ECOLE POLYTECHNIQUE

CENTRE NATIONAL DE LA RECHERCHE SCIENTIFIQUE

On the Confirmation of the Law of Demand

Philippe Mongin

June 2005

Cahier n° 2005-015

LABORATOIRE D'ECONOMETRIE

1rue Descartes F-75005 Paris

(33) 1 55558215

http://ceco.polytechnique.fr/

mailto:labecox@poly.polytechnique.fr

On the Confirmation of the Law of Demand[1]

Philippe Mongin[2]

June 2005

Cahier n° 2005-015

Résumé: L'article rapporte la loi microéconomique de la demande aux thèses sur la confirmation empirique développées, respectivement, par Hempel et par Popper. Après avoir rappelé les bases hicksiennes de la théorie du consommateur et souligné que la loi n'y avait jamais fait l'objet d'un test rigoureux, il analyse la contribution novatrice d'Hildenbrand. Dans Market Demand (1994), celui-ci propose une dérivation logico-mathématique originale de la loi de la demande au marché, qui vise à rendre enfin possible un test de cette loi. L'article montre que la démarche d'Hildenbrand s'inscrit dans une perspective qui est néo-hempélienne, et non pas poppérienne, et il tire argument de cette étude même en faveur de la première contre la seconde.

Abstract: The paper relates the microeconomic law of demand to the theories of empirical confirmation that are developed by Hempel and Popper, respectively. After restating the Hicksian basis of consumer theory and stressing that this theory has never subjected the law to a rigorous test, the paper analyzes Hildenbrand's novel contribution. In Market Demand (1994), Hildenbrand offers an original derivation of the law of demand to the market, with a view of making it eventually possible to test the law. The article shows that Hildenbrand's approach makes sense within a neo-Hempelian, as opposed to a Popperian, outlook on empirical confirmation, and it uses the case itself to argue for the former against the latter.

Mots clés : Loi de la demande, théorie du consommateur, confirmation empirique, réfutation empirique, Hempel, Popper, Hildenbrand

Key Words : Law of demand, consumer theory, empirical confirmation, empirical refutation, Hempel, Popper, Hildenbrand.

Classification JEL: B21, D60, D63, D71

[1] I would like to thank Roberto Lazzari for stimulating discussions on the case study of this paper, and the participants to the Cercle d'épistémologie seminar for helpful comments.

[2] Laboratoire d'économétrie, CNRS et Ecole polytechnique.

1. Introduction.

The theory of consumer demand, or consumer theory for brevity, is a convenient target for philosophy-of-science discussions of economics. Marshall coarsely sketched it at the dawn of 20th century, and Slutsky made the most lasting contribution shortly afterwards. It received its final touch from Hicks, Samuelson, and a handful of eminent post-war followers. Roughly speaking, by the 1950's, microeconomists could conclude that they had gained command over the main conceptual issues, and by the 1970's, that they had mastered the remaining mathematical difficulties.[1] In sum, the theory is stable without being too old, and its long history demonstrates that economists invest it with special importance. Philosophers of science should be pleased with working on such a well-behaved case.

This paper does not reassess consumer theory as a whole, a task which others have attempted.[2] I will specialize in one of its famous statements, the law of demand, and specifically investigate the contribution made by Hildenbrand (1994) to its theoretical and empirical analysis. Despite two slight drawbacks of this work for a reflective exploration - it is fairly technical and still ongoing - I will use it to illustrate how a high-caliber economist might conceive of, and attempt to solve, the classic problem of the confirmation of scientific theories. Specifically, I will show that Hildenbrand's contribution puts in focus the desiderata that Hempel (1965) considered for a satisfactory confirmation concept, most prominently the *consequence condition* and *converse consequence condition*. These two requirements go in opposite directions and roughly demarcate between Hempelian theories of confirmation, which accept the consequence condition and reject the converse consequence condition, and Popperian theories, which I will argue here (I do not know how novel this argument is) are bound to make the opposite choice. As I reconstruct Hildenbrand, he must endorse the consequence condition, hence be Hempelian, a conclusion which creates a problem in interpretation because - like many of today's economists - he insists on *refutability* and may thus seem to incline in the Popperian direction. I solve this apparent conflict in terms of a neo-Hempelian view of confirmation.

Section 2 sets the stage by explaining what remains of the law of demand in consumer theory. It restates the Slutsky equations, which are the starting point of any current work, including Hildenbrand's. Section 3 sums up this economist's specific programme and contribution. Section 4 is a bird-eye review of confirmation theory, which emphasizes the Hempelian requirements. Section 5 draws the philosophy of science and economics together.

2. The Slutsky equations 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 given market prices. This hypothesis entails the following theorem, which is due to Slutsky (1915) and was made famous by Hicks (1939): a small ceteris paribus change in price brings about a change in demand that decomposes additively into a *substitution effect* and an *income effect*. The former means the change in demand that would result if the individual's utility were kept at the level reached prior to the price change. The latter can be seen as the change in demand due to the variation in buying possibilities ("real income") accompanying the price change. Think of an increase in the price of bread, every other price and the money income remaining the same. Commonsense suggests that there will be a tendency for other goods to be substituted for the more expensive bread, so that the demand for bread should decrease, but also that this tendency will act more or less powerfully depending on whether, and by how much, the

[1] The collective volume by Chipman et alii (1971) signalled the end of the formative years of consumer theory.

[2] See Chiappori (1990) and Green and Moss (1994).

consumer is impoverished by the new price. After all, he might find that bread is the best commodity to buy, compared with meat or wine, since the price increase leaves him less well-off than he was. The maximization hypothesis warrants this general intuition by delivering a neat theory of how the two possibly conflicting effects add up together.

I now restate this reasoning formally.[3] Denote by $x = (x_1,...,x_l) \in R^l_+$ the various baskets of l goods, by $p = (p_1,...,p_l) \in R^l_{+*}$ the given price vector, and by I the individual consumer's budget.[4] Assuming that this consumer maximizes a utility function $u(x)$ under the constraint $p \cdot x = I$, one derives his *demand function* $x_j(p,I)$ for each good j. Then, one demonstrates that the following ("Slutsky") equations hold for all pairs of goods j, k and all pairs (p,I):

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

Here $\bar{u}(p,I)$ is the maximum utility value reexpressed as a function of the data (p,I) - the *indirect utility function* - and $h_j(p,\bar{u})$ is the demand for j reexpressed as a function of the prices and utility level - the *compensated demand function.*[5] Owing to the Slutsky equations, the effect Δx_j of a small change Δp_k can be decomposed as the sum of two elementary effects:

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

The first term on the left represents the substitution effect, and the second, the income effectTHE EFFECT OF A PRICE CHANGE IS THE SUM OF A SUBSTITUTION EFFECT AND AN INCOME EFFECT. Notice that this additive formula is not exact: the Hicksian decomposition is only a linear approximation for small price changes Δp_k.

