both supply curves which slope downward and demand curves which slope upward in the neighborhood of the solution... These results hold not only for each seller but for the aggregate of sellers, analogously in the case of demand" (p. 597). This interpretation is borne out by the remainder of the 1932 paper. The work of legitimation (and the legitimation of "work") came later.

The next section (6) of the paper develops a measure of elasticity of demand for competing versus complementary goods, patterned upon the formula for the classical correlation coefficient. Here again we observe Hotelling straining to construct a quasi-probabilistic foundation for demand theory, acknowledging "those subtle analogies of economics with the theory of probability to which Edgeworth was so fond of alluding" (p. 599).<sup>40</sup> But more significantly, the symmetry of partial derivatives [our equation (8) above] which he dubs "the integrability conditions" are elevated to the status of the core empirical content of his revised theory of demand. "The difference of two

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<sup>40</sup> This practice of Edgeworth of trying to justify utilitarianism on the ground of analogies with the theory of probability is discussed in the introduction to (Mirowski, 1994). What is fascinating about both Edgeworth and Hotelling is their combination of familiarity with formal probability theory and their reluctance to build it into the foundations of price theory. They both finesse the issue by evoking formalisms that "look like" ones found in existing probability theory. Hotelling resembles Edgeworth in that probability can be epistemic for the empirical scientist (hence their work in statistics), but somehow not for the economic actor. One could speculate this may have had something to do with their own prior commitment to the existence of economic laws.

symmetrically placed coefficients could be taken as a measure of the degree of inconsistency in buyer's judgments, or of the rigidity of an absolute limit on their money expenditures" (p. 598). The comparison of "irrationality" with the existence of a strict budget constraint telegraphs Hotelling's unflattering view of what later became the orthodox mode of framing the maximization problem. Section 7 on "Taxation Under Free Competition" makes use of the Hotelling Economy model to extend the Edgeworth paradox to conditions of free competition, so as to argue that the phenomenon has nothing intrinsically to do with monopoly. Instead, the Hotelling Economy permits one to specify the general conditions under which the paradox will be observed, namely, whenever the commodities taxed compete in consumption and in production, with the proviso that one conforms to his own proposed definition of "competing commodities." In this section, Hotelling also tips his hand on the political implications of the theory, suggesting that the welfare consequences of taxation and tariffs in a Hotelling Economy could be substantially beneficial. Section 8 reintroduces monopoly, in order to suggest "cases of competition may exist which are as much as one pleases like monopoly" (p. 608); the unstated implication being that government intervention need not be predicated upon divergences from perfect competition.

The 1932 paper was daring in its ambitions, its structure and its politics. Starting with a "counterintuitive" numerical example, it proceeded to propose a revision of demand theory which preserved some of the neoclassical idiom ("utility," "maximization," "elasticity" and so forth) which gave "laws" of supply and demand that everyone could understand; it also managed to endorse government intervention in the market on individualist welfare grounds; and if the skeptic still demanded a coherent rationale for the strange-looking equation  $U(x) - \sum p_x$ , they would be told that it could account for all existing "anomalies" in

demand theory without recourse to Giffen goods and perverse income effects. But best of all, this was real science in his view, because it displayed the sweet consistency one found in physics, and promised to put an end to endless metaphysical strife. As Hotelling wrote in the same year:

To the doubts whether utility is objective, the reply may be made that demand and supply functions are objective things, and that if utility is defined as an integral or other functional of these functions, it too is objective. In this sense, utility has the same legitimacy as a physical concept such as work or potential, which is the line integral of force, provided certain integrability conditions are satisfied. The weaknesses of discussions of utility which start on a psychological basis are those of treatments of force which start from muscular exertions. (1932b, p. 452)

This was music to Schultz's ears; after all, he was the man who exhorted his students to read Percy Bridgman's operationalist opus The Logic of Modern Physics in their classes at Chicago. Although Hotelling probably did not know it, his model was also a recapitulation of the set of images from energetics which had prompted the "Marginalist Revolution" in the first place (Mirowski, 1989).

From their correspondence, we can see that Schultz immediately set his laboratory to work calculating the demand functions and estimating the partial derivatives to test for the symmetry condition of the Hotelling Economy, as well as the quasi-correlation coefficient to measure complementarity. The letter of 22 September 1932 bore bad news: the symmetry or "integrability" conditions had frequently failed to hold, and worse, sometimes the signs differed, so the complementarity coefficient would give conflicting results when the order of calculation was inverted.<sup>41</sup>

The ensuing correspondence is a wonderful illustration of the ways in

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<sup>41</sup> Henry Schultz to Harold Hotelling, 22 Sept. 1932; [HHC; Box 1]. The letter of 31 October 1932 reiterated the results in greater detail. However, it also reports that, "I have been asked by two of the younger men of our Department to give them an explanatory talk on your paper... It is attracting the attention of the few really serious and competent students."

which scientists scramble to react when things go awry and empirical results apparently bear bad tidings. Philosophers from Popper to Kitcher fail to adequately capture this sense of impending chaos and how scientists of good will struggle to keep it from getting out of hand. The pragmatic routine is to keep the news under wraps until a very small community of researchers decides how to approach, interpret and report the news. In the case of Schultz and Hotelling, the injunction to pause and reconstruct the meaning of their activities was especially imperative, given that their shared credo had been to insist that demand theory was meaningless in the absence of an acknowledgment of the pervasive interdependence of the economy. This sword proved two-edged, because when simple stories of the nature of that interdependence were impugned, as they were in this instance, then a mantra that "everything depends upon everything else" was a prescription for all hell breaking loose. Because everything they believed in was now potentially up for grabs, it was necessary to restrict the discussion to the principals in the interests of reimposing a little order. Here is where the impressive range of prior sympathies between Schultz and Hotelling were crucial: they could more readily jointly assess possible escape routes precisely because they could take so much agreement for granted.

One could always resort to the standard moves for neutralizing the test, and Schultz immediately put these on the table: "An easy way to explain these exceptions is, of course, to assume that the economic behavior of farmers is not always rational -- which may be a true assumption. I prefer, however, one of both of the following hypotheses: (1) the data are inaccurate and/or inadequate; (2) the equations do not contain relevant factors which ought to be taken into account" [HHC: Box 1: 10/31/1932]. However, both were sophisticated enough to know that while these rebuttals might be expected from a mediocre researcher, an imaginative scientist should try and turn contrary evidence into

more elaborate or (even better) new explanations. To that end, at various junctures of their correspondence they tried out all of the following options on each other:

[a] Attempt to link the lack of confirmation to the categories of "completing vs. competing" commodities (we would now say "complements vs. substitutes"). This directly follows up on the 1932 Hotelling paper.

[b] Locate the problem in the functional forms of the demand equations.

[c] Schultz noticed that the residuals of the individual equations were correlated, and wondered if this might signal some flaw in the estimation procedure. In retrospect, some might view this as an anticipation of the Haavelmo simultaneous equations critique.

[d] Perhaps there are other, more intuitive restrictions on the individual demand elasticities (such as their relative magnitudes) which should have been taken into account.

[e] In the Hotelling economy individual demands were supposed to aggregate up to market demands with no problem, but perhaps there was something about aggregation that spoiled the results after all.

[f] There may be macroeconomic problems, such as monetary and price level fluctuations, which supervened upon the outcomes. Schultz had attempted to correct for this by calculating "real" prices. (This was, after all, the Great Depression!)

[g] One less appealing possibility was to give up on the Hotelling Economy and move instead to a model where utility was maximized subject to an independently given budget constraint, as in Walras and Pareto. But then, what would it mean for income or budget to be "constant" in a regime where prices and expenditures were frequently changing? [HHC, Box 1, 1/29/33]

[h] Perhaps the Hotelling symmetry conditions couldn't be tested unless

the demands for all relevant commodities were included. But this way lay empirical perdition.

[i] The meaning of 'integrability' was still contentious, even in physics, so perhaps it could be redefined and reinterpreted.

[j] So perhaps one should not try and test the conditions, but simply impose them a priori upon the demand estimations. The benefit would be that the extra information would render estimation more efficient and make individual demand curves simpler. This was the approach that was taken by most of those who followed Schultz in the field of empirical demand estimation.<sup>42</sup>

One lesson which might be drawn from this discussion is that under conditions of controlled (and not unrestrained Popperian) criticism, scientists will sometimes range widely over a broad spectrum of various conjectural responses to bad news, that this may be a function of the extent to which they can control negotiations, and that a history of events which restricted itself to "theory" or "empiricism" or disciplinary boundaries or "rational reconstructions" might (or more likely would) misconstrue where most of the action is located. Here Schultz and Hotelling ran the gamut: from rejecting the data to rejecting the legitimacy of least squares estimators, from tinkering with functional forms to locating the problem in their individualist grounding, from depending upon mathematical formalism to appealing to intuitions, and from simply imposing the theory a priori to scrapping it in favor of an unsavory (for Hotelling) alternative. Of course, leanings toward one or the other response correlated

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<sup>42</sup> For example after discussing various attempts to test the Slutsky conditions Philips (1974, p. 55) says: "Given that the demand equations have to be specified in some way, a valid testing against unrestricted data is probably impossible. We therefore think that, if we want measurement to be meaningful, we must impose the general restrictions whatever the results of the sort of test just referred to" (emphasis in original).

with prior interests: Hotelling leaned towards theoretical elaboration of the Hotelling Economy, though he might entertain innovation in the theory of statistical estimation; Schultz would be the only one to maintain control of the actual empirical process, and was persistently on the lookout for auxiliary hypotheses in that domain. We can readily imagine a different combination of priors in a counterfactual third party to the negotiations who, say, did not share the commitment to agricultural demand functions that Schultz and Hotelling took as gospel, deeply mistrusted most statistical estimation procedures, and wanted to repress the physics metaphor more actively by denying the importance of integrability conditions -- say, someone like Paul Samuelson.

