The Economic World View

Studies in the Ontology of Economics

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For KEKLU

12 Is macroeconomics for real?

KEVIN D. HOOVER*

> All are keeping a sharp look-out in front, but none suspects that the danger may be creeping up from behind. This shows how real the island was.
>
> J. M. Barrie, *Peter Pan*

Children are often thought to be peculiarly honest – witness the story of "The Emperor's New Clothes." My title comes from a group of my academic children: first-year graduate students. I teach a mandatory class in macroeconomic theory to graduate students in both an economics department and an agricultural economics department. The students in agricultural economics are typically more interested in crop patterns or natural resources – relentlessly microeconomic topics – than in unemployment, GDP growth, or interest rates. Each year at least one student, who I assume comes from the agricultural economics department, writes on the anonymous class evaluation something like this: "If macroeconomics were real economics – which it is not! – this would have been a good class." What is one to say to the honest and piercing doubts of an academic child?

The idea that macroeconomics stands in need of a microfoundational base is a commonplace among economists. I shall argue that what motivates this belief are principally ontological concerns, naïvely, but pointedly expressed, in my students' questions about the reality of macroeconomics. I shall argue that ontological reduction of macroeconomics to microeconomics is untenable. Thus, while the program of microfoundations may illuminate macroeconomics in various ways, it cannot succeed in its goal of replacing macroeconomics.

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To begin at the beginning, it might help to define the key terms. "Macroeconomics" is sometimes thought to be the economics of broad aggregates, and "microeconomics" the economics of individual economic actions. Although he did not use the terms "microeconomics" and "macroeconomics," John Maynard Keynes (1936, pp. 292–3) drew a related distinction: microeconomics is the theory of the individual industry or firm; macroeconomics is the theory of output and employment as a whole. As Maarten Janssen (1993, ch. 1) shows, these alternative definitions cut in somewhat different ways. They are, however, similar enough for present purposes, since any quantification of output and employment *as a whole*, is bound to involve broad aggregates. Macroeconomics is thus that area of economics that treats of GDP, unemployment, interest rates, the flow of financial resources, exchange rates, and so forth.

Uskali Mäki (1996) offers a careful discussion of realism in relation to economics that might help to define the term "real" in the title. Mäki distinguishes between ontological realism, which raises questions about what there is, and semantic realism, which raises questions about the connection between language and what there is. Semantic realism can be analyzed further, but the central claim of this essay is ontological: *macroeconomic aggregates exist*. More precisely, and again using Mäki's terminology, I will argue that macroeconomic aggregates exist *externally* (i.e., independently of any *individual* human mind) and *objectively* (i.e., unconstituted by the representations of macroeconomic theory). This claim, if it can be sustained, undermines one of the central rationales for the program of microfoundations for macroeconomics that since at least the mid-1940s has dominated thinking among economists.

1 The program of microfoundations

Before this century, the most common definition of economics was epitomized by Alfred Marshall (1920, p. 1): "[A] study of mankind in the ordinary business of life; [economics] examines that part of individual and social action which is most closely connected with the attainment and with the use of the material requisites of well-being." This definition, which is reasonably hospitable to macroeconomics, has now largely been supplanted by that of Lionel Robbins (1935, p. 16): "Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses." On Robbins's definition, economics must be fundamentally about the individual.

Modern macroeconomics developed in the wake of Keynes's *General Theory of Employment Interest and Money* (1936). Typical elements of Keynes's analysis were the consumption function, which related aggregate consumption to aggregate national income, the investment function, which related aggregate investment to the general rate of interest, and the liquidity preference function, which related the aggregate stock of money to aggregate national income and the general rate of interest. It is easy to understand that in a profession committed to Robbins's definition of economics, such aggregate relationships were at best rather unappealing way-stations on the path to an individualist economics. The program of microfoundations, as it has developed over the past fifty years, aims to explain all macroeconomic properties of the economy – *in principle*, at least – by reference to the behavior of rational economic actors such as postulated by microeconomics.[1]

Approval of the program of microfoundations is almost universal among economists. Those economists who have reflected on the matter at all deeply typically associate microfoundations with methodological individualism (e.g., Janssen 1993, pp. 26–9 *passim*; Boland 1979, chs. 2 and 5).[2] Blaug (1992, p. 44) defines *methodological individualism* as the principle that ". . . asserts that explanations of social, political, or economic phenomena can only be regarded as adequate if they run in terms of the beliefs, attitudes, and decisions of individuals."