We may rewrite the Slutsky equations in matrix terms as

$M^D = M^{SE} + M^{IE}$,

putting:

$$M^D = \left[\frac{\partial x_j(p,I)}{\partial p_k}\right]_{j,k=1,...,l},\ M^{SE} = \left[\frac{\partial h_j(p,\bar{u}(p,I))}{\partial p_k}\right]_{j,k=1,...,l},\ \text{and}$$

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

It is also a consequence of the maximization hypothesis that the substitution effect matrix M^D is negative definite.[6] In contrast, there is no way in which the theory can restrict the income effect matrix M^{IE}.

---

[3] For a fuller exposition, the reader is referred to Mas-Colell, Whinston and Green (1995) or any advanced microeconomics text.

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

[5] By assumption, the $u(x)$ are twice differentiable and satisfy the assumptions for the existence of a unique constrained maximum. Thus, the functions $x_j(p,I)$ and $h_j(p,\bar{u})$ are well-defined and themselves twice differentiable.

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

By putting $j = k$ in the Slutsky equations, one can investigate the effect of an *own* price change:

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

From what has been said about M^{SE} and M^{IE}, it follows that $\frac{\partial h_j(p,\bar{u}(p,I))}{\partial p_j} < 0$, i.e., the own substitution effect is negative, but nothing can be said of $\frac{\partial x_j(p,I)}{\partial I}$, i.e., the direction of the own income effect is indeterminate. For this reason, consumer theory does not recover the *law of demand* found in the older economists: everything else being equal, the demand for a good varies inversely with its price. (I am using here Marshall's restatement with its explicit ceteris paribus clause[7].) If the own income effect goes in the positive direction (i.e., if $-\frac{\partial x_j(p,I)}{\partial I} > 0$) and the magnitude of this effect exceeds that of the substitution effect, the time-honoured law fails. This is but a theoretical possibility, but 20th century economists take it very seriously. "It is only by making additional, and demonstrably arbitrary, assumptions that various writers have been able to derive the so-called law of diminishing demand" (Samuelson, 1947, p. 115, n. 17).

Empirically, the old law is not a robust generality, a fact that was recognized from the early days onwards. Its formulation always included *exceptions*, to be carefully distinguished from the qualifications that the ceteris paribus clause - whatever its exact meaning - aims at capturing. Among the well-recorded empirical exceptions, the so-called *Giffen goods* have the following characteristics: (i) ceteris paribus, if the consumer has a higher income, he demands less of them, and (ii), he devotes to them a large fraction of his current income. The name of these goods comes from the 19th century English civil servant whom Marshall credited for the classic observation: when the price of bread loaves increased, the poor Irish workers in the Midlands factories would typically consume *more* of them. The Slutsky equations account for Giffen's finding because it corresponds to the joint possibility that $\frac{\partial x_j(p,I)}{\partial I}$ be negative (see (i)) and $x_j(p,I) \cdot \Delta p_j$ large in magnitude (see (ii)). To the best of my knowledge, Slutsky (1915) developed his equations independently of the Giffen counterexample; this makes the account more impressive. As to Marshall, he did not know the equations and could only treat Giffen goods as if they constituted an anomaly.[8]

Actually, demand curves with increasing slopes are rarely documented for food, clothing, transportation means, and other ordinary consumer goods. The few findings similar to Giffen's relate to staple consumed among very poor quarters of preindustrial societies. When taken together, conditions (i) and (ii) are stringent, and since the Slutsky equations make them not only sufficient, but also necessary, for the income effect to predominate over the substitution effect, one might credit the equations not only for explaining the Giffen exception, but also for predicting that it will be rare. As Hicks puts it: "Although the law of demand does not necessarily hold in the case of inferior goods [i.e., those with positive income effect], it is in practice likely to hold" (1956, p. 66). Hicks eventually salvages the old law as a decent approximation warranted from the two sides of theory and evidence. "In strictness, the law of

---

[7] See Marshall (1890-1920, III, III). Cournot (1838-1974, p. 85) did not emphasize the ceteris paribus restriction to the same extent.

[8] Compare the analysis of Giffen goods by Marshall (1890-1920, p. 109-110) and Hicks (1939, ch.II). Although aware of the possibility of an income effect, Marshall ends up conflating the uncompensated and compensated demand functions. Slutsky's pathbreaking contribution is analyzed by Chipman and Lenfant (2002).

demand is a hybrid; it has one leg resting on theory, and one on observation. But in this particular instance, the double support appears to be quite exceptionally strong" (ib., p. 59).

Economists have long recognized classes of empirical exceptions other than the Giffen goods. They have always been aware that vintage wines, fancy clothes, or luxurious carriages, might sell better at higher prices. In due course, they observed that a rally on the stock market could attract buyers instead of discouraging them. Further common exceptions relate to capital goods and qualified labour force. Cournot and Marshall simply put these cases aside, but the modern theorists take a more systematic attitude towards them. They try to handle some of them in terms of the Slutsky equations by hypothesizing a strong positive income effect (the empirical reasons for this effect may differ from those prevailing in the Giffen case). For the class of *luxuries*, they sometimes make the "signalling" assumption that on some range of high values, prices enter the utility function directly. For the class of *speculative buying*, they typically introduce a second period in which buyers become sellers. Hicks (1939-1946, p. 36 and 56) sketches the two arguments, which later writers have developed more carefully. Another prominent group of exceptions concerns *factors of production*, which economists simply discard as being out of scope. According to the current categorization, consumer theory deals with demand for final goods alone, and it is the theory of firm which handles the demand for factors of production. It is not easy to summarize the all-considered view of the law of demand among today's economists, but I would argue that *most of them take the law to be empirically acceptable within broad limits*. The law, it is sometimes said, holds true of "ordinary consumer goods". The ill-defined expression suggests that the exceptions either are uncommon (Giffen goods), or are common but unimportant relative to the size of consumer's budget (luxuries and possibly speculative goods), or do not belong to the purview of consumer theory (factors of production).

