ON THE CONFIRMATION OF THE LAW OF DEMAND

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Abstract: The paper applies confirmation theory to a famous statement of economics, the law of demand, which says that ceteris paribus, prices and quantities demanded change in opposite directions. Today's economists do not accept the law unless definite restrictions hold, and have shown little interest in deciding whether or not these restrictions were satisfied empirically. However, Hildenbrand (1994) has provided a new derivation of the law of aggregate demand and used this theoretical advance to devise a test that may be the first rigorous one ever performed on the law. The paper accounts for Hildenbrand's and, in less detail, his predecessors' contributions within the philosophical framework of Hempel (1965) and Glymour (1980). Its salient result is that economists have accepted the "consequence condition", and rejected the "converse consequence condition", and thus implicitly selected an exemplification view of confirmation.

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1. Introduction

The present paper deals with one the famous topics of old and modern economics - the *law of demand*. Among the early statements, Marshall's (1890) is the most famous. It simply says that the consumer's demand for a good varies inversely with its price, given that every other price and the consumer's income remain fixed. But consumer theory was rebuilt by Hicks (1939), Samuelson (1947), and their post-war followers (Chipman et alii, 1971), and today's economists do not claim the inverse relation of price and demand in Marshall's straightforward way. As any current text in microeconomics explains, a price change triggers out two separate effects on demand which may go in opposite directions, with the consequence that the final effect is indeterminate. This may explain why, despite its obviously strong practical significance, the law of demand has virtually disappeared from the scene of economics. But there is a recent major exception to this state of affairs. A mathematical economist well-known to the profession, Hildenbrand (1994) has managed to recover an aggregate version of the law from assumptions put on the income distribution. He uses this original derivation in order to carry out an econometric test of the law against consumption data provided by national statistics. By and large, Hildenbrand concludes that his empirical check comforts the time-honoured statement, a conclusion that is the more striking since Hildenbrand also argues that economists have never attempted a serious test before.

This paper has been motivated by Hildenbrand's findings, but given the paucity of philosophy-of-science discussions of the law of demand, I investigate it more generally, and this leads me to explore consumer theory, one of the best structured parts of today's neoclassically inspired microeconomics. In a selective review of this theory; Section 2 will argue that the previous writers of this theory were not deeply concerned with the empirical test of the law of demand. This conclusion essentially vindicates Hildenbrand's surprising claim that the law had not been subjected to a real empirical test before him.

Section 3 moves on to the main case of this paper, i.e., Hildenbrand's contribution. I have selected it despite two shortcomings - it is fairly mathematical and, being still ongoing, not really known outside specialized circles. However, I do not see why philosophy of science should not try to interpret frontier work when it turns out to be revealing of some scientific trend. I take Hildenbrand to illustrate how, when demand theorists devise genuine tests, they think of, and try to resolve, the classic problem of the confirmation of scientific theories. I think Hildenbrand's strategy is representative much more widely in economics, but the present paper will not try to draw lessons beyond the already large reach of consumer theory.

The paper formulates the confirmation problem in terms of Hempel's (1965) tentative requirements for a suitable confirmation concept, prominent among which are the *consequence condition* and the *converse consequence condition*. As Section 4 reminds the reader, these two requirements conflict with each other. Accordingly, it seems natural to taxonomize logical confirmation theories according as they adopt the former or the latter. Hempel's and Glymour's (1980) exemplification theories explicitly claim the consequence condition. While Popper and his followers do not even address the question, I argue in effect that refutationists incline towards the converse consequence condition. In view of these conceptual and doctrinal contrasts, it seems relevant both to confirmation theory and the philosophy of economics to find out which of the conditions effectively prevails when economists test a general statement of their field. Section 5 shows that Hildenbrand implicitly opts for the converse consequence condition, hence for the Hempelian style of confirmation theories. I further argue that, consistently, Hildenbrand rejects the refutationist style of theories; this point needs stressing because he sometimes gives an impression to the contrary. In a final step, I extend the Hempelian account beyond the scope of the initial study, by showing how it applies elsewhere in demand theory. The full potential of the analysis will be explored in another paper.

2. Consumer theory and the law of demand

Consumer theory relies on the virtually unique hypothesis that the individual consumer maximizes the utility of his basket of goods under the constraint set by his money income and the prevailing market prices. This hypothesis entails the following theorem due to Slutsky (1915) and developed by Hicks (1939): a small change in the price of a good *j* every other price and the consumer's income remaining constant, brings about a change in the demand for a good *k* that decomposes additively into a *substitution* and an *income effect*. The first effect measures the change in demand *while making allowance for the change in the consumer's real income induced by the price change*. The second effect measures the residual, i.e., the change in demand resulting from the change in real income. When changes in price and in demand relate to the same good, the Maximization Hypothesis entails that the substitution effect goes in the direction predicted by the law of demand, i.e., price and demand changes have opposite signs. By contrast, the hypothesis never restricts the direction of the income effect. In the case of an own price change, the latter may run counter to the former, and if large enough, defeat the law. For concreteness, think of an increase in the price of bread, with other prices and income remaining constant. The increased cost of bread relative to other commodities would lead the consumer to substitute the latter for the former if his effective wealth - or real income, to be distinguished from his fixed money income - remained the same; however, the consumer is also impoverished by the price increase, and the consequence of this underlying change is prima facie unpredictable. After all, it may be best for the consumer to buy more bread, just because the increased cost of bread makes meat and wine less affordable than they were before. This theoretical possibility is central to modern consumer theory.

I now restate these arguments formally, using differential calculus in the economists' way.[1] Denote by $x = (x_1,..., x_l) \in R^l_+$ the physically available baskets of l goods, by $p = (p_1,..., p_l) \in R^l_{+*}$ the price vector, and by I the consumer's budget.[2] Assuming that he maximizes a utility function $u(x)$ under the budget constraint $p \cdot x = I$, and that this maximization problem has a well-defined and unique solution, one derives a *demand function* $x_j(p, I)$ for each good j. Denote by $(\bar{p}, \bar{I})$ the initial values of prices and income, and consider the mapping: $p \mapsto s(p, \bar{x}) = x(p, p \cdot \bar{x}))$. The value of s represents the basket of goods that the consumer would select at the new price vector p, *if, for these prices, he could afford the initial basket* $\bar{x} = x(\bar{p}, \bar{I}))$. The supposition introduced by "if" constitutes a precise rendering of the vague expression "the consumer's real income is unchanged". The mapping s is called the *Slutsky compensated demand function* because Slutsky (1915) introduced this particular rendering; an alternative one, due to Hicks (1939), will be mentioned shortly. To investigate the effect of a price change p_k under the ceteris paribus clause that $p_l, l \neq k$, and I are constant, one differentiates s partially with respect to p_k, taking into account the budget constraint equation (the latter requires that all the budget be spent). Evaluating the derivatives at $p = \bar{p}$, one obtains the *Slutsky equations*, which hold for all goods j, k, and all initial values $\bar{p}, \bar{I}$:

$$(*) \quad \frac{\partial x_j(\bar{p}, \bar{I})}{\partial p_k} = \frac{\partial s_j(\bar{p}, \bar{I})}{\partial p_k} - \frac{\partial x_j(\bar{p}, \bar{I})}{\partial I} \cdot x_k(\bar{p}, \bar{I}))\ .$$ [3]

Owing to (*), the effect on demand Δx_j of a small change Δp_k can be decomposed as the sum of two elementary effects:

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[1] I only restate what is needed for the purposes of this section and the next ones; for a full exposition, the reader is referred to Mas-Colell, Whinston and Green (1995).