By February of the next year, Schultz had more or less provisionally settled upon his version of events; after clearing it with Hotelling, it was published as (1933c). Earlier in the year, Schultz had revisited a problem that had worried him since his student days with Moore, namely, whether it was better to estimate demand curves using a time trend in the equation, or reprocessing the data into trend ratios or link relatives (1933a). Now he presented this paper as his first foray into the problem of interdependent demands for 'related goods,' although this also had bothered him for almost a decade. While in many respects the paper is just a summary report of the correspondence with Hotelling, it is interesting to see how he distances himself from the Hotelling Economy, even while using it to structure his empirical program. He begins by deriving what he calls the "integrability condition" from a model with a fixed budget, but with the further specification of a constant marginal utility of money. He then acknowledges,

The conditions... were first deduced by Professor Harold Hotelling, who followed a different procedure... by analogy from mechanics, [where he] made use of the notion of "price potential," whose partial derivatives are the quantities of the various commodities. I

prefer, however, to adopt as my point of departure the fundamental classical definitions of related and independent commodities in terms of utility functions,... (1933c, p. 481).

This may well be the first time someone had referred to the combination of generalized utility function and independent budget constraint as "classical," something we now commonly observe in neoclassical theory textbooks. After describing Hotelling's pseudo-correlation measure of complementarity, he rejects that as well (p. 484). Finally, he reports his regression equations for barley, corn, hay and oats. The major interest in the estimated coefficients is their possible conformity to the "integrability conditions," which are reported in his Table VII (p. 500). The news, as we know, was not good: of the six pairs of comparisons, two had opposite signs, three more were not significantly different from zero, and the barley/hay relation suggested that the commodity demands were independent, so that integrability conditions were unnecessary. The long final section of the paper is taken up with a list of auxiliary hypotheses which are intended to explain away what is plainly presented as a disconfirming result. It is of interest to note that at no time in the text does Schultz entertain the notion that it is the underlying neoclassical theory -- that is, the constrained maximization of an independently given utility function -- which should be rejected; this stands in contrast to the correspondence, where he admits this should be entertained "only as a last resort" [HHC, Box 1, 2/2/33]. Instead, he presents seven options in the paper, only to repudiate most of them: (1) the order of consumption is relevant; (2) the marginal utility of money is not constant; (3) the demand curve is shifting about through time; (4) some related goods were left out of the estimation procedure; (5) the demand curves for separate commodities actually refer to different populations of consumers; (6) data quality is poor; (7) something about the estimation procedure cannot account for the fact that elasticities turn out to have different magnitudes when one inverts



the demand curves and regresses price on quantities rather than vice versa.

This is of course a shining example of what philosophers of science would call the Duhem-Quine (or underdetermination) problem; rather than reject the theory, the negative implications of contrary evidence are aimed at the auxiliary hypotheses in the original test system. Schultz's response to this problem -- a problem that is often called the problem of "where to aim the arrow of modus tollens" -- seemed to be not to actually aim the arrow at all, but rather to fire a shotgun blast quite blindly into the set of auxiliary hypotheses.

Although Schultz's report would hardly seem encouraging for Hotelling's project, the two men remained in close contact, still worrying away at the meaning and implications of integrability. There was a personal visit in June, and a brace of letters in July, and another visit in August as Schultz prepared to embark upon his year-long tour of Europe. This persistence is all the more remarkable given that political differences also threatened to flare up.<sup>43</sup> Schultz's travels were salutary from the vantage point of learning what others were doing about the foundations of demand theory and problems of statistical estimation of interdependent functions,<sup>44</sup> but it did tend to put the damper on

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<sup>43</sup> See the letter of 3 August 1933:

Your stock in Chicago has gone up by leaps and bounds. As you probably know, we do not think much of the so-called economists and statisticians who are connected with the N.I.R.A administration. We have very good reason to believe they are economic ignoramuses, and that, well-meaning as they are, their work is likely to lead to another crash... When my colleagues would 'kid' me about the statisticians and economists who are prominent in the councils of the NIRA, my effective reply was: Are they economists? Are they statisticians? But when the news appeared that you and Roos were going to join their staff, I had no effective reply. Your letter, however, shows me that you are still the clear thinker you have always been, that you are not being taken in by shibboleths, and that your heart is in the right place. I am impatient to show your letter to Frank Knight as soon as he gets through with his class in the next few minutes.

<sup>44</sup> The diaries provide a useful window into Schultz's thinking right after the first round of intense negotiations with Hotelling. They reveal, for instance, that Schultz was not all that enamored with the Hicks/Allen approach, which he discussed with them personally. It also helps offset the tendency of modern historians to dismiss Schultz as some sort of prematurely obsolete thinker. For example, Mary Morgan (1990, p. 156) asserts that econometric work on

the correspondence and the empirical work, as Schultz lost almost the entire staff of his laboratory during his absence. Hotelling took advantage of the hiatus, however, to compose his own response to Schultz's research report, which appeared as "Demand Functions with Limited Budgets" (1935). This is the next critical juncture in the history of neoclassical demand theory, since up to this point neither Schultz nor Hotelling were aware of Slutsky's 1915 paper, and it is at this nexus that a third character, Milton Friedman, makes a critical appearance.

Hotelling (1935) is a curious paper, and one ignored by just about everyone. It indicates on page one that it is intended as an extension of the 1932 paper, but also that it takes off from Schultz (1933c). One striking thing about it is how Hotelling completely suppresses the negative empirical findings.<sup>45</sup> But that is just a prelude, for the paper is unremittingly theoretical; indeed, lacking even the spare level of "economic content" found in the preceding paper of 1932. It seems that Hotelling decided that he would present his Hotelling Economy at a higher plane of rigor, and then produce a parallel model of constrained maximization of utility subject to independent budget constraint (a model Schultz had prematurely called "classical") at the same level of rigor; the object

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demand had improved so rapidly in the 1920s and 30s that Schultz's work "was already outdated in some respects by 1938." Elsewhere (p. 188) she accuses Schultz of having ignored Tinbergen and Frisch's approach to identification. Such statements are called into question by the following diary entry:

Frisch has a wonderful command of technique and almost inexhaustible energy. Would that he had better statistical and economic judgment!... The work in dynamic economics is proceeding at a rapid pace. The papers by Tinbergen, Frisch and Kalecki are notable achievements. There is need, however, for a better synthesis of newer procedures with those of Walras and Pareto. I should like to see individual demand and supply functions introduced into the equations. [HSDY, Oct 2, p.6]

<sup>45</sup> "Schultz's results as a whole seem to confirm the applicability to these commodities of the integrability conditions. It must be remembered that even though a single discrepancy may be judged significant when it exceeds double its standard error, still, among six, it is quite probable that one will fall beyond this limit" (Hotelling, 1935, pp. 66-7, note 1). So much for shared statistical methodology giving discipline to interpretation!

would appear to be to compare the two and demonstrate the palpable superiority of Hotelling's version. But things didn't quite work out as planned: the integrability conditions for the "classical" model were a mess as he conceived them, and the only restrictions which could be derived from them for demand functions were some inequality constraints (equation 5.1, p. 76) which could not even be aggregated up with any dependability to market-level demands.<sup>46</sup> The paper ends there quite abruptly, perhaps because the implications of the exercise remained obscure even to the author. Certainly someone who had been sent to weather Schultz's absence "under [Hotelling's] protective wings" [HHC, Box 1, 11/9/33] thought so.

Given subsequent developments in the history of neoclassical demand theory, we think one of the great ironies of this narrative is that the first mention of the Slutsky equations among our protagonists was by Milton Friedman.<sup>47</sup> Friedman was the other major link between Schultz and Hotelling. Beginning

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<sup>46</sup> This may be the appropriate point to warn the reader about one of the very few authors who have commented upon this paper, namely, Paul Samuelson. (He is hardly a disinterested party, as we shall suggest in the next section.) In his essay "Harold Hotelling as a Mathematical Economist," Samuelson deploys his talent for rewriting history without being held to any particular historiographic standards. For instance, he praises the 1932 paper without telling the reader about how the Hotelling Economy differs from other neoclassical treatments. He hints, in his inimitable manner, that the integrability conditions were "related to Legendre transformations and Gibbsian thermodynamic potentials" to take them off the scent of the original energy gradients. But the major legerdemain comes when he moves on to discuss the 1935 paper: "To complete his 1932 program of taking into account demand generated by utility maximized subject to an income constraint [our emphasis], Hotelling in 1935 tackled the mathematics of this problem" (1966b, p. 1590). One has to go to his footnotes to catch the whiff of something slightly off: "Perhaps the root of Hotelling's difficulty is that his writings are never fully Walrasian... As he was aware, a rigorous interpretation of his results was possible in the special case where for everyone the marginal utility of income was strictly constant" (1966b, p. 1595). The "marginal utility of income" interpretation was Schultz's, not Hotelling's; and as for who is the Walrasian in good standing, see the next section.