Methodological individualism is a doctrine about explanation. Despite lip-service to it, it is not widely practiced by economists. The reason is what I have elsewhere labeled the "Cournot problem" after its lucid, early formulation by Augustine Cournot (1838/1927, p. 127), the nineteenth-century mathematician and economist: there are too many individuals (firms and consumers) and too many goods to be handled by direct modeling.[3] Mark Blaug (1992, p. 46) observes that few explanations of macroeconomic phenomena have been successfully reduced to their microfoundations, so that insistence on microfoundations would eliminate explanations of macroeconomic phenomena *tout court*. Even Lucas (1987, pp. 107–8), an important advocate of the program of microfoundations, holds up only the *hope* of the elimination of distinction between microeconomics and macroeconomics.

The commitment of economists to methodological individualism is

[1] See Weintraub (1979) and Janssen (1993) for general discussions of microfoundations for macroeconomics.

[2] I must be careful not to leave a false impression: both Janssen and Boland are critics of the program of microfoundations.

[3] Hoover (1988, p. 135). Also see Hoover (1988, ch. 9, esp. section 2; ch. 10, pp. 241–4); Friedman (1955).

thus not grounded in successful applications. Rather it appears to be based on an instinctive belief in *ontological individualism*: the doctrine that all that exists fundamentally for the economy are individual economic actors. Lucas and his fellow new classical economists have promoted representative-agent models, a class of models in which the mathematical methods of microeconomic optimal choice are applied to a single individual who takes national income as his budget constraint and whose choices are taken to represent the aggregate choices of the economy, because they appear to achieve the reduction of macroeconomics to microeconomics as required by the program of microfoundations for macroeconomics. A. P. Kirman (1992) severely criticizes the representative-agent model, not because it aspires to methodological individualism, but because it fails to fulfill the necessary conditions for perfect aggregation, so that the representative agent in the models fails to represent actual individuals successfully. Methodological individualism remains the goal. Similarly, David Levy (1985) argues that complete methodological individualism is impossible, because, given imperfect information, individual economic actors must make reference to collective entitites as part of their own decision-making processes. Nevertheless, Levy (1985, p. 107) writes: "These collectives have no real existence but are simply the product of theories." While defending macroeconomics against the strong claims of the program of microfoundations, Blaug (1992, p. 45) nevertheless writes: "ontological individualism is trivially true."

It is important to understand that there are some senses in which neither the methodological nor the ontological individualist denies the existences of aggregates, collectives, or wholes. No one denies that GDP calculations can be made and reported, or even that GDP may have some locally stable relationship to unemployment or average interest rates or some other aggregate. Similarly, no one denies the existence of social organizations such as governments or firms (in the sense that talk of governments and firms conveys meaning). What is typically denied is that such aggregates or organizations are among the fundamental units from which economic reality is constructed.

Hayek (1979, ch. 4) argues that such entities are secondary, and that the role of a social science is "compositive" – that is, that it must explain these entities as arising from the fundamental individual components.[4] Hayek (1979, ch. 6) denies that the wholes that social science explains through compositive methods are subject to scientific laws. He holds up the attempt to connect them through laws as an example of Whitehead's "fallacy of misplaced concreteness." He writes: "the wholes about which we speak exist only if, and to the extent to which, the theory is correct which we have formed about the connection of the parts which they imply, and which we can explicitly state only in the form of a model built from those relationships" (Hayek 1979, p. 98). Hayek thus argues that aggregates exist, but derivatively rather than fundamentally, and that in Mäki's terminology they do not exist objectively (i.e., unconstituted by the representations of theory).[5] Still, even Hayek does not endorse practicable methodological individualism, stressing the importance of a reduction to microfoundations *in principle* and himself citing the Cournot problem (Hayek 1979, pp. 74–5, esp. fn. 8).