[2] The notation R^l_+ and R^l_{+*} refers to the nonnegative orthant of the *l*-dimensional Euclidean space, with and without 0 included respectively.

[3] I made the innocuous change of notation from $s(\bar{p}, \bar{x})$ to $s(\bar{p}, \bar{I})$.

$$\Delta x_j \approx \frac{\partial x_j(\bar{p},\bar{I})}{\partial p_k}\cdot \Delta p_k = \left[\frac{\partial s_j(\bar{p},\bar{I}))}{\partial p_k}\cdot \Delta p_k\right] + \left[-\frac{\partial x_j(\bar{p},\bar{I})}{\partial I}\cdot x_k(\bar{p},\bar{I})\cdot \Delta p_k\right]$$

The first term is the *substitution effect,* and the second, the *income effect*; I have thus reached the decomposition sketched informally in the previous paragraph. Notice that this additive formula can only hold as an approximation for discrete - as against infinitesimal - changes Δp_k.

There is a more famous, but also more complex way of obtaining the decomposition. Denote by $\bar{v}$ the utility value reached initially, i.e., $\bar{v} = u(\bar{x}) = u(x(\bar{p},\bar{I}))$, and consider the mapping $p \mapsto h(p,\bar{v})$, which by definition associates to p the demand vector $x(p,I)$ leading to the utility value $\bar{v}$ for an appropriately changed I. That is to say, the value of h is the basket of goods that the consumer would select at the new prices if his income were adjusted so as to keep him at his initial utility level. This supposition delivers an alternative rendering of the vague expression, "the consumer's real income remains unchanged", and h is called the *Hicks compensated demand function*. Now, the Maximization Hypothesis entails that the following equations hold for all goods j, k, and all initial values $\bar{p}, \bar{I}$:

$$(**)\quad \frac{\partial x_j(\bar{p},\bar{I})}{\partial p_k} = \frac{\partial h_j(\bar{p},\bar{v})}{\partial p_k} - \frac{\partial x_j(\bar{p},\bar{I})}{\partial I}\cdot x_k(\bar{p},\bar{I}).$$

Comparison with (*) establishes the remarkable result that the Slutsky and Hicks compensated demand functions have identical price derivatives. Accordingly, *for small enough changes* Δp_k, *it does not matter which way the substitution effect is defined*. Henceforth, I follow the standard practice of treating (*) and (**) as if they were the same "Slutsky equations".

These equations can be written in matrix term as $M^D = M^{SE} + M^{IE}$, where (I drop the bars):

$$M^D = \left[\frac{\partial x_j(p,I)}{\partial p_k}\right]_{j,k=1,\dots,l}, M^{SE} = \left[\frac{\partial s_j(p,I)}{\partial p_k}\right]_{j,k=1,\dots,l}, M^{IE} = \left[-\frac{\partial x_j(p,I)}{\partial I}\cdot x_k(p,I)\right]_{j,k=1,\dots,l}.$$

The Maximization Hypothesis entails that the *substitution effect matrix* M^D is negative definite, but does not restrict the *income effect matrix* M^{IE}.[4] If $j = k$, the approximation formula becomes:

[4] A $l \times l$ matrix M is *negative definite* if for all $v \in R^l$, $v' \cdot M \cdot v < 0$, and *negative semi-definite,* if for all $v \in R_*^l$, $v' \cdot M \cdot v \leq 0$ (where v' is v in transposed form).

$$\Delta x_j \approx \left[\frac{\partial s_j(p,I)}{\partial p_j} \cdot \Delta p_j \right] + \left[-\frac{\partial x_j(p,I)}{\partial I} \cdot x_j(p,I) \cdot \Delta p_j \right],$$

and the negative definiteness of M^{SE} implies that the own substitution effect is *negative*, while the own income effect can be of any sign. If the latter is positive (i.e., if $\frac{\partial x_j(p,I)}{\partial I} < 0$) and its magnitude (which also depends on $x_j(p,I) \cdot \Delta p_j$) exceeds the magnitude of the substitution effect, the total effect will be positive. This formally explains why consumer theory does not recover the law of demand, as classically stated by Marshall:

"There is then one general law of demand: -The greater the amount to be sold, the smaller must be the price at which it is offered in order that it may find purchasers; or, in other words, the amount demanded increases with a fall in price, and diminishes with a rise in price" (1890-1920, p. 84) .[5]

The older economists were fully aware that this broad statement needed qualifying. Marshall (ibid., p. 84-85 and 94-95) lists a number of cases where the law fails, regrettably without clarifying the difference between genuine exceptions and failures of the appended ceteris paribus clause.[6] Economists have always known that vintage wines, fancy clothes, or luxurious carriages, might sell better at higher prices, and they have long realized that increasing wages do not always affect the demand for labour negatively. In due course, they observed that rallies on stock markets could attract buyers instead of repelling them. For a long time, they simply put aside the problematic classes of luxuries, factors of production, and speculative buying. Today's theorists take a more systematic attitude, if possible by handling exceptions in terms of strong positive income effects, more commonly however, by amending consumer theory itself. Thus, for *luxuries*, they make the "signalling" assumption that on some range of high values, prices enter the utility function directly. Within the same class, or taken as another group, fashion goods can be accommodated by the other non-standard assumption that preferences are ex post influenced by consumption. As to *speculative buying*, economists enrich the static framework of consumer theory with future periods in which buyers become sellers; only after this modelling change does the Slutsky decomposition become relevant again. That other prominent class, *factors of production*, is more simply discarded as being out of scope. According to the current categorization, consumer theory deals with the demand for final goods

[5] Cournot (1838-1974, p. 85) antidates Marshall by far, but his statement is less detailed.

[6] Here I take Marshall's ceteris paribus clause to be the same as that of today's theory, i.e., to refer to prices and income only. Alternative interpretations have been offered.

alone, and it is incumbent on the theory of the firm to investigate the demand for factors of production.

In sum, the more obvious exceptions to the law are still exceptions for consumer theory. They call for special explanations in which the Slutsky equations have only a subordinate rôle if they come to play at all. One way of assessing what consumer theory adds to the older economics is to find violations of the law where the Slutsky equations apply without too much discrepancy, so that positive income effects would provide the bulk of the explanation. Here is a possible example - the so-called *Giffen goods,* which are defined by the following characteristics: (i) they are *inferior,* i.e., the demand for them varies inversely with income, every price remaining constant; (ii) the consumer has a low income and devotes a large amount of it to these goods; (iii) they have close substitutes. The name for these goods comes from the 19th century English civil servant whom Marshall (1890-1920, p. 109-110) credited for identifying them. Ever since Marshall, Giffen goods have been a matter of dispute: first of all, do they exist at all, or are they just a theoretical possibility? Second, if they exist, do they give rise to an actual failure of the law, or is this failure a theoretical possibility? Marshall seemed to have little doubt that Giffen goods existed but did not clearly say whether upward sloping demand curves had been observed for them. It is part of the folklore of economics that the poor Irish workers in 19th century Midlands would consume *more* bread when the price of bread increased. However, careful investigations shed a doubt on the available evidence; it not even clear how Giffen would have had a chance of recording it. The few related examples in the literature are hardly more telling.[7] This explains why the founders of modern consumer theory, Hicks and Samuelson, take an ambiguous attitude towards the "Giffen case" or "Giffen paradox". Granting the real existence of Giffen goods, if they *had* an upward sloping demand curve, they *would* provide fascinating evidence for the theory; since the mathematical assumptions needed for a strong own positive income effect roughly correspond to (i) and (ii). Unfortunately, it is impossible to go beyond this conditional statement as long as the empirical issue is not sorted out, and given the constraints on consumption data and the heavy ceteris paribus clause, this may well never happen.