<sup>47</sup> The relationship of Milton Friedman's profound about-face with regard to the centrality of the Slutsky relations and the goal of the empirical recovery of utility functions to the rise of the "Chicago School" will be discussed in the next section. We believe that an understanding of this incident sheds light on his seemingly idiosyncratic promotion of a "Marshallian" over a "Walrasian" approach to neoclassical theory (Hirsch and De Marchi, 1990, chap. 1), as well as help in decoding his confusing essay on the methodology of positive economics.

as a graduate student under Schultz at Chicago, he worked with Hotelling in 1933/4, only then to return to Chicago in the fall of 1934 to re-establish Schultz's statistical laboratory upon his return from Europe (Schultz, 1938, p. xi). Schultz himself is a little coy on exactly when he "discovered" the 1915 Slutsky paper, hinting at one place (1938, p. 38fn) that he knew about it in 1933, and in another (1938, p. 620) in 1934; but it is most likely it was brought to his attention during his European tour. However, there is no question about when Hotelling was informed of its relevance. We reproduce most of the body of a letter from Friedman to Hotelling, written from Chicago, dated 6 March 1935:

I have been reading your article in the last issue of *Econometrica* and was interested in applying your condition (3.9) to those pairs of commodities studied by Schultz in his inter-relations paper, which did not satisfy the original integrability conditions. In each case condition (3.9) is satisfied. You may be interested to know that Slutsky in his paper in *Giornale degli Economisti* for 1915 developed valid integrability conditions under the assumption that income is fixed. He proceeded by getting expressions for the derivatives of the quantities with respect to income as well as to price. The basic result he obtains is (translated into your symbols):

$$\frac{\partial q_i}{\partial p_j} = - \frac{M_{ij}}{M}$$

where  $M$  is the determinant of the  $\frac{\partial q_i}{\partial p_j}$ 's bordered by the  $p$ 's. Since  $M$  is symmetrical,  $M_{ij}$  is equal to  $M_{ji}$  and we can write the integrability condition:

$$\frac{\partial^2 q_i}{\partial p_j \partial p_k} = \frac{\partial^2 q_j}{\partial p_i \partial p_k}$$

This condition reduces to the simple one, of course, when the marginal utility of money is kept constant. This paper by Slutsky is really most remarkable. He clearly recognizes the difficulty presented by the non-measurability of utility and indeed presents a proof that it is impossible to obtain the second derivatives of the utility function from empirical data. His paper contains practically everything that is in the Hicks-Allen paper recently published and much more besides.

One thing that has been bothering me is the question of the conditions under which the Slutsky integrability condition will hold for the aggregate demand if it holds for the individual

demand curves. [HHC, Box 1, Friedman to Hotelling]

Schultz, at least, thought this was answer to his prayers, namely the clarification of the general theory of interdependence of demands. After a long hiatus, he wrote Hotelling on 6 May 1935:

I had intended to write to you about my discovery of Slutsky's paper... but Milton Friedman anticipated me. The paper is remarkable for completing Pareto's work on demand and supply and for giving the Hicks-Allen results in a simpler and more understandable form. The discovery of this paper necessitated my rewriting a section of the first chapter of my forthcoming book which I thought had reached its final stage some three years ago. Believing that you would be interested in my summary of the Pareto-Slutsky results, I am enclosing my revised chapter as well as a translation of Slutsky's paper. [HHC, Box 1, 5/6/35]

Hotelling's response of 17 May to this letter is revealing; it is also the last bit of correspondence between the two we have been able to find on the topic. He provides comments on what would become chapter 1 of (Schultz, 1938) and he also reasserts the relevance of the inequalities derived in his 1935 paper, which, by the way, never gets cited in the final text of Schultz's book The Theory and Measurement of Demand. Then comes the crucial paragraph on Slutsky:

Slutsky's paper is a valuable find. You have performed a useful service by discovering it. I wish that I had known of it and of the work of W. E. Johnson and of Hicks and Allen in order to refer to them in my *Econometrica* article. Fortunately for me, all these writers narrowly missed finding my main result, the inequality (3.10). They also missed the integrability condition (1.5) on demand functions. But to my surprise I find that Slutsky discovered some relations which I did not. These include particularly his equations involving variation of demand with respect to income (i.e. the partial derivatives of quantities with respect to income), and the cyclic relation in the middle of p. 24 of the manuscript, which is the dual of my relation (1.5) in that prices are interchanged with quantities. This latter relation is of much interest, inasmuch as it implies the existence of a price potential, a function of the prices whose derivatives are proportional to the quantities. A complete duality between prices and quantities is thus established. In view of this, I suspect the later sections of my

recent article might be rewritten and improved. [HHC, Box 1, 5/17/35]

So there we have it: forgotten precursors are unearthed, the scales fall from everyone's eyes, all players converge upon an interpretation of events we recognize in retrospect as our own orthodoxy, empirical puzzles are clarified, testing and criticism finally lead to the truth. But wait -- not so fast. Even in the above passage, there remains a note of discord: Hotelling is still asserting the primacy of a price potential, even in the Slutsky model. But we have argued in previous sections of this paper this is precisely what one must relinquish when one moves from the Hotelling Economy to the Arrow-Debreu simulacrum. Did Hotelling in fact repudiate his own prior model? Or was it repudiated for him by someone else?

The next development in the sequence of events was Schultz's (1935) JPE paper, written with the assistance of Milton Friedman. The objectives of the article were to promulgate the Slutsky conditions as the culmination of the Paretian tradition in price theory, linking changes in income to those in price and demand; but also to provide a comparative "test" of the Slutsky conditions vis-a-vis the earlier Hotelling integrability conditions. It is clear from the text of this article that Schultz believed that here was the potential (no pun intended) explanation for the poor showing of his earlier empirical foray; he now asserted "they rest[ed] on weak foundations" (p. 457), so now Hotelling conditions should only be treated as a special case. Of course, "generality" is often lodged in the eye of the beholder -- and as Schultz himself had admitted in the previous decade, too much generality might not have such salutary effects on a theory which aimed to say something of use -- so he proposed to drive the point home by an empirical comparison of the Hotelling and Slutsky symmetry conditions in newly-estimated demand curves for beef, pork and mutton in the US during the

period 1922-33.

Yet once more, things did not turn out as he had hoped. Many of the standard errors were too large, and in the case of pork vs. mutton, the estimated partials had opposite signs. Worst of all, the Slutsky "correction" didn't seem to correct much of anything from the viewpoint of the theory. Schultz's prognosis was restrained: "the two conditions are satisfied only approximately, the more general Slutsky condition, which is free from the assumption of the measurability of utility and the constancy of the final utility of money, yielding approximately the same results as the simple Hotelling condition" (p. 477). It seems possible to date the suppression of the Hotelling economy on purely conceptual grounds from this point. The ironies of this impasse were apparently not savored by the duo. Hotelling, who all along had restricted his search for a more scientific demand theory to theoretical considerations, had found their empirical consequences largely intractable; whereas Schultz, the champion of an empirical operationalism, found himself downplaying empirical results in the interest of theoretical generality as he perceived it. One reasonable interpretation of Schultz's results would have been that Slutsky was not an empirical improvement upon the Hotelling Economy; and worse, all this concern over the interdependent character of demand was going nowhere, since none of the restrictions derived from those formally-derived quantitative considerations were apparently supported by the empirical estimations. These were pointedly not the interpretations broached in Schultz's conclusions, who opted instead for the involuted comment that "so apparently simple a concept as independent consumptions turns out to be a complex and elusive notion" (p. 481).

How about Hotelling? Did he renounce his Neoclassical Dream? Not by a long shot. Because the correspondence ceases here, we must depend upon

the last published statements he made on price theory, namely a short popular piece (1936), his 1937 Presidential Address to the Econometric Society (1938a), and his review of Schultz's final book (1938b). A summary synthesis of his position in the late 30s would need to incorporate all of the following points (although it would be possible to dispute Hotelling's weighting of their relative importance): First, demand curves are still treated as the primary economic phenomenon. Second, the interconnectedness of demands is treated as a fact, and by implication, the Walras-Pareto tradition is the right way to try and get at the nature of the phenomenon. Third, Hotelling showed no inclination to challenge Schultz's findings by actually going out and estimating some demand functions himself, although he continued to insist that the integration of theoretical and empirical studies was the hallmark of successful science. Thus, the dispute would be prosecuted on the theoretical plane, if at all. To our knowledge, he only mentioned the poor showing of Schultz's tests once, avoiding any admission that they may have impugned his own model (1939, p. 100). Fourth, henceforward he would always cite Slutsky as an interesting elaboration of the question of the symmetries of multiple demand functions, without actually endorsing it one way or the other as the sole correct approach. Fifth, he continued to cite his own symmetry conditions as valid, at one point writing, "I have shown that there is good reason to expect these integrability conditions to be satisfied, at least to a close approximation, in an extensive class of cases" (1938a, p. 247). Sixth, he continued to advocate an approach to welfare economics that required demand curves to satisfy his (not Slutsky's) symmetry conditions; he followed the previous quote on integrability conditions with: "If they are satisfied, the surpluses arising from different commodities, and also the surpluses belonging to different persons, may be added to give a meaningful measure of social value" (1938a, p. 247). And finally, directly related to his



welfare economics and perhaps of over-riding significance, he proceeded to make use of the interdependence of demands in neoclassical theory in order to prosecute his own political agenda and to propose his own analysis of the causes of the Great Depression.