The evidence on which today's economists base their conviction is remarkably shallow and patchy. Cournot and Marshall may be forgiven for exemplifying the law and its exceptions casually, but their cavalier attitude has persisted virtually up to now. Hicks was again casual, and Samuelson provided no evidence for or against the law. These two economists were exclusively theorists, but the modern gap between theoretical and applied economics is no excuse to the profession because *even applied economists have failed to clarify the empirical case*. In fact, they seem to have lost track of the law of demand almost entirely. They concentrate instead on estimating theoretical relations - primarily Slutsky's - or testing less ambitious empirical claims - for instance *Engel's law*, to the effect that consumers spend on food proportionally less the richer they are. The authoritative summary of econometric evidence on consumer theory by Deaton and Mullbauer (1980) hardly mentions the law of demand, and similarly with Phlips's (1983) and Blundell's (1988) surveys. This is the more curious since the reported results could be used in the testing of the law. For instance, if Engel's law holds, condition (i) for the Giffen exception is met for food. But even this celebrated exception has never be subjected to any systematic study, a lacuna which has led some to doubt the initial finding.[9] Generally speaking, before Hildenbrand's work, I do not know of any sustained attempt to substantiate the claim that the exceptions to the law are empirically rare or quantitatively unimportant.

To complete this background discussion, I should try to resolve a famous semantic ambiguity. Does the law of demand relate to the individual consumer or an aggregate of some sort? Cournot had in mind only the demand to the market, and Marshall was primarily interested in this application, but this does not imply, and should not be taken to mean, that these economists disbelieved that there were an *individual* law of demand. On the contrary, there is definite evidence in Marshall that he 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 of course a different point - the law only requires the

[9] Stigler (1947) casts a doubt on the very existence of Giffen goods.

functions 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). A glance at the passage shows that Hicks discusses the "law of demand" already in connection with the individual consumer (e.g., p. 32). His later work in consumer theory remains generally firm in this respect. For instance, the already quoted sentence that "in strictness, the law of demand is a hybrid, etc" refers to the individual law, not the collective one. I conclude from this quick survey that the influential writers have never called into question that there was a sense, both theoretical and empirical, in placing the law of demand at the individual level.

All 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 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. It may be the case that the former is easier to state and better supported empirically; the latter 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, but some have proposed to reconstruct the household's demand function as the collective (and in part unintended) result of prior interactions between its members. Supposing that the Slutsky equations hold for households in this context, the qualified law of demand they deliver could be said to be genuinely collective, without any individual law underlying it.[10]

Following a less detailed, but more widespread, non-individualistic construal, the law would emerge in roughly its Marshallian form at the *market* level, while being false in this form of any agent in particular. This view has been offered as a commentary on the existing work by Hausman: the law, he writes, is "a generalization about markets, not individuals" (1992, p. 28). Hausman's commentary may cleverly point out the direction of success, but it does not capture the orthodoxy of consumer theory. It cannot be taken to be a *description* of what Marshall and Hicks put into that theory. Hildenbrand shares the view that the law of demand must be reconstructed as a market phenomenon. Before assessing his work, it is important to stress that the law he inherited relates first to the basic decision units, be they construed as households or individuals, and only derivately, to the markets and the economy.

3. Hildenbrand's programme and contribution

In *Market Demand* (1994) and related technical pieces, Hildenbrand distances himself from today's treatment of the law of demand on two scores. First, he complains that consumer theory does not give a sufficient basis (a "justification" in his terminology) for accepting the law; second, as I just pointed out, he denies that the law could be placed at the individual level. I will discuss the two arguments before Hildenbrand's programme properly.

First, the complaint: "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,[11] are entirely grounded on a priori reasoning" (1994, p. 12). When reading this and similar comments, it is important to realize that economists do not use "a priori" in the philosophers' technical acceptation. Over and beyond propositions, such as linguistic truths, which are known to be true or false independently of experience, they mean to include a wide class of *empirical* propositions. These propositions have the distinctive feature that only the *existing* stock of knowledge, whatever its origin, whether it is technically a priori

[10] On the interactive theory of household demand, see Browning and Chiappori (1998).

[11] After Samuelson, economists sometimes take the axioms of Revealed Preference as alternatives to the utility maximization hypothesis; see below.

or not, supports the claims that can be made about their truth or falsehood. In other words, these propositions do not give rise to a specially devised investigation, and in particular, are not subjected to a test. On the interpretation that Hildenbrand is speaking the economists' idiom, his comment would serve to exclude two "justifications" at once, i.e., (i) defences of the law of demand that are a priori in the philosophical sense, for instance linguistically-based arguments (some were influential in the Austrian tradition), and (ii) defences that are based on casual empiricism, like the contemporary economists' (see last section).[12]

There would be no sense in rejecting a priori defences if it turned out that the law were intrinsically untestable, and somewhat surprisingly, Hildenbrand seems to suggest that this may be the case. "The Law of Demand ... does not refer to the actual evolution of prices, but to hypothetical changes within the same period" (1994, p. 5). The *instantaneous* interpretation of the law makes it not only "hypothetical" but even "counterfactual" because it takes time for any real consumer to express his demands as a reaction to a price-income pair, and this allegedly poses a major obstacle to the testing of the law. Hildenbrand's reasoning here strikes me as being questionable at every stage. First, contrary to his suggestion, there are more than one interpretations available for theoretical demand functions. Economists initially think of them *atemporally*, and the instantaneous version - to be carefully distinguished from the atemporal one - is only one way of temporalizing these relations; it is no less acceptable to suppose a lag structure. Second, concrete decisions on the length of the time unit blur the distinction between the instantaneous and lagged interpretations. Economic data on price, income and quantitities demanded are typically recorded for a year or a quarter, much more rarely for a month or a week. On any practical construal, the instantaneous law of demand is no more "counterfactual" and no less testable than is the lagged law.

Hildenbrand's final objective is to subject the law of demand to a test, so he cannot mean to say that the law is untestable. What he wants to convey is the more limited point that the law should be tested *on cross-sectional data, not on time series*. But to support this methodological claim, pragmatic reasons are more appropriate than the dubious semantic reasons just said. Although time series on households' consumption are becoming increasingly available, the bulk of today's evidence is still made out of cross-sectional data gathered for the national statistics. Even more importantly, only cross-sectional data exhibit the amount of variation in prices and incomes without which a test would not be probing (Hildenbrand mentions this argument at some point). Last but not least, these data seem to agree better than the others with the objective of testing propositions which involve heavy ceteris paribus clauses.