There is an alternative way of relating consumer theory to the law of demand that, if consistently pursued, would bring out the specific contribution of this theory. Instead of searching for empirical exceptions outside the established list, economists could conceivably use the Slutsky equations *as a theoretical warrant for the law within the limits set by the list*. Hicks's late treatment (1956, ch. VII) is exemplary for this attitude. He stresses that a

[7] Stigler (1947) dismissed all existing historical evidence. The issue is still debated.

diversified consumer cannot spend much of his income on any inferior good j in particular, which means that the positive income effect of such a good cannot be large either. For concreteness, Hicks supposes that the consumer spends a 5% fraction of I on sugar and the price of this commodity is halved. The income difference that compensates - in the sense (*) - for the real income increase is thus equal 2,5% of I, which means that the income effect cannot exceed the effect on the demand for sugar of a 2,5% rise in I. Such a small rise must be spread out on all goods, which leads Hicks to conclude: "we are justified in saying that the income-effect is normally very small" (1956, p. 65). This is how Hicks reaches the conclusion that, if the Slutsky equations hold, the law of demand is "in practice likely to hold" (1956, p. 66). Underlying the argument is an admixture of theoretical reasoning, computation, and sheer commonsense, that is typical of the way economists proceed when they lack detailed evidence.

Consistently with his attempted justification, Hicks argues that the level of aggregation *over goods* matters a great deal to the empirical confirmation of the law. Since income effects depend on the amount spent, they are stronger for goods grouped together than taken in isolation, as in the previous sugar example. Accordingly, observed violations of the law may be due to a measurement problem. Available consumption data rely on classifying goods into broad categories such as food, clothing, travel, and for these artificially constructed commodities the magnitude of the income effect can become quite large. If the construct has the property of an inferior good, this effect will be positive, with the consequence that a Giffen-like exception may emerge from the data.

It is not easy to assess the all-considered view of the law of demand among today's economists, but I would conclude that most of them take it to be *empirically acceptable within its established limits*. Rather than taking the Slutsky equations as a way of uncovering significant exceptions, they use them in Hicks's way in order to argue that exceptions are bound to be rare. The law, it is sometimes said, holds true of "ordinary consumer goods" for "an ordinary consumer's budget". The ill-defined statement is meant to exclude all troublesome cases at once, i.e., (a) luxuries, fashions, and speculative goods, because they weigh little in the budget; (b) factors of production, because they are irrelevant to the consumer; (c) Giffen goods *if* they exist and have upward sloping demand curves. Only specialists are aware of the problem created by aggregation over commodities.

Philosophers of science will probably be struck by the shallow basis on which the economists' conviction lies. This is how things often go in the field. The early founders, Cournot and Marshall, may be forgiven for discussing the law and its exceptions casually, but the cavalier attitude has persisted virtually up to now. Although suggestive, Hicks's argument is far from compelling. I did not invoke Samuelson because he refrained from taking sides, being content with the analytical claim that "it is not possible to deduce [from the Slustky equations] ... the ordinary expression for the law of demand" (1947, p. 115). Hicks and Samuelson were theorists, but the 20th century gap between theoretical and applied economics cannot serve as an excuse because *even applied economists have neglected the law of demand*. They concentrate on estimating and testing the Slutsky and associated relations; they also investigate more straightforward empirical claims, e.g., that consumers spend on food proportionally less the richer they are. The authoritative summary of econometric evidence by Deaton and Mullbauer (1980) does not discuss the law of demand. What these authors call "law of demand" (put in quotes, p. 44 and 51) is the statement that the own substitution effect is negative. Phlips's (1983), Blundell's (1988) and Chiappori's (1990) surveys similarly omit the topic. This is the more striking since some of the reported evidence could be used as a step towards the missing test. For instance, it helps to estimate the so-called *Engel curves*, which relate the consumers' demand to their income, prices being fixed, because this identifies inferior goods. Generally, before Hildenbrand's work, there is no sustained work on the empirical limits of the law of demand. So when Hildenbrand (1994, p. 12-13) claims that the available "justifications" of the law of demand are either "a priori" or introspective, he is entirely correct - leaving aside the perhaps questionable terminology.

I complete my general discussion of the law of demand by resolving two semantic ambiguities infecting today's talk when the topic is discussed again. The first relates to the very meaning of the expression "law of demand". With typical nonchalance, economists employ is to mean three different things, i.e., (a) the traditional law (Hicks's and Samuelson's use of the expression), (b) the property that the own substitution effect is negative (Deaton and Muellbauer's use), and finally, another mathematical statement (c) which I now introduce. Suppose that prices change from $\bar{p}$ to p, the consumer's income being constant, so that $x(\bar{p}, \bar{I})$ and $x(p, \bar{I})$ are the corresponding demand vectors. Then, the following inequality:

(***) $(p - \bar{p}) \cdot (x(p, \bar{I}) - x(\bar{p}, \bar{I})) < 0$

relates the two vectors of changes in prices and quantities. Geometrically, it says that the vectors point in opposite directions of the *l*-dimensional commodity space.[8] If the change takes place in p_j alone, the inequality reduces to the traditional law in a discrete (noninfinitesimal) version. A definite advantage of the present version is that it alleviates Marshall's ceteris paribus clause;

[8] Hicks (1956, p. 139) first introduced (***) as another form of the law of demand. An earlier version of his involved $h_j(p, \bar{v})$ instead of $x_j(p, I)$ (1939-1946, p. 52).

now, only the consumer's income has to be fixed. I need this *generalized* version of the law of demand because Hildenbrand specializes in it. His choice is unsurprising given his testing purposes. Beside the handier ceteris paribus clause, an important reason to investigate (***) instead of the one-good law is that very little can be said if the fails against purchase data. The failure could indeed be due to the inappropriate grouping of distinctive commodities into a single aggregate. This argument may rationalize the previous economists' loose attitude to some extent - they might have been sceptical of one-good tests, while being either unaware of (***) or unable to perfom a test *across the economy* as this form of the law requires.[9]

Here is the second, more subtle semantic problem: does the law of "consumer" demand relate to the individual or some aggregate of individuals? Cournot envisaged only the demand to the market, in which Marshall was primarily interested. This should not be taken to mean that these two economists disbelieved that there were an *individual* law of demand. On the contrary, there is definite evidence that Marshall conceived of the law as being well-established - granted the exceptions - at the individual level. He certainly believed that collective demand functions smoothed out irregularities, but this is a different point - the statement of the law only requires the function to be decreasing, not differentiable or even continuous. Similarly, nothing can be concluded from Hicks's often-quoted claim that "a study of individual demand is only a means to the study of market demand" (1939-1946, p. 34). Most of the passage around this sentence is in fact concerned with the individual consumer (e.g., p. 32), and I would argue that even his later work is oriented the same way. From this all too brief survey, I conclude that the most influential writers on the law of demand have never called into question that there was a sense, both theoretical and empirical, in placing it at the individual level.

These economists follow the strategy of establishing, first, a suitably qualified version of the law at the individual level, and then and only then, moving to the aggregate levels of the given market or the economy as a whole. The resulting aggregative law may call for qualifications - in terms of both ceteris paribus clauses and exceptions - different from those required by the individual law. The aggregative statement may be easier to make and better supported empirically, but the individual statement would be no less of a putative law for that. Admittedly, economists often replace the individual unit by the *household*. As long as they do not analyze this entity, the shift remains verbal. Some have proposed to reconstruct the household's demand function as the collective (and in part unintended) result of prior

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[9] Hildenbrand has a further reason for preferring (***), but it does not belong to the present topic. The law in generalized form has implications for the stability of general equilibrium, while it has none in the one-good form.

interactions between its members. If the law of demand held true of household demand functions, where "household" is taken in this serious sense, the law could be said to be genuinely collective; however, much remains to be done to extend consumer theory in this new direction (see, e.g., Browning and Chiappori, 1998).