Perhaps it is no accident that at the point at which Hotelling decided to become more insistent about the political content of the theory he favored, the pipeline to Chicago and the correspondence were both shut off. Hotelling continued to provide employment opportunities for Chicago products such as Milton Friedman and George Stigler during the war, in the Statistical Research Group at Columbia (Stigler, 1988, p.61), but the bloom seemed to be off the rose, and their later works such as Friedman's (1942) paper with Wallis on indifference curves and his paper on the income tax (1952) were intended as reprimands of Hotelling's program. What Hotelling did in his last economics papers was use both the Slutsky version of neoclassical theory and his own Hotelling Economy to argue that government intervention at the micro level was eminently justified by these theories. Sometimes he might use the Slutsky version, as when he argued for the superiority of an income tax over excise taxes; but just as frequently he would use the Hotelling Economy in order to point to a "meaningful measure of social value" to make welfare arguments. His policy prescriptions, never entirely spelled out, seemed to imply a kind of quasi-nationalization of production which would "put into effect a system of sales at marginal cost, with overhead paid out of taxes on incomes, inheritances, and the site values of land, [then] there would exist a possible system of compensations and collections such that everyone would be better off than before" (1938a, pp. 257-58).

It is here that reactions to the Great Depression and general economic conditions have a looming presence in understanding subsequent attitudes and

responses to the appropriate elaboration and development of neoclassical price theory. In a Hotelling Economy, incomes are not broken out as a separate invariant term but conflated with utility in one over-arching conservation principle: therefore, Hotelling did not believe that aggregate shortfalls of income/expenditure were the root cause of the Depression; rather it had to do the impact of technological development upon the number of industries facing falling marginal costs (1938a). Hence, for Hotelling, the solution involved microeconomic manipulation of the price system by the government under the guidance of his Dream-based welfare economics. In contrast, Schultz felt that existing theory said nothing about the causes of the Depression. This was acknowledged by Hotelling in his eulogy for Schultz: "His results do not, for example, offer an explanation of business cycles. He was aware of this, and preferred to study static theory, rather than joining the great procession into monetary and business-cycle theory" (1939, p. 99). But there was nothing sacrosanct about this interpretation of the Slutsky version of price theory, either. Since income was broken out as a separate magnitude in the Slutsky equation, one could equally maintain that Slutsky-based demand functions were the ideal scientific microfoundations for an income-driven Keynesian macroeconomics, an interpretation which gained two different sets of adherents in Cambridge, Mass. and at the Cowles Commission. These schools simply opted to ignore the warnings of Hotelling and Schultz that the Slutsky version did not seem to have nice properties under aggregation. And then there were those who wanted to pronounce a pox on all of the above because of their not-so-hidden socialist tendencies, suppress the interactive income terms, and return to a "Marshallian" mode of theorizing in the sense that Schultz had disparagingly used the term in 1928. This position, of course, is exemplified by Frank Knight, Milton Friedman and the Chicago School from the late 40s onwards.

One of the most common narratives in the literature of economic methodology is a saga where mathematical savants take various imprecise intuitions in applied sciences, render their implications more clear and transparent in the idiom of formalized theory, and then pass them on to statistical specialists in empirical work, who test the mettle of the tradition with quantifiable decision rules. At first glance, the collaboration of Schultz and Hotelling would seem a prime historical candidate exemplifying this "method" (Christ, 1985; Morgan, 1990; Mosak, 1987) but our sojourn in the Field of Dreams should insinuate substantial doubts about the validity of such a tale. Schultz, of course, always made ceremonious appeal to this construal of his project: "The equations summarizing the interrelations in question constitute a category of laws which is comparatively rare in the social sciences: they specify quantitatively definite relations which must exist between the variables -- if the theory is true" (1938, p. 646). Yet, as we have seen, nothing was ever quite so clean as this image of bold precise conjecture and definitive attempted refutation. What happened instead was some polite sparring over the conceptual legitimacy and exact implications of the mathematical restatement of the theory which were never completely reconciled, and some statistical exercises which were delphic, at best, in their endorsement of any versions of the theory being entertained. Worse, this was the best that the duo could produce after extensive pre-publication negotiations over any number of auxiliary hypotheses, negotiations carried out under the most favorable circumstances one could ever hope for: out of the limelight, between two fundamentally friendly parties, sharing a commitment to essentially the same research program. When all was said and done, the only excuse that Schultz could come up with after laboring so mightily to bring forth his pathetic mouse was to appeal to a view of science which, if broached in the 1990s, would be referred to with disdain as

"social constructivist":

The foregoing summary strongly suggests that the success of a new method or idea depends on the social and intellectual milieu in which it is launched. At best, statistical research is difficult, expensive, time-consuming, and not so productive of doctoral dissertations as is historical or literary research. A student may spend several years in an effort to determine the factors affecting the demand for or the supply of a commodity and have little to show for his work, while the same time and effort spent on antiquarian research would almost certainly have yielded material for one or more publications. Unless, therefore, conditions are exceptionally favorable to statistical research in economics, it will fail to take root and proliferate. (1938, p. 660)

Although this is not the way the subsequent history is usually presented (with mathematical neoclassical economists and econometricians fighting a brave uphill battle against all the formidable social forces arrayed against them) because we are favorably inclined towards SSK, we shall entertain it briefly. Although it is certainly true that the risks of Big Science are palpably greater than the cottage handicraft science of days of yore, it is not at all obvious that the reason something like Schultz's vision of price theory eventually came to dominate economic orthodoxy was technological progress in empirical economics leading to a fall in the costs of computation and data compilation, to the point where they were comparable to what he calls "antiquarian research." The best way to telegraph this point is to temporarily cast our gaze forward to subsequent "tests" of the Slutsky symmetry relations in the period after Schultz and up to the present.

Let us ask a potentially embarrassing question: If the Slutsky relations did not pan out so nicely for Schultz in the 1930s, when did they finally become empirically respectable, so the doctrine could occupy its contemporary price of place enshrined in modern textbooks? The answer is: it never happened. You would never glean this from those very same textbooks, nor indeed from most

surveys of neoclassical empiricism; instead, one has to look for this admission in out of the way places, like some explicitly methodological writings by econometricians (Gilbert, 1991; Keuzenkamp 1994; Keuzenkamp & Barten, 1995) or the few comprehensive surveys of "consumer behavior" (Deaton & Muelbauer, 1980). (One might also observe that the economists admitting this are almost exclusively Europeans, whereas the active stabilization of neoclassical price theory was primarily an American phenomenon, as described in the next section.) It seems that, even in the face of the infinite number of compromising considerations which bedevil estimation of a system of demand equations, the Slutsky symmetry conditions and the related condition of zero degree homogeneity in prices are persistently rejected in statistical tests.<sup>48</sup> The relative silence which greets this unmistakably disappointing outcome is deafening; but more to the point, the excuses which are tendered a half-century after Schultz, after massive investments of time and effort about which he could only dream, show very few signs of improved imagination or innovation:

We do not believe that, at this stage, it is necessary to abandon the axioms of choice in the face of the results of this chapter. Ultimately, of course, given sufficiently convincing evidence, we

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<sup>48</sup> [As methodologists] "we have to be able to explain why the rejection of additivity [of utility] was accepted by the profession while the rejection of symmetry and homogeneity, implied by the same studies, were apparently ignored. Equally we need to explain why Blundell et al (1989) were prepared to ignore the rejection of symmetry on their micro dataset but at the same time were concerned to reject rank two specifications of the income terms" (Gilbert, 1991, p. 151).

"We have looked at different models, each embodying different approximations, and these have been fitted to different data sets from several countries, but the same conclusions have repeatedly emerged. Demand functions fitted to aggregate time series data are not homogeneous and probably not symmetric . . . But these models produce a conflict with the theory. The restrictions of homogeneity and symmetry, basic to the assumptions of a linear budget constraint and the axioms of choice, are consistently rejected by the data" [our emphasis] (Deaton & Muelbauer, 1980, pp. 78, 80).

should be prepared to do so. But, for reasons given in this last section, it is clear that there are many more obvious misspecifications that should be corrected first. (Deaton & Muelbauer, 1980, p. 82)

We would like to signal to methodologists that our narrative of the development of neoclassical demand theory is making progressively less reference to notions of hypothesis testing and rigorously formulated theory as we move forward in time. That was certainly the case for Hotelling and Schultz, at any rate. Hotelling, as we have observed, essentially dropped out of the neoclassical theory community by 1943. There is some evidence that he did so out of disappointment in the direction that the economics profession had taken. In a 1959 letter to Carl Shoup, he wrote:

I have had the impression that few people ever read my 1932 paper and it is good to know that you, at least, think it worth taking seriously. Each of my two articles in the Journal of Political Economy, the 1931 article on mineral economics and the 1932 to which you allude, represented about a quarter of my time for several years and it is a pleasure to know that the work was not in vain. (quoted in Darnell, 1990, p. 23).