I now move to Hildenbrand's second disagreement with today's conception of the law of demand. He strongly believes that if this proposition 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 housholds, for example to all private housholds in Germany or the United Kingdom" (1994, p. 3-4). In view of what last section said, this claim means a radical departure from consumer theory. So it comes as a surprise that Hildenbrand finds a supportive passage in Hicks: "The market *I* [income effect] is the sum of the individual *I*'s... For the market *I* 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). The passage is striking indeed, but even within the chapter it is extracted from, it remains exceptional, and as Hildenbrand himself notes, it means a gesture without serious technical consequence.[13]

---

[12] This analysis of the economic a priori draws on Mongin (2004).

[13] See Hildenbrand (1988, p. 258-259).

Now to the programme more explicitly stated. It preserves the conventional distinction between a negatively signed substitution effect and a nondescript income effect. Elaborating on Hicks's glimpse, 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. Assuming that all consumers have the same utility function and that the income distribution follows a uniform law over $[0,1]$, one concludes that the average income effect is non-positive:

$$-\int_0^1 x_j(p,I).\frac{\partial x_j(p,I)}{\partial I}.dI = -\frac{1}{2}\int_0^1 \frac{\partial x_j^2(p,I)}{\partial I}.dI$$

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

Of course, in any relevant community, utility functions will differ across individuals, and the statistical distribution of income is unlikely to be skewed (see Hildenbrand, 1994, p. 63, for an empirical example). But the irrealistic case disposes of the *theoretical* preconception that to move the law from the individual up to the aggregate level can only mean further trouble. Given that the Slutsky equations disprove the law at the individual level, there seeemed to be little hope to restore it by taking income distribution into account. One would have expected the income effect to become large while remaining of indeterminate direction. The simple calculation demonstrates that this may not the case. The challenge becomes to explore the class of statistical distributions that deliver either a negative average income effect or a positive average income effect that is smaller in magnitude than the negative average substitution effect. This is a theoretical project, 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.

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 leads him to introduce the condition of Increasing Spread of Household Demand, on which I will have much to say shortly. This more sophisticated approach leaves Hildenbrand within the confines of his programme. The guiding idea is again that the average demand can be well-behaved if problematic income effects are appropriately rare. Importantly, Hildenbrand does not consider the possibility that the average income effect may be of the wrong sign, though small compared with the average substitution effect. Like Hicks in the previous passage, and unlike Hicks in his work elsewhere, Hildenbrand is exclusively concerned with the possibility that the former turn out to be of the right sign.

Hildenbrand's target is not the Marshallian law, which is restricted to one good at a time, but at a more powerful version due to Hicks, which takes account of all goods at a time. Suppose that $p = (p_1, ..., p_l)$ et $q = (q_1, ..., q_l)$ are two price vectors, and $X(p) = (x_1(p), ..., x_l(p))$ et $X(q) = (x_1(q), ..., x_l(q))$ are the corresponding demand vectors for a household. (Hildenbrand does not analyze consuming units, but prefers to say "household" than "individual" because empirical data typically relate to households.) Hicks's *generalized law of demand* states that:

(**) $(p - q) \cdot (X(p) - X(q)) < 0$.

If the price change takes place in j alone, all other prices remaining the same price, one gets the Marshallian law as a particular case:

$(p_j - q_j) \cdot (x_j(q_1, ..., p_j, ..., q_l) - x_j(q_1, ..., q_j, ..., q_l)) < 0$.

With the generalized law, several prices may change at a time, and these changes may even be of opposite signs. This alleviates the heavy ceteris paribus clause of the Marshallian law; now, only the household's income has to be fixed. Geometrically, inequation (**) says that the two vectors of changes in prices and quantities point in opposite directions of the *l*-dimensional commodity space.[14]

The price changes considered in (**) are discrete but the generalized law can be restated differentially to facilitate comparison with the Slutsky equations (*). It is equivalent to require that the Jacobian matrix

$$M^D = \left[\frac{\partial x_j(p, I)}{\partial p_k} \right]_{j,k=1,...,l}$$

be negative definite. Notice that this property implies the Marshallian law in differential form: $\frac{\partial x_j(p, I)}{\partial p_j} < 0, j = 1, ..., l$.

In view of the restatement, consumer theory provides a sufficient condition for the generalized law of demand to hold. Returning to the matrix form of (*), $M^D = M^{SE} + M^{IE}$, we know that M^{SE} is negative definite. Hence, it is sufficient for the generalized law to hold that the other term M^{IE} be negative definite.

Conveniently, both the generalized law of demand and the Slutsky equations keep the same mathematical form when they relate to household demand functions or their *average* or *total* aggregate. Hildenbrand's analysis goes in terms of average uncompensated and compensated demand functions, respectively:

$$x_j(p) = \frac{1}{|P|} \sum_{i \in P} x_j{}^i(p, I^i) \text{ and } h_j(p, \bar{u}(p, I)) = \frac{1}{|P|} \sum_{i \in P} h_j{}^i(p, \bar{u}(p, I^i)),$$

where x^i and h_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 in the economy, and $|P|$ for their number. We may now reinterpret equations (*) and (**) as bearing on average substitution and income effects, and the generalized law of average demand, respectively. The latter constitutes Hildenbrand's mathematical definition of the *law of market demand*.

Hildenbrand's theoretical strategy is to fulfill the sufficient condition just said, i.e., to derive the following condition.

Negative Average Income Effect: the matrix M^{IE} of average income effects is negative semi-definite.[15]

Thusfar, I have only mentioned already known material. What comes next contains Hildenbrand's genuine contribution. He demonstrates that Negative Average Income Effect is equivalent to a more interpretable property.

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 technical and somewhat unusual measure of statistical diversity, which takes into account not only the dispersion around the mean, but also the distance to the origin. I just give a coarse intuition for the condition. If one visualizes a household's demand vector as a point in the commodity space, the condition says that the set of points determined by the new

---

[14] The generalized law of demand is stated in (**) for *uncompensated* demand functions $x_j(p)$ in Hicks (1956, p. 139). An earlier version involved *compensated* demand functions instead (1939-1946, p. 52).