Following a less detailed, but more widespread, non-individualistic construal of the law, it is roughly correct at the *market* level, while being false as a generality when taken at the individual level. The construal has been offered as a commentary on the existing work by a philosopher of economics: the law, Hausman writes, is "a generalization about markets, not individuals" (1992, p. 28). This statement and the related ones in economics, like Becker's (1962), may point out the direction of empirical success, but I do not think that they capture the orthodoxy of consumer theory. In view of what I just said, they cannot be taken to *describe* what Marshall and Hicks put into that theory, and I could have expanded even more easily on Samuelson (1947). If Hildenbrand stresses that the law of demand should be reconstructed as an aggregate phenomenon, this is precisely because this line was *not* pursued consistently before.

3. Hildenbrand's programme and contribution

In *Market Demand* (1994) and related pieces, Hildenbrand distances himself from the standard treatment on two scores I have already discussed. First, he argues that consumer theory does not give a sufficient theoretical and empirical basis for accepting the law of demand. He states his complaint as follows: "I am afraid that all properties that have been formulated so far for individual demand functions, for example, the hypothesis of utility maximization or the Weak Axiom of Revealed Preference,[10] are entirely grounded on a priori reasoning" (1994, p. 12). When reading this sentence, philosophers should bear in mind that economists never use "a priori" in the technical acceptation of "being true or false independently of all experience". Over and beyond propositions, such as linguistic truths, which satisfy this sense, economists mean a wide class of empirical propositions. The latter have the distinctive feature that only the *existing* stock of knowledge, whatever its origin, supports the claims made about their truth or falsehood; they have not given rise to special investigations, and in particular, to proper empirical tests. Hence, by criticizing "a priori" defences of the law of demand, Hildenbrand excludes both linguistically-based arguments for the law of demand (some were influential in

[10] After Samuelson, economists sometimes take one of the revealed preference axioms as alternatives to the Maximization Hypothesis; see below.

the Austrian tradition of economics), and incomplete empirical defences (the main representative being Hicks's argument in last section).[11]

Breaking away from "a priori" reasoning thus conceived, Hildenbrand aims at providing the law with the genuine test it never had. However, in order to reach this target, *Market Demand* follows a heavily theoretical strategy, which comes to a surprise, given the direct empiricism of his critique. Hildenbrand argues for his roundabout approach on the ground that the law of demand, *even in the generalized form*, cannot be tested directly: "The Law of Demand ... does not refer to the actual evolution of prices, but to hypothetical changes within the same period" (1994, p. 5). I understand this terse statement as follows. It takes time for any real consumer to express his demand vector as a reaction to prices and income, which poses a first obstacle to the test, since many things - not only price-income variables, but even the consumer's utility function - may change from one period to another. Now, one may take the periods to be so short that they exclude any disturbing change. In other words, with a suitably small time unit, each period's *empirical* price-demand schedule can meaningfully be compared with the *theoretical* mapping $p_j \mapsto x_j(p, \bar{I})$. But this only leads one to bump into another obstacle: the smaller the time unit, the fewer the available observations. In the limit, there will be a *single* price-demand pair observed for any given period; any other price-demand pair is "hypothetical" in the sense of not belonging to the empirical scheme of the same period. In brief, Hildenbrand's view amounts to a dilemma: if the time unit chosen for statistical measurement is too long, the test is impossible, and if it is too short, the test is also impossible.

What I have described as a *dilemma*, others would describe as a *trade-off*. There may be a value of the time-unit that reduces both horns to an acceptable level of inconvenience; applied economists may sensibly conclude that three or six months approximate the optimal trade-off, and these are the time units present in many consumption studies. I do not want to push this objection too far because, if pressed, Hildenbrand could call important pragmatic reasons to the rescue. The evidence on consumption is roughly divided between *time series*, which are data relative to different time periods, and *cross-sectional data*, which relate to different households within the same time period. The most revealing time series, called *panel data*, follow the same households' consumption at different times. Although panel data are becoming increasingly available, and are now specially favoured by consumption theorists, the bulk of evidence is still made out of either cross-sectional data or time-series relative to changing populations; both are indeed common in national statistics. The latter involve, or may involve, changes in too many of

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11 The analysis of the economic a priori in this paragraph draws on Mongin (forthcoming).

those variables - the households' incomes, utility functions, and even personal identities - which a test of the law of demand requires one to keep fixed. It makes things worse that time-series do not normally exhibit sufficient variation in the price and quantity variables which are the objects of the statistical investigation (Hildenbrand, 1994, p. 9; see also Deaton and Mullbauer, 1980). There remain cross-sections, which have the single defect of letting the households' identities change. All of Hildenbrand's tests are based on these data. Instead of claiming that the law of demand cannot be tested directly, he could argue for the weaker point that *it is pragmatically better to test it indirectly* because (i) cross-sectional data are the only ones that are both widespread and usable, and (ii) a test based on these data is of necessity indirect (since it involves considering different individuals, contrary to the law).

I move to Hildenbrand's second disagreement with today's conception of the law of demand. He believes that if it ever approximates an empirical truth, this can only be *at the market level*. "The Law of Demand does not refer to the demand of an individual household, but to market demand, that is to say, to the mean demand of a large population of households, for example to all private households in Germany or the United Kingdom" (1994, p. 3-4). At the end of last section, I have argued that claims of this sort clash with the orthodoxy of consumer theory. So it comes as a surprise that Hildenbrand finds support in Hicks: "The market [income effect] is the sum of the individual *I*'s... For [this effect] to be negative, there must be a balance of negativeness among the individual *I*'s that compose it... The probability of exceptional cases is diminished when we take a large group of heterogeneous consumers together" (1956, p. 136). However, as Hildenbrand notes elsewhere, Hicks's passage means a gesture without serious consequence. It is typical of the way economists toy with theoretical possibilities that they do not really entertain.[12]

Now to the programme explicitly stated. It preserves the conventional distinction between a negatively signed substitution effect and a nondescript income effect. Pursuing Hicks's glimpse in his way, Hildenbrand finds out that *the income distribution might have the favourable consequence of wiping out positive income effects*. "Aggregating individual demand over a large group can lead to properties of the market demand function which, in general, individual demand functions do not possess. There is a qualitative difference in market and individual demand functions" (1983, p. 998). Hildenbrand's belief is based on a simple calculation that might have provided the heuristic for his whole research programme. If all consumers have the same utility function and the income density is uniform over $[0,1]$, it

[12] See Hildenbrand's (1989, p. 258-259) comment on Hicks. Becker's (1962) crude argument for the aggregate law of demand is another example to the point.

follows that the average income effect is non-positive.[13] Of course, in any relevant community, utility functions will differ across individuals, and the statistical distribution of income is unlikely to conform with the uniform assumption. But the irrealistic case disposes of the *theoretical* preconception that to move the law from the individual up to the market level can only mean further trouble. The challenge becomes to explore the class of statistical distributions that deliver a negative average income effect. This is a purely theoretical move, but depending on whether acceptably realistic distributions are found in the class, it may lead to an empirical grounding of the law of market demand. A distinctive feature of the resulting programme is that income effects become the sole object of attention. Hicks's hint was compatible with the different, actually much more Hicksian, strategy of establishing that aggregate income effects are small *in magnitude* relative to corresponding substitution effects. At the individual level, Hicks is prepared to strike a balance between effects of opposite signs; similarly when he considers the collective level.