As for the other member of our duo, there are some indications that Schultz was not held in high esteem by his colleagues towards the end of his tenure at Chicago (Stigler, 1988, pp. 25-7; Reder, 1982; Houthakker, 1983, p. 60, note 7; Friedman in Hammond, 1993, p. 222). Frank Knight's contempt for Schultz was now openly expressed; and what is of greater moment for our narrative, there is some indication that Schultz's favored student may have begun to turn on him: "Two of the students not favorably impressed by Schultz were Friedman and Stigler" (Reder, 1982, p. 4). This hostility is apparently the origin of numerous slurs cast upon Schultz's mathematical abilities in the secondary literature (Reder, 1982, p. 4; Stigler, 1988), which seems to be overstated. Even Samuelson engages in this disparagement, while revealing

that one of the primary inputs to the Foundations was his own attempt to provide a resolution to the impasse precipitated by Schultz. "Already in 1936, on a trip back to Chicago, I had unsuccessfully tried to interest Henry Schultz in Jacobian conditions for rigorous aggregation (functional dependencies of separability). Also, I spent countless hours pursuing testable regularities that aggregate demand data must theoretically obey" (Samuelson, 1986, p. 223). This rush to press an indictment of crippling mathematical deficiencies against the man who brought the issue of the scientific pretensions of neoclassical theory to a head would seem, at least to an historian, itself to require some serious explanation. While Schultz clearly treats Hotelling as the superior mathematician in their correspondence, Hotelling treats him in return with unhesitant respect as an equal.

Schultz, as already mentioned, died in a car crash in November 1938; but his The Theory and Measurement of Demand was a sad anticlimax for those who had closely followed his earlier publications. The book was largely a reprint of the two articles discussed above (1933c and 1935), with a few more estimated demand curves to augment the empirical report; but the conclusions were basically the same. "Most of the terms of the calculated conditions agree in sign but differ in absolute value, while the others differ also with respect to sign. Furthermore, the standard errors of the statistical parameters are so large that no conclusion about the integrability conditions of the true, underlying demand functions has a large degree of probability" (1938, p. 600). Thus the neoclassical price theorist at the end of the 30s was confronted with a book that insisted upon the "integrability conditions" of Slutsky as the core empirical content of that theory, but simultaneously reported that said content was either wrong, or so inaccessible to state-of-the-art empirical techniques of the time such that very little could be said one way or the other about the theory.

But something of the encounter with Hotelling had rubbed off, and it had to do with the reasons why one had to keep hammering away at the integrability conditions, even though results to date had been so very disappointing. In the Hotelling Economy, as we argued earlier, the price potential is modeled upon a system of potential forces in mechanics; and Hotelling's "symmetry conditions" were the analogue to the law of the conservation of energy. Schultz understood that he was trying to extend the analogy to the Slutsky symmetries, but that the program had yet to settle upon the definitive invariants.

The third principle [in physics] is that of the conservation of energy and is the most important of all... But what equations of motion and what laws of conservation of comparable scope do we have in economics? To ask the question is to answer it. There are none that have the definiteness and universal applicability of the corresponding physical laws. Our economic laws of change are simply empirical extrapolations of the present situation... True we can write down equations which are analogous to the laws of conservation... we can think of the total utility function-if it exists-as corresponding to the energy potential whose partial derivatives measure the forces which guide the movements of the individual. But, unfortunately, we know neither the values nor the forms of the required functions. (1938, p. 57)

Incongruously, herein lie the origins of how such an unpropitious set of events and circumstances could become within two decades the basis for the core doctrine of a growing, thriving, self-confident school of economic analysis. Two mathematical theories of price were proposed, closely related by their heuristic inspiration drawn from the king of the sciences, physics. They differed by what seemed at first a minor variation: the treatment of income in the optimization problem. Both protagonists recognized that the choice of theoretical variant had something to do with the postulation of conservation principles within their respective constructions of the economy, but there they rested in a state of suspended animation, stymied by the lack of any transcendental criterion with which to convince the other of a demonstrable



superiority. Notions of conceptual generality, empirical success, political efficacy, logical necessity, mathematical tractability, sociological efficacy -- nothing would do the job right; no argument could decisively trump the others. Neither Schultz nor Hotelling could overcome the obstacle, and thus they were removed by Chance or withdrew by Choice from the arena.

It is our thesis that this impasse marked the defining moment of the postwar American neoclassical orthodoxy, one whose significance was buried by the subsequent contest between at least three distinct schools to transcend the impasse between the "price potential" model and the Slutsky symmetry interpretation. Most of the major players of the three schools were present at the creation, and two of them -- Milton Friedman and Paul Samuelson -- played supporting roles in the Hotelling/Schultz dialogue. The stinging truth we can see in retrospect, and documented as such in prior sections of this paper, is that the Hotelling Economy could have given each of these schools what they said they wanted, if only they had not gotten so hung up on the interpretation of what they considered to be the "correct" budget constraint. Why was this particular treatment of the budget constraint the itch that absolutely had to be scratched, when neoclassicals of all schools were willing to overlook numerous other bizarre, counterintuitive and outlandish aspects of their models? Was it related to the very notion of "scarcity" which lay dormant at the heart of their image of "economics"? Discussion of this question deserves an entire paper, one which we beg the reader's indulgence to postpone till a future date and setting. For now, we merely wish to indicate how the treatment of the "income effect" in demand theory is the key to the splitting of the American neoclassical orthodoxy into the three distinct schools of the post-WWII period. When Hotelling and Schultz exited the stage, other dreamers felt impelled to dream a future for neoclassical economic science, their dreams pieced together from the

shards of Hotelling's Dream.

## 6. Demand Demended; or, the Dream Defiled

*The Walrus and the Carpenter  
Were walking close at hand  
They wept like anything to see  
Such quantities of sand.  
If seven maids with seven mops  
Swept it for half a year  
Do you suppose' the Walrus said,  
That they could get it clear?'  
I doubt it,' said the Carpenter,  
and shed a bitter tear.*

We offer one last invitation to step into the time machine, now to be transported to the travails of a nation at war. War disrupted the careers of most of the protagonists of the next chapter of our saga; but it did more than that. First, it provided a five-year hiatus right at the point where the death of Schultz and the failure of the Schultz/Hotelling project had helped raise the question: "Whither mathematical economics?" There is some evidence that the crisis was not merely localized around these two individuals: for instance, a case can be made that the Cowles Commission was floundering in the years 1937-38, and perhaps through 1942.<sup>49</sup> The Chicago Economics Department was also in

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<sup>49</sup> The evidence comes from Christ (1952, pp. 17-18). In January 1937, Charles Roos resigned as Research Director to move to the private sector, from which he never returned to academia. Harold Davis took his place for eight months, but then he, too, decamped to the Northwestern mathematics department. Neither individual was especially favorably inclined towards Walrasian neoclassical theory. Ragnar Frisch, Jacob Marshak and Theodore Yntema were recruited as replacements, but all declined to move to Colorado Springs. The upshot was Cowles was bereft of a research director for two long years. In the absence of strong leadership, the unit threatened to devolve into another stock market research organization, still the primary interest of its benefactor. The situation was permanently resolved by the death of Alfred Cowles' father in January 1939, requiring him to move to Chicago. Cowles and his attorney Laird Bell, a trustee of the University of Chicago, negotiated with Robert Hutchins to move the unit to Chicago in September 1939. Christ (1952, p. 20) paints the move as constituting a conscious replacement for Schultz at Chicago, but the circumstances belie this interpretation.

trouble in the early 40s (Reder, 1982, pp. 5-6). Although it seems almost crass to mention such a trivial consideration in the midst of a major worldwide conflagration, the war did provide breathing space to take time out and reconsider the possible options for a future neoclassical theory. But second, the war had a more profound impact upon the next generation of social scientists in terms of the way that they understood "science." While some were recruited to government jobs strictly for their economic expertise, the individuals we are most concerned with were thrown together with physical scientists in some entirely novel situations. Often because of their mathematical expertise they were engaged in research that might normally be the province of engineers or meteorologists or physical statisticians along side physicists who were also temporarily engaged in "applied" work; sometimes this new form of collaboration gave rise to a new hybrid called "operations research."<sup>50</sup> The

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Elements of the Economics Department were hostile to endowing Cowles with University status. Yntema, who was in the Business School, was named Research Director, but seems to have been on leave during much of his brief tenure from 1939-42.

<sup>50</sup> There is a growing consensus in the science studies community that something rather dramatic happened to the social sciences in America in World War II; partly it has to do with such "external" considerations as being ushered into the era of Big Science and governmental funding, but many have argued it extends to the very content of the social sciences, which became much more patterned upon specific aspects of physics and the other natural sciences. See, for instance, (Pickering, 1995a; 1995b; Heims, 1991; Mirowski, 1992; Leonard, 1995; Fortun and Schweber, 1993). We believe that the history of modern neoclassical price theory would be much more closely linked to broader cultural trends if it were situated in this new understanding of "cyborg science."