[15] To deal with a negative semi-definite, instead of a 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.

incomes $I^i + \Delta$ will look be more scattered, relative to the null demand point, than the set determined by the initial incomes I^i. With this ingenious restatement of the Negativity condition, Hildenbrand makes a step towards concreteness. However, he believes that he has not yet reached the *testability* stage. In order to test Increasing Spread, one would have to submit all households to simultaneous income changes by an equal amount, and he argues that this is more of the nature of an hypothetical experiment than of an effective test (see 1994, p. 22). Even abstracting from the simultaneity problem, to base a test on Increasing Spread would run into the usual difficulty of the ceteris paribus clause: prices may change at the same time as incomes.[16]

Hildenbrand's testability problem would be resolved if he could substitute the time changes in income and demand experienced by *the same* individuals with external comparisons between incomes and demands of *different* individuals. This would be an improvement because cross-sectional data, like *Family Expenditure Surveys* (UK) or *Enquête Budget de Famille* (France), would become the relevant evidence instead of time series, and I have explained that the former are more commendable than the latter. This replacement heuristic motivates Hildenbrand's next mathematical step. Given a sufficient condition to be discussed shortly, he 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 P population 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 $\{x^i(p, I^i)\}_{i \in P(I+\Delta)}$ are on average more spread than the vector families $\{x^i(p, I^i)\}_{i \in P(I)}$.

This condition makes sense only if the population P is infinite (otherwise, the $P(I + \Delta)$ group may not belong to P). It would be more transparent to require that $\{x^i(p, I^i)\}_{i \in P(I+\Delta)}$ is more spread than $\{x^i(p, I^i)\}_{i \in P(I)}$ not on average on the values of I and $I + \Delta$, but for each such pair individually. However, this much stronger statement is unnecessary for the purpose of deriving Increasing Spread of Household Demand.[17]

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 problematic joint increase Δ in income, it is enough to compare $P(I)$ with the other income group $P(I + \Delta)$. Such a comparison can be performed empirically.

Here is a sufficient condition that makes it possible to derive the first version of Increasing Spread from the second.

Metonymy: those variables other than income which influence demand are probabilistically independent of income.

I sketch a crude argument to the effect that Metonymy and Average Increasing Spread of Conditional Demand imply Increasing Spread of Household Demand. If income is not correlated with the other relevant variables, the effect of income on demand can be appraised independently, in a probabilistic sense, of the effect on demand of those variables. Hence an income variation from I to $I + \Delta$ should bring about the same change in demand, whether it is incurred by the same $P(I)$ or relative to $P(I)$ and $P(I + \Delta)$. If the results differed in a significant way, this would be due to some personal characteristic of the household, say, age or

[16] This is one of the recurring difficulties in empirical studies; see Deaton and Mullbauer (1980).

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

location, which would be correlated with income, contrary to the Metonymy assumption. I have selected Hildenbrand's strongest sufficient condition for his sought for derivation. Technically, it is sufficient that a property like Metonymy be satisfied locally and on average (1988, p. 261-262 and 1994, p. 153).

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 when corresponding cross-sectional data are; for another, even if both are available, the former is often thought to be less reliable than the latter. There is a choice to be made between two disputable assumptions - for one, that the relations governing the behaviour of the same individuals are constant through time, and for another, that identical relations govern the behaviour of different individuals at any point of time. The balance of advantage and disadvantage of each assumption depends on the particular problem and the quality of data, and econometricians sometimes prefer the strategy opposite to Hildenbrand's, i.e., replace synchronic by diachronic information. The ideal case is when both sets of data are available and appear to be equally reliable. Then, it becomes possible to relate them to each other.[18]

This paper is not about econometrics, and I will not comment on the tests performed by Hildenbrand and his collaborators[19]. Suffice it to say that Average Increasing Spread of Conditional Demand is the only condition that *Market Demand* submits to a test, and that he finds it to be "well supported by the cross-section data" (1994, p. 27). He concludes that this finding gives support to ("justifies") the Law of Market Demand. The support that Hildenbrand means is a good deal theoretical because he has attempted to test neither the Hicksian preliminaries, nor his Metonymy condition. However, this support must also in part be 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 or, at least, are in principle falsifiable. 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).

Having isolated an important claim about empirical confirmation, I will inquire how existing philosophical theories can rationalize it. This analysis is carried out in section 5. The next section spells out what these confirmation theories are.

4. An aside on confirmation theory

Very roughly speaking, there are three groups of confirmation theories in current use and discussion. The first and perhaps best established group takes an hypothesis to be confirmed by some piece of evidence if the latter provides a *positive instance* of the former. Paradigmatic in this group is Hempel's (1965) theory of the confirmation relation holding between sentences *H* and *E*, where *H* refers to an hypothesis and *E* to an evidential report. By definition, the direct confirmation relation holds if one can deduce from *E* what *H* asserts for the class *I* of logical individuals mentioned in *E* - in Hempel's jargon, if one can deduce from *E* the *development of H for I*. Thus, if *H* is "For all *x*, *F(x)&G(x)*", then "*F(a)&G(a)*" directly confirms *H*, while "*notF(a)&G(a)*", "*F(a)&notG(a)*" and "*notF(a)&notG(a)*" do not. This definition does not recover the commonly shared intuition that "*F(a)&G(a)*" confirms the universal conditional "For all *x*, if *F(x)*, then *G(x)*". This and related arguments led Hempel to state his final concept of the confirmation relation as follows:

---

[18] The typical manoeuvre is to estimate the parameters of the theoretical relations on one set and test the resulting hypothesis on the other. Friedman (1957) used this method in his pioneering study of consumption.

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

E confirms *H* if *E* directly confirms every member of a class of sentences *K* such that *K* entails *E*.
Hempel's disconfirmation relation is derivative:
E disconfirms *H* if *E* confirms *notH*.

As one would expect from a confirmation theory, Hempel's makes *verification* and *refutation* limiting cases of confirmation and disconfirmation, respectively. By definition, *E* verifies *H* if the latter can be deduced from the former, and *E* refutes *H* if the two sentences are logically contradictory. Importantly, the development of *H* is restricted to *finite* classes of logical individuals *I* (1965, p. 36). This reflects the presumption that evidential reports are also *observational* reports, and as such, can only cover a finite number of individuals at a time. With this finiteness restriction, universal hypotheses interpreted over infinite domains are automatically inverifiable, althought they are confirmable in Hempel's sense. It was very important for him to substantiate this possibility.