Compared with the 1983 paper, which exemplifies Hildenbrand's heuristic, his 1994 book reveals a shift in the way he captures the statistical element of the law. Instead of putting his asssumptions on the income distribution directly, he now makes them on *the functional dependence between variations in income and the statistical diversity of consumption*. This more sophisticated strategy leaves him within the confines of the programme, because he still aims at showing that aggregate income effects are non-positive. There are two versions of the new approach, which differ in how they define the functional dependence just said. Hildenbrand's condition of Increasing Spread of Household Demand is a less transparent measure of statistical variation than his alternative condition of Increasing Dispersion of Household Demand, but it leads to simpler mathematics, which is why I selected it here. (When I say, following Hildenbrand, "household" instead of "individual", this is just a nominal change motivated by the fact that statistical data concern households, not individuals.)

Hildenbrand's target of analysis is the generalized law of demand (***), and the prices changes considered in this statement are discrete, which blocks direct comparison with the Slutsky equations (*) or (**). However, (***) can be shown to be equivalent to the property that the matrix M^D be negative definite, a property which implies the Marshallian law in differential form:

[13] Here is the calculation:

$$-\int_0^1 x_j(p,I).\frac{\partial x_j(p,I)}{\partial I}.dI = -\frac{1}{2}\int_0^1 \frac{\partial x_j^2(p,I)}{\partial I}.dI = -\frac{1}{2}\left[x_j^2(p,1) - x_j^2(p,m)\right] = -\frac{1}{2}x_j^2(p,1) \le 0.$$

$$\frac{\partial x_j(p,I)}{\partial p_j} < 0,\ j = 1,...,l\ .$$

In view of this restatement, the Slutsky equations deliver a sufficient condition for (***) to hold. Returning to the matrix form of these equations, we know that M^{SE} is negative definite. Hence, it is sufficient for the generalized law that the other term M^{IE} be negative definite.

Conveniently, both the generalized law and the Slutsky equations keep the same linear form whether they bear on household demand functions or the *average* or *sum* of these functions. Hildenbrand's analysis goes in terms of average uncompensated and compensated demand functions:

$$X_j(p) = \frac{1}{|P|}\sum_{i\in P} x_j{}^i(p, I^i)\ ,\ S_j(p) = \frac{1}{|P|}\sum_{i\in P} s_j{}^i(p, I^i)\ .$$

Here, x^i and s_j^i stand for household *i*'s uncompensated and compensated demand functions, I^i for *i*'s fixed income, P for the set of households, and $|P|$ for their number. It is routine to reformulate (*), (**), and (***) in terms of X_j and S_j. When rewritten in this way, the last equation constitutes Hildenbrand's definition of the *law of market demand*.

The theoretical strategy will be to fulfill the sufficient condition just explained, i.e., to derive

Negative Average Income Effect: the matrix M^{IE} of average income effects is negative semi-definite.[14]

Thusfar, I have only mentioned known material, but what comes next contains Hildenbrand's genuine contribution. By showing that Negative Average Income Effect is equivalent to Increasing Spread of Household Demand, he demonstrates that the former can receive a statistical interpretation.

Increasing Spread of Household Demand: for any $\Delta > 0$, the vector family $\{x^i(p, I^i + \Delta)\}_{i\in P}$ is more spread than the vector family $\{x^i(p, I^i)\}_{i\in P}$.

The spread concept is a measure of statistical diversity that takes into account both the dispersion of points around their mean and their distances to the origin. If one visualizes a household's demand vector as a point in the commodity space, the condition says that the cloud of vectors after an income change Δ will look be more scattered - relative to both the mean and

14 To deal with a negative semi-definite, instead of definite, income effect matrix facilitates the deductions to come. The condition is sufficient for the generalized law as long as the substitution effect matrix is negative definite.

the origin - than does the cloud determined by the initial incomes. With this ingenious restatement, Hildenbrand makes a step towards the test; however, the condition is evocative of time-series, not cross-sections, which means, given Hildenbrand's reliance on the latter, that he is not yet done.[15]

Hildenbrand's test could proceed if he had a theoretical argument to substitute the *time* changes in income and demand that are experienced by *the same* households with *external* comparisons between incomes and demands that the observer makes for *different* households taken at a time. With such an argument, cross-sections and time-series would become interchangeable. This replacement heuristic motivates the next step. Given a sufficient condition to be discussed shortly, Hildenbrand demonstrates that Increasing Spread of Household Demand can be obtained from the following condition, in which $P(I)$ and $P(I+\Delta)$ denote the subsets of the population P having income I and $I+\Delta$, respectively.

Average Increasing Spread of Conditional Demand: when I and $I+\Delta$ vary over all possible values, the vector families $\left\{x^i(p,I^i)\right\}_{i\in P(I+\Delta)}$ are on average more spread than the vector families $\left\{x^i(p,I^i)\right\}_{i\in P(I)}$.

It would be more transparent to require that $\left\{x^i(p,I^i)\right\}_{i\in P(I+\Delta)}$ is more spread than $\left\{x^i(p,I^i)\right\}_{i\in P(I)}$ not on average over the values taken by $(I, I+\Delta)$, but for each such pair individually. However, this very strong statement is unnecessary for the purpose of deriving Increasing Spread of Household Demand.[16]

With the present condition, Hildenbrand claims to have made the Law of Market Demand testable. Instead of submitting the income group $P(I)$ to a joint change Δ, it is enough to compare $P(I)$ with the coexisting group $P(I+\Delta)$. This comparison can be performed from national cross-sectional data such as Family Expenditure Surveys (UK) and Enquête Budget de Famille (France).

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[15] Hildenbrand would be no better off if he were resorting now to time-series; for it is unlikely that any such series describes changes in consumption resulting from a *uniform* change in income. This difficulty is often mentioned in consumption studies.

[16] It would also raise an empirical problem in view of a finding mentioned by Hildenbrand (1994, p. 130).

I now state the sufficient condition that permits deriving the first version of Increasing Spread from the second.

Metonymy (strong form): those variables other than income which influence demand are probabilistically independent of income.

This is a provisional statement, which only serves heuristically. Suppose that the effect of income on demand can be appraised independently, in a probabilistic sense, of the effect on demand of any other variable influencing demand. Then, an income variation from I to $I+\Delta$ should bring about *the same* change in demand, whether it is incurred by one group $P(I)$ or relative to coexisting groups $P(I)$ and $P(I+\Delta)$. If the results differed in any significant way, this could only be due to some household characteristic being correlated with income, contrary to the initial assumption. Hildenbrand's real **Metonymy** only requires probabilistic independence to be satisfied locally and on average (1988, p. 261-262 and 1994, p. 153). I do not formalize this final condition, and just stress that it differs from Metonymy (strong form) to a very significant extent. The sweeping independence claim would be all too easy to refute empirically. Think of the influence exerted by age and education level on the income distribution; inspection of the statistics strongly suggests that these two characteristics are positively correlated with income level.

To replace a diachronic piece of information by a synchronic one is relatively common practice in econometrics. For one, temporal series may not be available while corresponding cross-sectional data are; for another, when both are available, economists often prefer the latter to the former. Generally, a choice should be made between two disputable assumptions - for one, the relations governing individual behaviour are constant through time, and for another, identical relations govern the behaviour of different individuals at a given time. With ordinary time-series, the constancy assumption must hold of an evolving population, and this makes it extremely dubious. However, the final choice depends on the problem and quality of data, and econometricians sometimes adopt the strategy opposite to Hildenbrand's, i.e., replace synchronic by diachronic information. The ideal case is when both sets of data are exploitable for the test.[17]

[17] It becomes possible to estimate the theoretical relations on one set and test those taken to be hypothetical on the other set; see Friedman's (1957) pioneering study of consumption.