The wartime activities of the postwar neoclassicals have yet to receive any attention. For instance:

Arrow worked for the Statistical Research Group at Columbia, and then the Weather Division at the US Air Force.

Friedman and Stigler, along with Wald and some early game theorists worked for Statistical Research Group at Columbia under Hotelling.

importance of this phenomenon for our present narrative is that it fostered the enhanced importation of newer notions of science into the economics profession, as well as blurring the conceptual boundaries between persons and things. Given some of the subsequent programmatic statements of the principals, it is all too easy to overlook the fact that in the heat of battle each school brandishing a different reaction to the problems of income effects in neoclassical price theory made explicit reference to how they thought analogous problems were handled in physics, as we shall indicate below.

As our time machine whizzes past bombs bursting in air, it may be prudent to suggest how our narrative of events might differ from more conventional versions. The first, and most obvious, is that where Whig histories see unity of purpose and concepts, we see diversity.<sup>51</sup> At any particular juncture, for rhetorical or tactical purposes, protagonist one could assert that he was discussing the same entity as protagonist two, but there is no rule of thumb that protagonist two's acquiescence was a prerequisite for the discipline to maintain its integrity. The second difference is that many

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Samuelson worked at the MIT Radiation Lab, the same unit which housed Philip Morse's Operations Research unit.

"By 1942, something like half the staff at Cowles Commission had been drawn into work directly or indirectly connected with the war effort" (Christ, 1952, p. 23).

"Kenneth Arrow's two Nobel-status contributions to economics were financed by the Office of Naval Research and the RAND Corporation." (Samuelson, 1986, p. 867).

<sup>51</sup> Consider the following, quite telling, example. Reder's (1982) article provides valuable documentation of the oral tradition about the history of the Chicago school, but as with so many other texts in economics, it misses the diversity within neoclassical economics. It is hard to believe that anyone who had read the texts from the 1940s and 50s could assert that with regard to "'pure theory' -- I do not consider there are any generic differences between Chicago and non-Chicago theorists" (p. 11). This seems clearly to be a projection of experience of the 1970s/80s.

methodologists often write as though some protagonist first adopts a methodological stance or set of principles, and only after that fashions a theory or empirical exercise to conform with those epistemological beliefs. Short of having all your epistemological doubts and fears settled in the womb or in grade school, we find it difficult to believe that the process of research activity is so rigidly unidirectional. Indeed, we suspect that much of the methodological warfare, backpedaling and inconsistency of Friedman, Samuelson and others is as much an artifact of their initial commitments in the arena of price theory as it may be of their philosophical "influences". In other words, we think it as likely that research schools coalesce around certain doctrinal theses -- say, the treatment of the budget constraint and the Slutsky symmetry conditions -- as around some abstract methodological ukase about "testing" or "rationality" or "science." Third, we find that our narrative inverts a whole range of conventional wisdom about how economics ended up the way it did. For example, in our view, it was Keynesian doctrine which acted in a critical way to determine the shape of neoclassical microeconomics in America, rather than (initially) the other way round. Or, again, it was neoclassical price theory which shaped the initial orthodox configuration of econometrics, especially in its treatment of error terms, rather than the other way round. Or, once more, Chicago did not reject the Slutsky-tinted version of general equilibrium theory in the 1940s because they were somehow lagging in appreciation for its cogency, perhaps due to a deficit in mathematical sophistication; they rejected it after being responsible for its introduction into the American context. Things are indeed not what they often seem.

As we have repeatedly intimated in this text, we believe there were three major reactions to the breakdown of the Schultz/Hotelling program, each characterized primarily by its approach to the budget constraint and the

treatment of the "integrability" or symmetry conditions, precisely because that is where our duo had located the problem. There were, of course, yet other options; but we will make no attempt to deal with them here.<sup>52</sup> There was also a further development which muddied the waters and obscured the sharp outlines of these three schools, namely, von Neumann's axiomatization of "expected utility theory" which raised the stakes of the challenge as to the appropriate location of stochastic considerations within the theory of consumer choice. Again, we must beg the reader's indulgence for not dealing with this issue here, even though it is of utmost importance for a comprehensive history of postwar neoclassical price theory. Instead, the reader should understand we are only dealing with purely deterministic consumer choice theory, in artificial isolation from all considerations of production, uncertainty, knowledge, strategic behavior as in game theory, questions of less than perfect competition, and the like.

With those caveats, let us briefly fly over the three main schools precipitating out on the American landscape in the immediate postwar period: the Cowles Commission approach, which incorporates Arrow-Debreu general equilibrium theory as a later development; the Chicago approach of Frank Knight and Milton Friedman; and the approach of Paul Samuelson, which became regnant at MIT. Each of these three schools could be compared along a number of different dimensions, such as their conception of methodological individualism, their politics, their favored mathematical formalism, their vision of empiricism, and so on; but we wish to make the strong assertion that many of their earmarks or traits can be readily organized and rendered comprehensible through their characteristic treatment of the symmetry conditions and budget

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<sup>52</sup> Griffith Evans, Nicholas Georgescu-Roegen, the Cassel version of disavowing utility altogether in favor of demand functions -- to name just a few.

constraint in neoclassical price theory. While it would take prodigious documentation and a fair amount of explication not found in price theory textbooks to elevate such seemingly obscure doctrines to pride of place in the topography of neoclassical economics, we opt here to simply point in the directions of the history which needs to be written. We will start with the Cowles view of neoclassicism.

The origins of the Cowles school can be traced to the appointment of Jacob Marschak as Research Director in 1943, and can be divided into two periods which more or less correspond to the directorships of Marschak (1943-48) and Tjalling Koopmans (1948-54). In the first sub-period, Cowles was engaged in the development of structural estimation econometrics, a story which has been told in a number of different places.<sup>53</sup> Because Marschak has not received the attention lavished by historians upon the other Cowles luminaries, it is not widely recognized that he took as one of his immediate research tasks the clearing up of the debacle left by Schultz: namely, estimation of restrictions upon demand equations dictated by the Slutsky version of neoclassical theory. Contrary to the impression given in Morgan (1990), in this sense wartime Cowles was the direct descendant of Henry Schultz. Marschak had already made one pass at the problem of conceptualizing the budget constraint in his (1939); upon arrival at Cowles he published his (1943), which was an attempt to test price homogeneity and the Slutsky symmetry for the demand for meat. As we are now growing accustomed, things did not turn out favorably for this version of neoclassical price theory.<sup>54</sup> Marschak's response was to push research into two

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<sup>53</sup> For the econometrics see Morgan (1990); Christ (1952;1994); Epstein (1987); for the change in regime see Weintraub & Mirowski (1994).

<sup>54</sup> Keuzenkamp & Barten (1995) identify this as the first attempt to statistically test the zero degree homogeneity condition of demand theory. This paper also illustrates our contention that philosophy takes its cue from price theory as much as vice versa. Apparently, Karl Popper claimed that this paper by Marschak inspired his own "zero method" (Popper, 1961, p. 141).

directions: into Keynesian theory, which he believed sanctioned the possibility of price level changes having real effects, and into Haavelmo's insight that there might be something fundamentally biased about statistical techniques constructed to estimate single equations in a situation where the appropriate specification involved simultaneous systems of equations.

Thus Cowles embarked upon the econometrics program for which it is most fondly remembered. The original impetus for the fascination with identification problems, Neyman-Pearson hypothesis testing, simultaneous equations estimation bias, and the rest was the a priori conviction of the interdependence of all economic phenomena through the Walrasian/Slutsky lens, again a tribute to Schultz. Marschak had a good sense of who might share this prior orientation, and from his experience in New York recruited Haavelmo, Koopmans, Lawrence Klein and Kenneth Arrow, among others. And yet, in the words of one historian of the field, "Econometrics started to become a secondary interest of the Cowles staff as the 1940s ended" (Epstein, 1987, p.110). This decline in econometric focus occurred for a number of different reasons; the primary one being not that it had worked, but rather, in the judgment of many members of the Cowles staff, it had failed its original promise. The new-fangled simultaneous estimation techniques rarely gave coefficient estimates much different from simpler OLS techniques; and worse, the track record of predictions from the models were fairly abysmal. For all of Koopmans' attacks on the bumbling research devoid of theory at the NBER, actual quantitative restrictions implied by neoclassical price theory rarely held in the empirical exercises undertaken at Cowles. And in addition, the Cowles staff had to put up with Milton Friedman's attendance at their seminars, training his formidable debating skills and withering skepticism upon their enthusiasms for Keynes,



structural econometrics, and Walrasian price theory.<sup>55</sup>

The second sub-period of Cowles neoclassicism began with Koopmans' accession to the research directorship, but is best exemplified by the work on general equilibrium theory by Kenneth Arrow and Gerard Debreu (Weintraub & Mirowski, 1994). Since we have already run through this model above, perhaps we can simply point to the ways in which the treatment of the budget constraint helped constitute the school. The hallmark of this school is an admission that neoclassical price theory may have been prompted by physical analogies in the past, but that the modern task was to extract the "abstract structure" of Walrasian system and subject it to the most rigorous axiomatic scrutiny; in this way they managed an acknowledgment that the imposition of the budget constraint had caused the model to diverge from the Hotelling Economy.<sup>56</sup> This abstract structure took the postulate of an independently fixed budget constraint for the individual agent as inviolate, and then sought to elaborate the logical consequences for a system of completely interdependent demand functions. However, the Slutsky symmetry conditions were obscured by the choice to rephrase the data of "preferences" in more abstract set theoretic terms, eschewing any reliance upon the differential calculus. This, in turn, had substantial implications for the way that a "dynamics" of price adjustment would need to be conceptualized (Weintraub, 1991; Hands, 1994). Nevertheless, one can discern the skeleton of the Slutsky conditions buried within the system, as we have

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<sup>55</sup> "By 1944, a fairly intense struggle was underway between Knight and his former students on one side, and the Cowles Commission and its adherents on the other... It continued for almost 10 years" (Reder, 1982, p. 10).