Despite its relative sophistication, Hempel's definitions are open to well-recognized paradoxes, difficulties, or limitations, among which I single out two dubious assumptions. For one, *H* and *E* are phrased in the same observational vocabulary; for another, they are related directly, i.e., without any intervening auxiliary assumption. In the most interesting scientific cases, *H* represents a theoretical hypothesis, i.e., a claim made about nonobservable entities, properties, or states of affairs, no less than about observable ones. Furthermore, the confirmation problem typically 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 on the logical individuals mentioned in *E*. Starting from these two critical points, Glymour (1980) has reelaborated the theory of confirmation by positive instances. Essentially, he redefines confirmation as being a trinary instead of a binary relation. It is supposed to hold between an observation report *E*, a sentence *H* representing the target hypothesis, and another sentence *H'* collecting the assumptions needed to make *E* logically and semantically relevant to *H*. Glymour's *bootstrapping* construction is subtle and controversial (see the revision in Glymour, 1983). I need not to expand on it here and will simply stress that it is a natural development of Hempel's definition, which itself proceeds from the commonsensical view that to confirm a statement is to exemplify it.

The second group of confirmation theories takes an hypothesis to be confirmed by some piece of evidence if the latter does not deliver a *contradiction* with, and typically a *counterinstance* to, the former. The main representative is of course Popper (1963, 1972), who has repeatedly argued that positive confirmation, in Hempel's or any related sense, was worthless for the scientifically most significant cases. Popper does not deny that there are some scientifically relevant sentences that can be verified, hence confirmed in Hempelian sense. He argues that this is the case for existential sentences stated in the observational language ("There is a sea-serpent"), and at least in one of his accounts, that this also holds of basic statements, which are the counterpart of elementary observations in his theory ("There is a black raven at space-time coordinates (*s*,*t*)"). However, as the familiar story goes, confirmation is unavailable to the universal sentences of science representing putative laws of nature. Given their infinite domain of variation and other relevant features, these sentences cannot be verified, *and they cannot even be confirmed in any satisfactory sense*. The italicized addition brings Popper's falsificationism in sharp focus. Allegedly, the only sense in which observational evidence can be relevant to the confirmation of a putative law is by allowing basic statements to refute the universal sentence representing this law. A well-confirmed law, or in Popper's preferred terminology, a well-*corroborated* law, is one which has survived many attempts at falsifying it. Generally, the corroboration record of a sentence is the list of all cases where the sentence was

related to basic statements having a potential for refutation. The less effective refutations there are in the record, the higher the qualitative degree of corroboration of the sentence.[20]

When one translates the counterinstance view of confirmation into logical terms, it turns out to be difficult to separate from the positive instance view which it opposes vehemently. Take the putative law that all ravens are black, and assume that observation decides of each member of a given population whether or not it is a raven, and whether it is black or white. Logically, we compare "For all *x*, if *F(x)*, then *G(x)*", and the four possible reports "*F(a)&G(a)*", "*notF(a)&G(a)*", "*notF(a)&notG(a)*", and "*F(a)&notG(a)*". Only the last sentence qualifies as a potential falsifier. However, neither Popper nor anybody would claim that the second and third sentences correspond to a genuine test - one does not test a statement about ravens by considering pigs. There remains only the first sentence to match Popper's notion of a failed attempt at refutation, and embarrassingly, it also matches Hempel's notion of a confirmatory instance. It would be hasty to conclude from this and related arguments that the positive confirmationist and the corroborationist accounts collapse into each other. But it is a fact that neither Popper nor his followers have found a satisfactory logical expression of the latter account that would identify it clearly, let alone make it defensible. For lack of a better solution, I propose below that the two accounts be demarcated in terms of Hempel's abstract requirements on the confirmation concept.

The first two groups of theories have in common their search for a *qualitative* notion of confirmation based on the analysis of the *logical* relations holding between representative sentences. These features differentiate them sharply from probabilistic theories of confirmation. In this third group, the objective is to provide a numerical measure of the *degree of support* that *E* confers on *H*, and to do so, the analysis exploits not only the logical relations, but also probabilistic relations that hold, or are supposed to hold, between sentences. Besides, there is a general shift of emphasis towards *dynamical* statements - the typical conclusion of a probabilistic theory being that *E* increases, or decreases, the degree of support of *H* by such-and-such. The archetype in the third group is of course Bayesian confirmation theory, which derives its dynamical conclusions from repeated applications of Bayes's rule for updating probabilities. Howson and Urbach (1993) illustrate how this extremely simple tool helps handle complicated cases. These writers, as well as Jeffrey (1975), have explained how Bayesian reasoning recovers a number of informal ideas from commonsense or the earlier philosophy about confirmation and disconfirmation.

In terms of the previous distinction between positive instance and counterinstance theories, Bayesianism is definitively on the former side against the latter. This explains why Popper and his school have amalgated Hempelian with Bayesian theories,[21] despite the case made by some Hempelians, like Glymour (1980, ch. III), against Bayesianism. I privilege the classification in terms of tools employed - logic versus probability - above the other for reasons that are not unlike Glymour's. Despite the attractions of probabilistic theories, they run into the problem that the probabilistic relations they need for their conclusions rarely belong to the subject matter they investigate. More often than not, it is *the philosophical observer* who superimpose probabilitic relations - with their specific numerical figures - on the logical or pseudo-logical relations that scientists only envisage. The Bayesians are especially exposed to this general criticism since their conclusions depend not only on likelihood values, which may be questionable, but also on prior probability values, and the latter are notoriously difficult to attribute non-arbitrarily.

---

[20] By "degree of corroboration", falsificationists often mean a *numerical* measure, but they have never managed to explain what it was. Popper's (1972, Appendix ix) metric enters an ad hominem argument and does amount to a final proposal. Watkins (1984) has pursued the issue no more successfully.

[21] Under the misleading expression of "verificationism".

Admittedly, logical theories also involve a significant element of reconstruction. Even the best scientists are often fuzzy about the logical interrelations holding between statements of their theories, and when they say that *A* implies *B*, or *B* follows from *A*, they do not necessarily mean that one goes from *A* to *B* by a *deductive* step. They often have other, possibly ill-defined, notions of inference in mind. Even granting this, I believe that probabilistic theories of confirmation are two steps remote from the scientific subject matter, whereas logical theories are only one step remote. In the particular application, the neat mathematics of consumer theory makes logical theories, like Hempel's and Popper's, more relevant than probabilistic theories, and I will dispense with the latter altogether.

In order to clarify the disagreement between the remaining contenders, I briefly recast Hempel's (1965, ch. 1) classic list of prima facie conditions for the confirmation relation *C(E,H)*. The symbols $A \triangleright B$ and $A \triangleleft\triangleright B$ indicate that *B* follows deductively from *A*, and that *A* and *B* are deductively equivalent, respectively.

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

In other words, verification is a particular case of confirmation.. This is a nearly definitional, and as such, a desirable property.