The paper is not about econometrics, and I will not comment on the tests effectively performed by Hildenbrand and collaborators[18]. Suffice it to say that Average Increasing Spread of Conditional Demand is the only condition that *Market Demand* submits to a test, and that the book finds it to be "well supported by the cross-section data" (1994, p. 27).[19] Hildenbrand concludes that this finding "justifies" the Law of Market Demand. The support he means is *a good deal theoretical* because he has attempted to test neither the Slutsky equations, nor his Metonymy condition. However, this support is *also in part empirical*. "It seems to me impossible to give a deductive validation of the Law of Market Demand where *all* required hypotheses are supported by empirical evidence... My goal therefore is to make the unavoidable a priori assumptions on households' behavior as weak as possible and to base the deductive validation on at least one hypothesis that has good empirical support" (p. 18). Note again the use of "a priori" to refer to the vague forms of empiricism that fall short of a testing attitude.

Having isolated an important claim about empirical confirmation, I will inquire how existing theories of confirmation could possibly rationalize it. Section 4 provides the necessary background for this analysis, which is carried out in the final section 5.

4. An aside on confirmation theory

In order to discuss the confirmation of the law of demand, I propose to rely on Hempel's (1965, ch.1) classic list of abstract conditions for the *confirmation relation*. This relation, which I denote by $C(E,H)$, definitionally holds between any two sentences in some logical language, but the purported interpretation is of course that E represents an *evidential report* and H a *hypothesis*. The fine analysis of what counts as evidence, and more specifically observational evidence, goes beyond the scope of Hempel's work, as he himself recognizes.[20] I will follow him in assuming that evidential reports relate to the simplest constituents of potential knowledge, and that the logical language adequately captures these constituents in its atomic sentences $F(a,...)$, $\neg G(b,...)$,..., where F, G,... are n-ary symbols of predicate and a, b,... are symbols of constants. An evidential report has the logical form of a finite conjunction of atomic sentences. By requiring that all symbols refer to observable entities and properties, one gets the

[18] They are explained in Härdle, Hildenbrand and Jerison (1991) as well as *Market Demand* (ch. 2, p. 97 sq, and p. 133 sq).

[19] Data are borrowed from British and French national data.

[20] Neo-positivists have surprinsingly little to say about what constitutes an observation, and more generally, a piece of evidence.

more specific concept of an observation report. In this case, the restriction to finite conjunctions is more than a modelling device; it encapsulates the claim that only a finite number of entities and properties can be observed at a time. Hempel also employs a *disconfirmation relation*, which he defines derivatively as $D(E,H)$ iff $C(E,\neg H)$.

The main purpose of Hempel's and related conditions is to compare the $C(E,H)$ relation with the deductive inference relation of the logical language. This relation will be symbolized by $K \triangleright B$ where B is a sentence, and K a set of sentences. By abuse of notation, $A \triangleright B$ means that sentence B deductively follows from sentence A, and $A \triangleleft\triangleright B$ that the two sentences are deductively equivalent. For ease of expression, I often identify syntactical signs with the logical objects they mean or refer to.

Entailment Condition: If $E \triangleright H$, then $C(E,H)$.

If verification and refutation are defined in the standard way - i.e., H can be deduced from E, and H is contradictory with E, respectively - then the condition is equivalent to saying that verification is a particular case of confirmation, and that refutation is a particular case of disconfirmation. These seem to be analytical properties, and as such, they are desirable.

Minimal Consistency Condition: If $C(E,H)$, then E and H are logically compatible.

Again, this sounds definitional, and it purges the entailment condition from the trivial objection that a contradictory E confirms every H.

Maximal Consistency Condition: If $C(E,H)$ and $C(E,H')$, then E, H and H' are logically compatible.

The maximal condition goes far beyond the minimal one by requiring that the same piece of evidence cannot confirm two inconsistent hypotheses. This heavy requirement may be desirable abstractly but does not have to be imposed in all and every circumstances.

Consequence Condition: If $C(E,H)$ for all $H \in K$ and $K \triangleright H'$, then $C(E,H')$.

Special Consequence Condition: If $C(E,H)$ and $H \triangleright H'$, then $C(E,H')$.

Converse Consequence Condition: If $C(E,H)$ and $H' \triangleright H$, then $C(E,H')$.

Equivalence Condition: If $C(E,H)$ and $H \triangleleft\triangleright H'$, then $C(E,H')$.

This is the truly important group of conditions. According to the first and second (a particular case of the first), confirmation "descends" along the way of deductive inference; according to the third, it "ascends" by reversing the direction. Each of these three conditions implies the fourth. A theory limited to that condition would assert very little. At the other extreme, to accept Special Consequence and Converse Consequence both at a time would trivialize the confirmation relation, as the following example shows. Think of two semantically unrelated sentences, say E = "Tweety is a raven", and H = Slutsky's equations. Then, E would confirm $E\&H$ from Converse Consequence, and confirm H from Special Consequence. The problem for the theory of confirmation appears to take one of these two options: *either choose between*

Special Consequence and Converse Consequence, or restrict them both so as to get rid of this "tacking paradox".

Hempel's own theory of confirmation follows the former route and chooses Special Consequence, and even Consequence, against Converse Consequence; it actually satisfies all the list except for this last condition. His theory proceeds in two steps. For one, Hempel's *direct* confirmation relation holds if one can deduce from E what H asserts for the class I of entities mentioned in E - in Hempel's jargon, if one can deduce from E the *development of H for I*. Second, the confirmation relation - Hempel's final definition for $C(E,H)$ - holds if there is a set of sentences K such that E directly confirms every H' in K and $K \rhd H$. To illustrate these definitions, take $H_1 = (\exists x)\ F(x)$ and $H_2 = (\forall x)\ F(x) \to G(x)$. If $I = \{a,b\}$, the development of H_1 is $F(a) \vee F(b)$, and of H_2, $(F(a) \to G(a)) \& (F(b) \to G(b))$. The report $E_1 = F(a) \& \neg F(b)$ directly confirms, hence confirms, H_1, while $E_2 = F(a) \& \neg G(a) \& \neg F(b) \& G(b)$ does not directly confirm H_2 and does not confirm it either. Now, assume that $(\forall x)\ H(x) \to G(x)$ is an axiom of the logical language; then, $E_3 = H(a)$ confirms H_1 without directly confirming it.

Hempel's theory is open to well-recognized paradoxes, difficulties, or limitations, of which I single out only two. For one, H and E are to be phrased in the same observational vocabulary; for another, they are to be related irrespective of other sentences. In the most interesting applications, H represents a theoretical hypothesis, i.e., a claim made about nonobservable entities, properties, or states of affairs. Further, the confirmation problem often arises in the context of an experiment, in which auxiliary assumptions are bound to play a rôle. In such applications, H may well assert nothing of the entities mentioned in E. Starting from these two points, Glymour (1980) has reelaborated the theory of confirmation by positive instances. He relies on a trinary confirmation relation $C(E,H,K)$, which involves a set K of sentences that make E logically and semantically relevant to H. Glymour's *bootstrapping* construction is too subtle and controversial to be applied here explicitly.[21] But it will serve as a meta-philosophical warrant for my use of Hempel's cruder account. Bootstrapping also explores the commonsensical view that to confirm a statement is to instantiate it, and despite creating problems of its own, it takes care of several commonsensical objections that commonsense might also wish to raise against Hempel.