<sup>56</sup> See footnote 19 above. The point to be made here is that, if neoclassical price theory had been lifted intact from simple energy physics, there would be no need to axiomatize it, since it would, in effect, have already been done. Again we would like to stress that an alternative narrative like (Ingrao & Israel, 1990) is written from within the self-image of the Arrow-Debreu program, and therefore cannot encompass the actual historical diversity of neoclassical price theory.

explained above. Unfortunately, axiomatic elaboration also eventually led to the conclusion that the system placed no effective restrictions upon excess demand functions, thus subverting the original impetus for the research program.<sup>57</sup>

It would round out a much more ambitious narrative to note how much of Hotelling's Dream is still sustained in Arrow's conception of this program. Although to our knowledge he only promoted the actual model in one paper (Arrow and Hurwicz, 1962), Hotelling's protégé has kept alive in spirit many of his original motives.<sup>58</sup> For instance, like Hotelling, Arrow has never accepted that the fundamental justification of the neoclassical program could be grounded in empiricism *per se*. "But then the question arises... After all, why bother with theory? Go out and estimate your demand functions... The reason we do rely on theory is because we feel that these empirical estimates are not very good" (Arrow in Feiwel, 1987, p. 205). Note that in Arrow's words "theory" becomes conflated with one variant of neoclassical theory, while statistical endeavor is relegated to a definite subordinate role. In another vein, Arrow has for much of his career promoted a politics of market intervention very similar in style to that of Hotelling. Further, Arrow has tended to express the "relevance" of abstract general equilibrium through an attempt to construct and prosecute a "new welfare economics"; the divergence from Hotelling comes from the fact that his treatment of the budget constraint blocks access to Hotelling's simpler welfare theory where value is interpersonally comparable.

Before leaving Cowles, it may help cement our case that there exist three

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<sup>57</sup> See note 17.

<sup>58</sup> Arrow has dedicated his Collected Papers to Hotelling in ringing terms: "who encouraged my entrance into the field of economics, bolstered my limited self-confidence, and set the example of human concern combined with analytical rigor that I have always attempted to follow" (1983, p. i).

substantially distinct schools of neoclassicism to observe that Arrow has differentiated himself from our other protagonists. While one of the most courteous and open-minded of economists of his generation, he has been known on occasion to venture a minor rebuke to Samuelson and Chicago. For instance, "Samuelson has been less systematic than Walras or Hicks in his pursuit of GET" (in Feiwel, 1987, p. 204). Or, in a defense of general equilibrium theory,

[I]t allows you to keep in mind a sense of what the economy is all about. As far as I can see, nobody within the neoclassical tradition can deny the validity of GET. For example, no matter how much the Chicago school tends to stress the methodological advantages of the partial equilibrium approach, they cannot deny that what they are talking about is embedded in a GE world. Their only defense is that by limiting their scope they can do more practical things (in Feiwel, 1987, p. 201).

But let us move to Chicago, the second main school of neoclassicism, and decide whether the sources of difference are as simple as a mere preference for partial over general equilibrium exercises. While the school sports many luminaries, we shall restrict ourselves here to three texts which outlined the explicit response to Schultz: Friedman & Wallis (1942), Knight (1944), and Friedman (1949).<sup>59</sup> The secondary literature on Friedman has grown to enormous proportions; and some of it we have found quite helpful (e.g., Hirsch and De Marchi, 1990; Reder, 1982); but it would seem that even the most useful misses the major clue to Friedman's career, namely, he began by writing up part of Schultz's Theory and Measurement book, spent some time with Hotelling, and then spent the next decade or more criticizing, repudiating and perhaps even obscuring their entire project. Thus the private ironies must have been bittersweet when E. B. Wilson wrote Friedman in November 1946 asking for "a

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<sup>59</sup> We are obviously limiting ourselves to elements of what might be called 1st and 2nd generation Chicago, and neglecting entirely the messy issue of the 3rd generation of Lucas et al.

list of half a dozen jobs in economics which you think thoroughly good."

Friedman replied in December:

Another book I am somewhat uncertain about is Henry Schultz, The Theory and Measurement of Demand. It is an exceedingly careful and systematic attempt to put empirical content into a pre-existing theory. I have excluded it because there seems to me no reverse influence of the empirical work on the theoretical structure. Schultz took the theory as fixed and given, and tried to measure what he thought were essential functions in the theory. He imposed extremely high standards of care and thoroughness in the measurement process-- but he nowhere attempted what seems to me to be the fundamentally important task of reformulating the theory so it would really generalize the observable data; he always tried to wrench the data into a pre-existing theoretical scheme, no matter how much of a wrench was required. (in Stigler, 1994, p. 1200)

If you look at this as if it were an abstract epistemological manifesto, as so many methodologists try to do, it looks puzzling, confused, inconsistent, or worse (Hirsch and De Marchi, 1990, ch. 1). But that is because Friedman without Schultz and Hotelling is like Hamlet without Rosencrantz and Guildenstern. In effect, Friedman's reaction to Schultz's impasse was to first, repudiate the attempt to extract laws of utility from price data (Friedman and Wallis, 1942); next, extricate price theory from any binding commitment to the Slutsky relations (Knight, 1944), and as the piece de resistance, deny the practical importance of income effects for demand theory. Now, this definitely was not the same move as suppressing the independent budget constraint, as Hotelling had done. Rather, Friedman packaged his program as returning to Marshall -- but not the actual historical personage of Alfred Marshall -- no, the return was to the "Marshallians" that Schultz had declared as his sworn enemies. These were the folks who could ignore interdependent utility and demand functions, lay down tear gas whenever they wanted to make some "long run" arguments, reaching for their immunizing stratagems in a "classical" theory of money, and

cluck their tongues with steely disapproval whenever someone wanted to raise some fine points about mathematical inference. This was all done in the name of a neoclassical economics that "works." Income effects were not denied outright; they were just treated as uninteresting second order effects whose general influence were inherently unknowable.

Was this bland nostalgia for simpler days of yore? While it certainly was "conservative," it was also substantially much more than that. For example, Knight (1944) provides an extensive critique of the physics metaphor which underpins the Walrasian system: "There is no direct analogy with equilibrium between objects stationary in a field of force. The true physical analogy would require an elaborate construction hardly undertaken so far in the literature" (p. 309). But there was also a subtle economic point, namely, there had been no good reason given for presuming that income could remain fixed during a virtual movement along a demand curve: "the negative price elasticity of demand for a good cannot be dissociated from declining positive income elasticity, all prices being constant and with the consumer free to add new items" (p. 297).<sup>60</sup> In Knight's mind this mistaken notion that one could separate out income effects was the first step down the primrose path to Keynesianism: "the general point of view and habit of mind reflected in the Hicks-Slutsky analysis has wide ramifications in recent literature and has led to utter confusion in the whole body of economic thought. We refer, of course, to the huge corpus of discussion beginning with Keynes' General Theory" (p. 300, note 10). Knight and his students read what they considered to be misunderstandings about

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<sup>60</sup> One of the authors thinks Knight was right on both counts, and tried to make both arguments in (Mirowski, 1989) prior to any familiarity with this article. However, where he might diverge from Knight is on the lessons one should draw from these observations. Knight believed that one must relinquish Walrasian-style neoclassical theory, but keep the demand curves. Another equally valid conclusion is that one should jettison both constructs.

income to actually be surreptitious statements about the nature of money, which reinforced their convictions about the validity of classical monetary theory. And when the Keynesians weren't trying to turn income effects into a kind of macroeconomics, then they were concocting some sort of illegitimate welfare arguments out of a price theory supposedly free of personal biases (Friedman, 1952).