Consistency Condition: If *C(E,H)* and *C(E,H)*, then *H* and *H'* are logically consistent.

In other words, one and the same piece of evidence cannot confirm two inconsistent theories simultaneously. This is a heavy requirement, which may be desirable abstractly but does not have to be imposed in all and every circumstances.

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, confirmation "descends" along the way of deductive inference; according to the second, it "ascends" by reversing the direction. Any of these two conditions implies the third, which is by itself a source of famous paradoxes I leave out entirely.[22] A theory limited to this condition would assert very little. To accept both Special Consequence and Converse Consequence is to trivialize the confirmation relation, as the following example shows. Think of two 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 logical theories of confirmation appears to select one of these two conditions appropriately.

It is easy to check that Hempel's confirmation theory satisfies all the conditions except for Converse Consequence. I do not think that Popper or his followers have ever stated their views in terms of Hempel's conditions and would like to do so, but it appears that they have no choice but to accept the Converse Consequence and reject the Special Consequence. If what makes *E* confirmatory is that it fails to contradict *H*, the same argument applies to *E* and any *H'* deductively entailing *H*. Converse Consequence is warranted by modus tollens, which falsificationists say captures the logic of a test. They cannot accept the Special Consequence on pain of trivializing corroboration, and anyhow, there appear to be no good arguments within their position to accept it. If I am correct in mapping each group of theories onto one of the conditions, then these theories can be distinguished logically after all. In the next section I use this criterion to separate what in Hildenbrand's work agrees with one side and what agrees with the other.

5. A neo-Hempelian analysis of Hildenbrand's demand theory

One may summarize Hildenbrand's theory as follows:

Optimizing Hypothesis (household)

[22] Hempel (1965) takes the view that these paradoxes are only psychological illusions and that any rational theory of confirmation has to accept the Equivalence Condition.

⇓

Hicksian Decomposition (households) & Negative Substitution Effect (households)

⇓

Hicksian Decomposition (average) & Negative Substitution Effect (average)

with Increasing Spread of Houshold Demand

or Metonymy & Average Increasing Spread of Conditional Demand

⇓ ↓ ↑

Law of Market Demand ↑

---------- CROSS-SECTIONAL MARKET DATA ----------

The ⇓ and ↓or ↑arrows represent the deductive implications of the theory and the suggested direction of confirmation, respectively. In this scheme confirmation by market data bears on Average Increasing Spread of Conditional Demand, and it is then channelled by deductive implication from this proposition conjoined with others to the Law of Market Demand. The other premisses are, for one, straight borrowings from consumer theory, and for another, Metonymy, without which the Law would not be confirmed. The salient point is that confirmation is descending, not ascending. Hildenbrand is implicitly a Hempelian, not a Popperian.

Hempel's theory is not a quantitative one, but it is compatible with 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 comparative sense does the Law of Market Demand receive from Hildenbrand's theory and empirical tests? Not very much, it would seem, because among the premisses of the Law, only Average Increasing Spread is subjected to a test. The Hicksian component is certainly worrying, given the mediocre empirical record of the so-called *Slutsky conditions*, among which is the negative definiteness property of the substitution effect matrix M^{SE}. However, it must be said in fairness that the other two Slutsky conditions - which are the symmetry of M^{SE} and the homogeneity of degree 0 of the uncompensated demand functions $x_j(p, I)$ - are the main target of econometric criticism (see, e.g., Deaton and Mullbauer, 1980, p. 80). The trouble created by negative definiteness is not that it is known to be empirically shaky, but that it has not been subjected to independent tests. A difficulty may be that the standard tests for negative definiteness are devised fo *symmetric* matrices; that is to say, they tackle two Slutsky conditions at a time.[23]

As a matter of theory, Hildenbrand does not insist on Individual Optimization (1994, p. 18). He considers replacing it with a weaker hypothesis that is sufficient to secure a negative substitution effect: Samuelson's *weak revealed preference axiom*.[24] This move is coherent with a general emphasis on the law of market demand as a collective phenomenon: it makes sense to minimize the assumptions on individual rationality. However, on the empirical side, the replacement leads to disappointment. Samuelson's axiom turns out to be more easily testable than infinitesimal conditions, and the tests made of it are rarely considered to very favourable. Because it implies that the $x_j(p, I)$ are homogeneous of degree 0, the fact that this Slutsky condition is not well-supported by consumption data is worrying.

---

[23] Mathematically, these tests amounts to checking the signs of the eigenvalues of the matrix. But it is only for symmetric matrices that these signs are determined to be negative.

[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

$p.x^i(p', I'^i) \leq I \Rightarrow p'.x^i(p, I^i) \geq I'^i$.

From *Market Demand*, it is not clear whether Hildenbrand does not test Metonymy because he is sufficiently busy elsewhere, or because he regards it as untestable (see 1994, p. 133, where he just says "the untested metonymy condition"). In previous work, Hildenbrand (1988, p. 266-267) seems to have considered Metonymy as a constraint put on the data in order to carry his estimation procedures; this does not help answer the question.[25] The simple form of the sufficient condition, which says that non-income determinants of demand are probabilistically independent of income, seems amenable to an equally simple test: select different levels of income, and for each such level, estimate the marginal distributions of demand on relevant non-income information. If, say, the estimated marginal on age turns out to vary systematically with income level, the condition would be refuted. However, the test is not telling because it ignores the ceteris paribus clause implied in the probabilistic independence assumption. Also, Hildenbrand has made it clear that he did not need the full force of this assumption. The actual form he gives Metonymy, which I did not restate here, makes it less demanding than it first seems, but also problematic to test.

There are some reasons to treat this condition as *metaphysical*, in a sense which I have tried to clarify and apply to economics broadly. A proposition in a scientific theory counts as 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 easier to achieve if the proposition appears nowhere as a conclusion). Metonymy fulfills the first two properties and arguably meets the third. Another - actually easier - example of a proposition that I treat as metaphysical in the same triple sense is the "common prior assumption", which recurs in game theory as well as many social sciences applications.