It has become standard in the textbook treatment of confirmation to contrast positive instantiation with hypothetico-deductive theories. However easy to the specialist, this distinction is not well taken in science and economics, and even philosophers of science

[21] See, e.g., the revision prompted on Glymour (1983) by his critiques.

occasionally miss it.[22] It is not a minor contribution of Hempelian analyses that they fully clarify the difference between, for one, confirming by *positive instantiation*, and for another, confirming by *deduction*. It seems uncontroversial to define a positive instance of H as any E satisfying Hempel's direct confirmation relation relative to H. With this definition, it becomes straightforward that positive instances are not the same as logical consequences, even when both are relativized to the same constants. Thus, H_2 admits as a positive instance $F(a)\&G(a)$, which is not a logical consequence, and the logical consequence $F(a)\rightarrow G(a)$ does not count as a positive instance. There are, of course, many routine cases, like H_1, in which the two ideas coincide, which probably explains why they were mixed up for so long.

A related reason why I emphasize Hempel's work is that it dramatizes the choice between the Special Consequence and Converse Consequence conditions. This feature connects with his rejection of hypothetico-deductivism, which, if unrestricted, must endorse Converse Consequence. Hempel regards the choice of Consequence as being so obvious as not to need any argument. This is a kind of superficiality that, contrary to others, Glymour has not fully remedied, and I have found only two hinted defences in his work. "Sometimes confirmation does seem to follow entailment, at least on some paths" (1980, p. 31). "As evidence accumulates, we may come to accept a hypothesis tentatively, and when we accept a hypothesis we commit ourselves to accepting all of its logical consequences" (ibid.). The first hint is an appeal to the scientists' practice. Clearly, they often use Special Consequence, and even Consequence, without saying so much. If economics may count as a science, the present study adduces clear-cut evidence to Glymour's factual claim. The difficulty, however, is that the implicit use of Converse Consequence is sometimes also well documented. If the issue can be resolved by a choice between the initial conditions, this will only be at the level of abstract philosophizing, so Glymour's second hint matters more than the first. Arguably, to regard a hypothesis as being confirmed entails *accepting* it, and to accept it entails regarding it *as being tentatively true*. If tentative truth follows the same path as truth, an argument has been adduced for the Consequence condition. Although it needs unpacking, this argument sounds reasonably promising.

There is a third argument for Consequence that neither Hempel nor Glymour states, perhaps simply because both take it to be obvious. It is an advantage of Hempel's calculus of confirmation that it does not deal only with hypotheses in the intuitive sense - i.e., $C(E,H)$ may apply to any sentence H. In this way, the statement that $C(E,E)$ is expressible, and since it can

[22] I suspect that some Popperian arguments against "confirmationism" conflate hypothetico-deductivism with positive instantiation.

be assumed to hold, Entailment becomes a corollary to Special Consequence. *To be a logical strengthening of a quasi-definitional condition reinforces the case for the latter*. Dually, Converse Consequence becomes a logical strengthening of the following condition outside the list:

Converse Entailment Condition: If $H \triangleright E$, then $C(E,H)$.

This condition is by no means definitional; indeed, it is highly problematic since it amounts to hypothetico-deductivism.

As far as I know, neither Popper nor his followers have ever stated their views on confirmation in terms of Hempel's conditions, and it seems obvious why they would not wish to do so. Popper's own concept of "corroboration", as he prefers saying, is based on the notion of a *severe* test, which has thusfar eschewed any formal modelling except perhaps in probabilistic terms.[23] This is an ambiguous state-of-affairs, because it may due either to the expressive possibilities of Hempel's logical language, which remain limited even after Glymour's improvements, or to the much more worrying problem that severity is an ill-defined concept. Things being as they may, I propose the short-cut of asking where Popperians *would* locate their position on Hempel's map if they *could* be convinced to take it seriously. No doubt, the question will strike serious Popperians as being non-sensical, but it seems to be relevant nonetheless, because many scientists and methodologists content themselves with the notion that Popperian "corroboration" obtains when the hypothesis is put to the test of empirical data and not contradicted by these data. Roughly speaking, they drop the severity component of "corroboration" to keep only its refutationist component - i.e., confirmation gathered from failed counterexamples rather than successful examples. Economic methodology is replete with variants of this simplified position.

As against the fuller one, the last meaning of "corroboration" is amenable to some discussion within Hempel's logical framework, and it appears that *anybody accepting it should also accept the Converse Consequence condition and accordingly reject the Special Consequence condition*. If what makes *E* confirmatory is that it fails to contradict *H*, the same argument applies to *E* and *H'* when *H'* deductively entails *H*. Converse Consequence is persuasive because of modus tollens, which is the logical core of refutationism. Special Consequence has to be rejected because of the trivializing argument; anyhow, there is no argument to accept it within the position I am exploring now. While granting that this is a blunt criterion, I will ask which of the two conditions agrees better with Hildenbrand's work, and

[23] See Popper (1935-1972, Appendix IX) and the ensuing discussion of this famous passage among Popperian and Bayesian philosophers of science.

depending on the answer, classify his implicit confirmation theory with either the Hempelian or the pseudo-Popperian group of theories.

5. A Hempelian analysis of the confirmation of the law of demand

Hildenbrand's theory of demand can be summarized as follows:

Maximization Hypothesis **or** Weak Axiom of Revealed Preference (households)

⇓

Slutsky's Equations & Negative Definiteness of Substitution Matrix (households)

⇓

Slutsky's Equations & Negative Definiteness of Substitution Matrix (average)

& IS (1st form) ← ⇐ **IS (2nd form) & Metonymy**

⇓ ↓ ↑

Law of Market Demand CROSS-SECTIONAL MARKET DATA

The double and simple arrows represent the deductive implications of the theory and the suggested direction of confirmation, respectively. In this scheme, confirmation by market data bears on Increasing Spread (2nd form) alone, and it is then channelled by a deductive chain to the Law of Market Demand. The first step is from Increasing Spread (2nd form) and Metonymy to Increasing Spread (1st form), and the second, from Increasing Spread (1st form) and some consumer theory premisses to the Law of Market Demand. The salient point is that confirmation past the stage of data is descending, not ascending. *Hildenbrand's confirmation scheme obeys Special Consequence, not Converse Consequence*.

There is no ambiguity surrounding the absence of Converse Consequence from the confirmation scheme, because the logical structure of the theory gives enough room to apply it if there is a point to do so, but Hildenbrand does not seize this possibility. Converse Consequence would entail that market data confirm the conjunction of IS (2nd form) with Metonymy, not just the first conjunct. If upheld, this alternative version would make an empirical test of Metonymy unnecessary. But Hildenbrand writes as if he were worried that only Increasing IS (2nd form), *and not also Metonymy*, had been tested. So there is - admittedly indirect - evidence that he does not consider the alternative variant. Remember the comment quoted at the end of section 3: "It seems to me impossible to give a deductive validation of the Law of Market Demand where *all* required hypotheses are supported by empirical evidence...".

This apologetic tone would make no sense if Hildenbrand believed that conjunctions are confirmed when one of the conjuncts is.

Related to Hildenbrand's rejection of Converse Consequence is the point that he nowhere attempts to relate his test of Law of Market Demand to the Maximization Hypothesis. If, glossing over the trivialization result, Hildenbrand was prepared to let confirmation both descend and ascend, he would conclude from his confirmatory test that the Maximization Hypothesis is also confirmed. Not only does he not take this step, but there is all indication that he would declare it to be unwarranted. If he were willing to divert some empirical support from his data to the Maximization Hypothesis, *Market Demand* would not replace it by the *Weak Axiom of Revealed Preference* (see 1994, p. 18). This statement, which I have not introduced earlier to simplify my exposition, performs in Hildenbrand the rôle of a less demanding sufficient condition to reach the Slutsky equations and the Negative Definiteness of Substitution Matrix, i.e., the second line in the table.[24] There are two other assumptions the logical content of which Hildenbrand has managed to minimize (Metonymy and IS(1st form)). Generally, this attitude is consistent with one's channelling confirmation towards the consequences, and inconsistent with the other way round.