[4] While not denying that aggregates such as GDP or the general price level can be calculated, Mises (1949/1966, p. 217) goes further than Hayek in arguing that they are quite devoid of meaning (also see Lachmann 1976, p. 96).

2 Is macroeconomics ontologically problematic?

One might concede the main point of the last section – namely, that the drive for microfoundations is driven by ontological individualism – yet not believe that any interesting metaphysical issue is involved, because the ontology of economics is too well understood by common sense to pose any serious puzzle. Mäki (1996, p. 434), for example, contrasts *folk economics* with *scientific economics*, arguing that scientific economics merely presents modifications of the "'ontic furniture' of the general folk views of 'man' and society." He lists some types of possible modifications: selection, abstraction, idealization, exaggeration, projection, aggregation. But he maintains that none of these modifications or combinations thereof "accomplishes a major departure from the ontic furniture of the ordinary realm. No new *kinds* of entities or properties are introduced" (Mäki 1996, p. 435). Mäki illustrates his point with a discussion of the business firm in standard neoclassical analysis, and concludes "that folk economics and neoclassical economics have real business firms as their shared referent even though they represent these firms differently."

Mäki's case for the "ontological commonsense realism" of economics in which the ontic furniture poses no special challenges to the under-

[5] The terminology of "fundamental" or "derivative" existence is fraught with difficulties. It is beyond my purpose to try to sort such matters out here. It is enough for the point at hand, however, to note that Hayek does not believe that economic aggregates can be causes in their own rights. They might serve as some sort of shorthand, but he argues that there is always an adequate causal mechanism independent of that shorthand.

standing is more persuasive for some parts of economics than others. I want to argue that for some important macroeconomic aggregates, it is not particularly cogent, and that some such aggregates do not share a referent with folk notions. The case can be illustrated with reference to the related notions of "real GDP" (or "real income" or "real output," these terms being used almost interchangeably) and the "general price level."

On any interpretation, macroeconomics takes a larger view of the economy and deals with aggregates, which are, in turn, constructed from characteristics of individual economic actors. It is helpful to distinguish two types of aggregates.

First are what we might call *natural* aggregates: simple sums or averages. Examples of natural aggregates are the level of total employment or the average rate of interest on six-month commercial paper. I call these natural aggregates because they are measured in the same units (i.e., they have the same dimensionality) as the individual components that they comprise and, therefore, preserve a close analogy with those individual components. Employment, for instance, is measured by the number of workers or the number of man/hours at both the level of a single individual and in aggregate. The rate of interest on a bond and the average rate of interest on a group of similar maturity bonds are both expressed as a percentage yield per unit time.

A second type of aggregate, what we might call *synthetic* aggregates, are important for macroeconomics. I call these synthetic because they are fabricated out of components in a way that alters the structure of the components, so that they are dimensionally distinct from the components and so that there is no close analogy (despite their sometimes sharing a common name) with the components. The nature of the synthesis is well illustrated by the general price level. The notion of a general price level aims to capture a pre-scientific insight: "a dollar just ain't what it used to be!"; "when I was a lad a penny would buy what a quarter does these days." To capture this insight, one would like to have some notion of the average level of prices. A simple average will not of course work: (10¢/orange + 20¢/apple + $27,948/Volvo station-wagon)/3 does not convey any useful information. One cannot add apples and Volvos, as they say.