I wrote that Hildenbrand was *implicity* a Hempelian, and one may wonder whether he would accept this judgment, given his proclaimed interest in *falsifiability*. Sometimes he emphasizes what I described as his main objective - to investigate the confirmation of the law of demand - but sometimes also, he prefers to lay the stress on another objective - to put forward testable propositions, *in the potential falsification sense*. "To go from here [the Weak Axiom] to the Law of Demand, we should use on 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 Increasing Spread (first version) is that it is not "falsifiable", and the reason he gives to retain Increasing Spread (second version) is that, by contrast, it is "falsifiable" (1994, p. 22 and 26). From this textual evidence, one may be tempted to conclude that there is an unresolved tension in Hildenbrand between his Hempelianism and a lingering adherence to Popperianism. However, this derogatory conclusion relies on too crude a contrast between the two groups of theories. Hempel does not insist on refutability in the confirmation context, but Glymour (1980, p. 130-131, condition iv) does: he makes it a precondition for his bootstrapping confirmation. Since both Hempel and Glymour obtain the Special Consequence Condition by stipulating it within their final definition of the confirmation relation *C(E,H)*, one may clarify the rôle of refutability thus: it is a precondition of *direct* confirmation though not of confirmation in general. As a paradigmatic case, a proposition for which no potential falsifier has been adduced can be confirmed nonetheless, if it is deduced from propositions one of which is directly confirmed, hence also refutable. This case seems to fit Hildenbrand's work reasonably well. Supportive (confirming?) evidence can be found in the fact that he investigates the refutability or otherwise of his premisses but says near to nothing *on the refutability of the law of demand itself*. Had he been a lingering Popperian, he would have raised this question before any other.

[25] Hildenbrand's 1988 estimations directly bear on the sign property of the income effect matrix.

I may now atttempt to reverse the order of the argument, and instead of rationalizing the economist's work in terms of confirmation theories, use the former to make a *normative* point about the latter. A weakness of Hempelian theories shows up plainly here: if one does not insist on confirming *all* the premisses, either directly or indirectly, it may be too easy to confirm the conclusion. For any theory of sufficient complexity, like consumer theory and its variants, there is a large number of conceivable derivations for any target proposition, like the law of demand; it is enough to find *one* derivation in which *one* premisse is well supported. The corresponding weakness on the Popperian side is that one will attribute corroboration too liberally not to the consequences, but to the premisses.

The case also suggests that Hempelian theories may be closer than Popperian theories to actual scientific practice. What is exemplary in Hildenbrand is the search for *sufficient* conditions, not for necessary conditions or equivalences, when it comes to replacing a troublesome proposition like Increasing Spread. If scientists cannot test *H* by *E*, then they will more naturally look for evidence *E'* bearing on *H'* implying *H* than for evidence *E"* bearing on *H"* implied by *H*. That is to say, they appear to take Hempel's Special Consequence Condition more seriously than Popper's Converse Consequence Condition.

REFERENCES

Blundell, R. (1988), "Consumer Behaviour: Theory and Empirical Evidence – A Survey", *Economic Journal*, 98, p. 16-65.

Browning, M. et P.A. Chiappori (1998), "Efficient Intra-Household Allocations: A General Characterization and Empirical Tests", *Econometrica*, 66, p. 1241-1278.

Chiappori, P.A. (1990), "La théorie du consommateur est-elle réfutable?", *Revue économique*, 41, p. 1001-1025.

Chipman, J.S, L. Hurwicz, M.K. Richter, et H.F. Sonnenschein (sous la dir. de) (1971), *Preferences, Utility and Demand*, New York, Harcourt Brace Jovanovich.

Chipman, J.S. et J.S. Lenfant (2002), "Slutsky's 1915 Article : How It Came to be Found and Interpreted", *History of Political Economy*, 34, p. 553-597.

Cournot (1838), *Recherches sur les principes mathématiques de la théorie des richesses* (reprint, Paris, Calmann-Lévy, 1974).

Deaton, A. et D. Muellbauer (1980), *Economics and Consumer Behavior*, Cambridge, Cambridge University Press.

Friedman, M. (1957), *A Theory of the Consumption Function*, Princeton, Princeton University Press.

Glymour, C. (1980), *Theory and Evidence*, Princeton, Princeton Univerity Press.

Glymour, C. (1983), "Revisions of Bootstrap Testing", *Philosophy of Science*, 50, p. 626-629.

Green, E.J. et K.A. Moss (1994), "The Reorientation of Neoclassical Consumer Theory", in T. Horowitz et A.I. Janis, *Scientific Failure*, Center for Philosophy of Science Publications, Rowman and Littlefield, p. 49-78.

Härdle, W., W. Hildenbrand and M. Jerison (1991), "Empirical Evidence on the Law of Demand", *Econometrica*, p. 1525-1549.

Hausman, D.M. (1992), *The Separate and Inexact Science of Economics*, Cambridge, Cambridge University Press.

Hempel, C.G. (1965), *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*, New York, The Free Press.

Hildenbrand, W. (1983), "On the "Law of Demand"", *Econometrica*, 51, p. 997-1019.

Hildenbrand, W.(1989), "Facts and Ideas in Microeconomic Theory", *European Economic Review*, 33, p.251-276.

Hildenbrand, W. (1994), *Market Demand*, Princeton, Princeton University Press.

Hicks, J.R. (1939), *Value and Capital*, Oxford, Clarendon Press (2nd ed. 1946).

Hicks, J.R. (1956), *A Revision of Demand Theory*, Oxford, Oxford University Press.
Howson, C. et P. Urbach (1993), *Scientific Reasoning. The Bayesian Approach*, Chicago, Open Court (1st ed. 1989).
Jeffrey, R. (1975), "Probability and Falsification: Critique of the Popper Program", *Synthese*, 30, p. 95-117
Marshall, A. (1890), *Principles of Economics*, London, MacMillan (8th ed., 1920).
Mas-Colell, A., M.D. Whinston, and J. Green (1995), *Microeconomic Theory*, New York, Oxford University Press.
Mongin, P. (2004), "L'a priori et l'a posteriori en économie", mimeo, Laboratoire d'économétrie, CNRS & Ecole Polytechnique, Paris
Phlips, L. (1983), *Applied Consumption Analysis*, Amsterdam, North Holland.
Popper, K.R. (1972), *The Logic of Scientific Discovery*, Londres, 1972 (1st German ed. 1935).
Popper, K.R. (1963), *Conjectures and Refutations*, London, Routledge.
Samuelson, P.A. (1947), *The Foundations of Economic Analysis*, Cambridge (Mass.), Harvard University Press.
Slutsky, E. (1915) "Sulla teoria del bilancio del consumatore", *Giornale degli economisti e rivista di statistica*, 51, p.1-26.
Stigler, G. (1947), "Notes on the History of the Giffen Paradox", *Journal of Political Economy*, 55, p. 152-156.
Watkins, J.W.N. (1984), *Science and Scepticism*, Princeton, Princeton University Press.