I have thus shown that Hildenbrand's confirmation scheme accords with Hempel's choice of a condition. Even though this choice does not characterize a *C(E,H)* uniquely, it would perhaps not be too much of a jump if I concluded that this scheme agreed with Hempel's positive instantiation theory. But there is a major exegetical problem standing in the way of this conclusion. Hildenbrand has a proclaimed interest in *falsifiability*. He sometimes deemphasizes his objective of confirming the law of demand to lay the stress on another objective: *to put forward testable propositions in the potential falsification sense*. See, e.g.: "To go from [the Weak Axiom] to the Law of Demand, we should use properties of the consumption sector which are at least in principle falsifiable by observable consumption behavior" (1988, p. 257). Consistently with this claim, the reason he gives to discard IS (1st form) is that it is not "falsifiable", and the reason he gives to retain IS (2nd form) is that, by contrast, it is "falsifiable" (1994, p. 22 and 26). The claim suggests an unresolved tension between Hildenbrand's Hempelianism and some latent form of Popperianism. According to the criterion put forward in

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24 If *p* and *p'* are two price vectors, and I^i et I'^i two levels of income of consumer *i*, the resulting vectors of demand are such that $p.x^i(p', I'^i) \leq I \Rightarrow p'.x^i(p, I^i) \geq I'^i$. This condition was introduced in a slightly stronger form by Samuelson.

the previous section, he should make a clear choice between one family of confirmation theories and another.

However, the criterion is not crude to the point of excluding any consideration of falsifiability in the Hempelian family of confirmation theories. Hempel himself does not insist on it, but Glymour (1980, p. 130-131, condition iv) does; he makes falsifiability a precondition for his bootstrapping confirmation. Because both Hempel and Glymour introduce Special Consequence at the stage of their final definitions only, one may clarify the rôle of refutability thus: it is a precondition of *direct confirmation alone* (positive instantiation); it has not bearing on confirmation in general. As a paradigmatic case, a statement which has no potential falsifiers is confirmed nonetheless, if it is deduced from a collection of statements one of which is directly confirmed, hence also refutable. This description fits Hildenbrand's commitments very well. Remember that he declared the one-good law of demand, and even the more assertive generalized law, to be untestable. He understands testability in the potential falsification sense, and this is consistent with his overall position. He means to say that the law is unfalsifiable, but that this does not prevent it from being confirmable (or testable in a different sense).

Granting that Hildenbrand's work conforms altogether with Hempelian-Glymourian strictures, it remains to decide whether it provides *an informative test* against this philosophical background. Neither Hempel's nor Glymour's definitions are intensive, but they are certainly compatible with qualitative comparisons of the following sort: the more confirmed propositions and the less disconfirmed propositions there are among the premisses, the more confirmed is the conclusion. How much confirmation in this sense does the Law of Market Demand receive from Hildenbrand's empirical tests? Not very much, it would seem, because among the premisses of the Law, only IS (2^{nd} form) is subjected to a test - remember Hildenbrand's complaint on this score. The components borrowed from consumer theory are certainly worrying, given the poor empirical record of the so-called *Slutsky conditions*. These are: (i) the negative definiteness of the substitution effect matrix M^{SE}, which is the condition of interest here; (ii) the symmetry of M^{SE}; (iii) the homogeneity of degree 0 of demand functions $x_j(p, I)$. Conditions (ii) and (iii) have been the target of much econometric criticism (see Deaton and Mullbauer, 1980, p. 80). The trouble with (i) is not that econometricians have shown it to be empirically shaky, but that they have not managed to test it independently of (ii) and (iii).[25] Hildenbrand's replacement of

[25] A major difficulty here is that the standard tests for negative definiteness are devised for *symmetric* matrices (mathematically, they amount to checking the signs of the eigenvalues of

the Maximization Hypothesis with the Weak Axiom of Revealed Preference does not alleviate the problem. In a sense, it makes things worse because tests have been effectively performed on the Weak Axiom and they are not considered to be very favourable.[26]

Metonymy raises problems of its own. From *Market Demand*, it is not clear whether Hildenbrand does not investigate it because he is sufficiently busy testing IS (2nd form), or because he regards it as being unfalsifiable, hence not directly confirmable (see 1994, p. 133, where he just says "the untested metonymy condition"). In previous work, Hildenbrand (1988, p. 266-267) treated Metonymy as a constraint put on the data in order to carry out his estimations; this does not help answer the question.[27] Metonymy (Strong Form) appears to be falsifiable, and even falsified, but Hildenbrand's much less demanding Metonymy may be neither. Going here beyond his meaning, I suggest viewing it as *metaphysical*, in a sense which I have elsewhere tried to clarify and apply to economics broadly. On this account, a proposition in a scientific theory is metaphysical if, first, it serves always as a premiss and never as a conclusion (it is a "first principle"); second, it is not specific to the theory and perhaps even to the field of inquiry; and third, it is untestable, both in the Popperian sense of being irrefutable and the Hempelian sense of being unconfirmable (this last feature is related to the fact that the proposition serves only as a premiss). Metonymy satisfies the first two properties and arguably the third, although this should be checked in all detail.

I complete my investigation of the law of demand by asking whether the Hempelian reconstruction also fits the earlier work on the topic. Hicks's virtually unique justification shares with Hildenbrand's the crucial property of being deductive. It is supposed to channel empirical support from the Slutsky equations, plus another premiss, to the law of demand conceived of as a logical consequence (although the effective derivation is unrigorous). As I reconstruct Hicks's view of the Slutsky equations, they belong to the Hildenbrandian "a priori". So if the law inherits genuine empirical support, it must come from *the added premiss,* which simply says that the consumer diversifies his buyings. Indeed, this statement blocks an empirical good from being Giffen; it must itself be taken to be empirical in a straightforward sense. Very roughly, it

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the matrix, but it is only for negative definite symmetric matrices that these signs are determined to be negative).

26 Contrary to the Slutsky conditions, the Weak Axiom is a discrete condition, and as such, easier to relate to consumption data. Another reason for being suspicious of the Weak Axiom is that it still entails the empirically dubious condition (iii).

27 Hildenbrand's 1988 estimations directly relate to the sign property of the income effect matrix.

plays the rôle of IS (2nd form) in Hildenbrand - whatever direct confirmation it receives is transformed into indirect confirmation for the law, granting that the deduction also requires "a priori" assumptions. This is the same kind of confirmation scheme, except of course that there is no genuine test, only casual empiricism to guarantee the diversification property.[28]

Another relevant contrast is that Hicks gives no reason why the confirmation of the law of demand has to be indirect, whereas Hildenbrand argues that the law is not falsifiable. Without tackling this question, Hicks implicitly answers it when he lists the standard exceptions; he must take the law to be falsifiable, at least at the level of casual observation. Consistently, Hildenbrand ignores the exceptions. Despite this strong dissimilarity, Hicks considers only *negative* direct evidence. I do not think he saw any satisfactory way of *confirming* the law except, again, by deductive means. I conclude that by filling the gaps in his loose reasoning, one could show it to conform with the Hempelian reconstruction presented here. This discussion suggests that the confirmation scheme spelled out in the case study extends more generally to demand theory. Whatever richer potential of application it may have belongs to another paper.

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[28] Readers who underplay this difference may be prone to conclude that Hicks's justification fares better than Hildenbrand's because it is so much simpler. They should remember that the former is concerned with the law of demand, and the latter with its generalized form.

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