The upshot of all these objections became clearer through the subsequent work of Milton Friedman. The main prescription was to stop searching for the "real laws" underneath the demand curves; rather, one should learn to manipulate the few theoretical entities that you could generally trust. This did not imply jettisoning utility theory tout court. Knight may have maintained that you couldn't legitimately derive a demand curve from a set of indifference curves (1944, p. 301); but Friedman took the line that there were too many different demand curves that might be deduced from the constrained maximization of utility, and that most of them were empirically inaccessible (1962, chap. 2). But when push came to shove, demand functions were real, the alpha and omega of analysis, or as Knight (1944, p. 310) put it, "undoubtedly the most solidly real of all the functional relations dealt with in economic theory". This unswerving commitment to demand functions superimposed upon an acknowledgment of the essential impossibility of grounding them in anything remotely more "fundamental" or lawlike patterned upon physical analogies, combined with the tenuous residual commitment to neoclassical utility theory,<sup>61</sup> explains Friedman's infamous methodological pronouncements on the

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<sup>61</sup> This is consistent with Becker's 1962 paper on "irrationality"; later flirtations with sociobiology; Stigler & Becker "De Gustibus," etc. Why not treat everyone alike and push everything off into the "constraints"? There seems to be no ontological fixity in this school. "Can one say more about the demand functions if they are derived from utility functions?" (Stigler, 1965, p. 147).

unimportance of assumptions, the salience of predictions, and statements like, "The purpose of *ceteris paribus* is methodological not substantive. One really is not asking what will or will not remain constant but rather what principle will be used to select provisionally the things that are assumed to remain constant" (1962, p. 23).

This brand of Marshallianism was an anathema to someone who had observed it up close and firsthand, and agreed with Schultz's fundamental suspicions about it: Paul Samuelson. But Samuelson also seemed to have a residual appreciation for the Chicago skepticism about the vaunted generality of the Walrasian general equilibrium (1947, p. 8), combined with an enthusiasm for the rhetoric operationalism from Schultz. Mix this together with a disdain for statistical demand curves reminiscent of Arrow, and an avid enthusiasm for Keynes, and it would seem we would have very little left in the way of microeconomics to work with. Yet the originality of Samuelson was to use his position at Harvard away from the centers of neoclassicism to combine these disparate elements and innovate a middle ground between the full general equilibrium of Cowles and the pragmatic Marshallianism of Chicago, namely, the theory of "revealed preference."

The twists and turns in the purported goals of "revealed preference theory" from the 1930s to the 1950s have been effectively documented by Wong (1978),<sup>62</sup> so we need not recapitulate them here; rather, we need only add that augmenting his account with the Schultz/Hotelling discussion connects his narrative to the larger history of neoclassical price theory. As we have observed, Schultz was one of the points of departure for the composition of the Foundations. It appears that Samuelson set out to liberate demand functions

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<sup>62</sup> Also see Cohen (1995) for a discussion of Samuelson's operationalism and his theory of revealed preference.

from any dependence upon utility theory, by positing a few simple axioms concerning consumer behavior "up front" which could be (purportedly) validated either by introspection or by direct observation of realized price-quantity pairs. This move was in direct reaction to Schultz, where one instead pursued the empirical implications of the theory at "the back end" of the process, namely, comparisons restrictions upon statistical estimates of the elasticities of the demand curves. Samuelson thus shifted the entire focus of where "the empirical" was to enter into the theory of demand; if preferences were "revealed" then the empirical was already in at the start of the theoretical exercise and thus there wasn't any reason to "test" empirical implications as Schultz had attempted to do. Samuelson could claim operationalist purity -- as Schultz had done before him -- but without all of the messy consequences of actually confronting demand theory with the empirical data. This, of course, explains why Samuelson would loudly trumpet the irrelevance of the integrability conditions throughout this period, injecting more than a little surplus confusion into discussions of the relative importance of the Slutsky conditions ever since. It also accounts for one of Samuelson's most annoying rhetorical habits: denying the remote relevance of physical analogies to neoclassicism, while simultaneously making numerous references to specific physical theories (especially thermodynamics) as exemplars throughout his writings.<sup>63</sup> It appears that Samuelson believed that he had indeed liberated demand theory from a grounding in metaphors of physical force fields; but that he had done so by following the example of modern physicists, whom he thought were coldly

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<sup>63</sup> "My own debts were not primarily to Pareto or Slutsky. Rather they were to the great thermodynamicist Willard Gibbs of Yale... Since I owed a debt to thermodynamics for inspiration on how to deduce the negativity of firm's demand slope, it pleased me to be able to balance the accounts by providing for thermodynamics a rigorous explanation and derivation of what the Le Chatelier principle was all about" (1986, p. 863). We are sure physics was grateful for so magnanimous a gesture.



operationalist and behaviorist in their approach to physical laws. Here it behooves us to recall that Samuelson, unlike so many other postwar neoclassicals, did not have any formal training in any physical science.

As Wong explains, this entire pretense of a rival theory of demand fell apart when Houthakker demonstrated in 1950 that the full content of demand theory required a "strong axiom of revealed preference" which, mirabile dictu, was a restatement of the despised integrability conditions. Although modern textbooks tend to omit this fact, Samuelson himself was forced to concede "in principle there is nothing to choose between the formulations" (Wong, 1978, p. 111); and therefore Samuelson had pioneered a cul de sac rather than an escape route from Schultz, the defective physics metaphor, the unsavory consequences of income effects, Slutsky integrability conditions, and everything they entailed. But by this time, Samuelson had instituted an entire MIT style and school which had come to dominate a broad swath of American graduate education, not to mention many economics journals, which one might call "mid-range theory." Aspects of this style included the neoclassical Keynesian "synthesis," government interventionist politics combined with skepticism regarding welfare economics (particularly welfare economics of the Hotelling sort<sup>64</sup>), frequent resort to representative agent models while maintaining a veneer of multimarket interdependence,<sup>65</sup> a coolness towards econometric innovation,<sup>66</sup> a distance

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<sup>64</sup> "In closing I should like to state my personal opinion that nothing said here in the field of consumer's behavior affects in anyway or touches upon at any point the problem of welfare economics, except in the sense of revealing the confusion in the traditional theory of these distinct subjects" (Samuelson, 1938, p. 71).

<sup>65</sup> It is interesting to note that one place where Samuelson might have significant overlap with the Arrow-Debreu program is on the topic of the use of the Weak Axiom of Revealed Preference in Walrasian systems (initiated by Wald 1951), and yet Samuelson is quite critical of this application. The Weak Axiom applied to aggregate (as opposed to individual) excess demand produces a model which behaves "exactly as if the market consisted of a single rational

maintained from Bourbakist abstraction, the finessing of dynamics with the "correspondence principle," and so on and so on.

## 7. California Dreamin'

*Children yet, the tale to hear  
Eager eye and willing ear  
Lovingly shall nestle near.  
In a Wonderland they lie,  
Dreaming as the days go by  
Dreaming as the summers die.  
Ever drifting down the stream  
Lingering in the golden gleam  
Life, what is it but a dream?*

One thing about dreams: they rarely conform to the rules of narrative, with a clear-cut beginning, middle and end. Our story has no definitive end, because we and our fellow economists are still stuck in the middle of it. This is where the methodologist frequently gets dissatisfied and wants to rewrite the ending, like one of those bad 50s movies where everyone either gets married or goes to jail in the last 5 minutes. They wanted to get demand curves out of Walrasian general equilibrium? Well, the axiomatic method and the Sonnenschein/Mantel/Debreu results show they can't, so hooray for

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consumer" (Dorfman, Samuelson, and Solow, 1958, p. 375, note 2); in other words it behaves like a Hotelling Economy.

<sup>66</sup> "Some of the skepticisms of Knight and Jacob Viner concerning the empirical statistical studies that their colleagues Paul Douglas and Henry Schultz were attempting, I readily admit, were well taken" (Samuelson, 1986, p. 792).

mathematics and good-bye neoclassicism!<sup>67</sup> Or: Who cares about Walrasian general equilibrium now that economists have seen the strategic light of game theory and changed the subject? (Cf. Rizvi, 1994). Or: If only those earlier generations had seen that effective empiricism is really about the construction and "calibration" of small-scale "empirical general equilibrium" models, and not fruitless estimation of demand systems, then we need not have wasted all this effort on a grand detour! Or...

Or what is it that neoclassical economists really believe in? At one time the answer would have been "The Law of Demand." One objective of this history is to demonstrate that particular response really will not wash: not for the present, and not for the past. The reason Hotelling and Schultz set out upon their dialogue was a dissatisfaction with "phenomenological" demand curves and a desire to find the "laws" beneath or behind the Law of Demand. There were other collateral motivations as well, but mostly their concerns came down to specifying the symmetry conditions, the "invariant" of value theory from which one could build up various practical observable results. This quest for the lawlike entity "behind" the prices and quantities is actually an old search, one familiar to historians of economics. Some neoclassicals may deny they have any need for a theory of value, that they are agnostic, but we can see now that is just one more variant reaction to Hotelling's and Schultz's conundrum.

We might close on one more note of irony. It would seem audacious that Paul Samuelson has taken it upon himself to repeatedly criticize Marxian economic theory as having become stranded upon the shoals of the "transformation problem," especially after his acknowledged failure with regard

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<sup>67</sup> This would be our reading of (Ingrao & Israel, 1990), for instance. Parenthetically, we find it a delicious concatenation of ironies that Hugo Sonnenschein is now President of the University of Chicago.

to the original intentions of the program of revealed preference theory. For, after all, what is the difference between the sorry saga of the transformation problem in Marxian economics and the narrative of integrability and Hotelling's Dream which we have just recounted?

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