It might be argued that any sort of an average is altogether the wrong way to start. What is really wanted is an estimate of the price of money itself, and not the average price of goods. The price of money would, like the price of oranges, be a single number. The general price level could be defined to be its inverse ($1/p_m$). Since relative prices of goods change because of changes in the conditions of demand and supply, there would

be, at best, a rough proportionality between individual prices and the general price level. Indeed, it would permit one to isolate which changes in individual prices were the result of "real" factors and which of monetary factors.

This approach, however, does not do justice to the pre-scientific insight, for it does not provide us with a concept or a measurement of prices that is independent of highly particular and highly inadequate theoretical models. To see this consider, how one would actually determine the value of p_m. One might, for instance, write down a complete Walrasian general equilibrium model in which commodities were expressed in natural quantities, prices in terms of money, and all assets denominated in money were valued using p_m. Aside from the impracticality of formulating and solving such a model for an actual economy (the Cournot problem again), it is well-known that p_m might not be determinable in such a model, or if it is, might not be unique (Hahn 1965; Samuelson and Sato 1984; see Hoover 1988, ch. 5, section 1 for a simple exposition). The essence of the problem is that the real quantity of money (i.e., the useful services that it provides), unlike the real quantities of apples or Volvos, depends fundamentally on the price of other goods. In adjusting the prices of various goods, including money, unique convergence may not be possible, because each time prices adjust to remove an excess demand or supply, the quantity of money changes – possibly in a discontinuous manner – which can increase rather than diminish some of the excess demands or supplies.

This problem in the foundations of monetary theory has yet to be satisfactorily resolved. But what if it had been? It would tie the notion of the general price level extremely closely to a particular theoretical analysis. The measurement of p_m would be "derived" rather than fundamental (see Ellis 1966, ch. 8). The pre-scientific notion is not tied to such a derivation. That would not pose any special problem if the general price level derived in this way correlated closely with numerous other theoretical methods of deriving it, which in turn correlated reasonably with the pre-scientific sense of a general rise in prices.[6] Temperature provides an example of what is wanted (Ellis 1966, ch. 6). The notion of hotter and colder is pre-scientific. The first attempts to provide quantification relied on some presuppositions – e.g., the linearity of the expansion of the various fluids used in thermometers – but were not tied to particular theories. Temperature measures can now be derived from particular theoretical understandings – e.g., from the kinetic theory of

[6] Avogadro's number, for example, can be computed to take the same value from numerous theoretically independent methods (see the discussion in Hacking 1983, pp. 54–5).

gases. Such derived measures show considerable consilience with the pre-scientific notions of hotter and colder and with the atheoretical measurement systems. They permit the extension of temperature scales beyond ordinary experience – e.g., to the measurement of the temperature of the sun – but retain their independence from particular theories because of the consilience of measurements derived from different theoretical starting points. In contrast, the measurement of the general price level remains at the atheoretical stage in which the makers of price index numbers, Laspeyres, Paasche, and Fisher, are the economists' Fahrenheit and Celsius.

The point of raising these difficulties in measuring the general price level is not that the existence of aggregates is tied to their measurement. Rather it is that the difficulties in measuring them help to expose what problematic entities they are, and undermine the appeal of seeing them as close analogues of their components (particular prices, particular goods, and so forth). The disanalogies can be made clearer through a more detailed examination of the general price level.

The fundamental insight of the index number is that one can avoid some of the dimensional nonsense of averaging disparate prices by averaging percentage rates of change instead. A simple average, however, does not capture the commonsense feeling for the degree of price change. A change in the price of gasoline should count for more than, say, a change in the price of caviar in measuring the change in the general price level. How to weight various price changes turns out to have an irreducible degree of arbitrariness.

In general the percentage change in the general price level, indicated as p (where p is the logarithm of the general price level), is related to the individual underlying prices as

$$\Delta p = f(\Delta p_1, \Delta p_2, \ldots, \Delta p_n), \tag{1}$$

where p_j is the price of good j for $j = 1, 2, \ldots n$. Now, the properties that theoretically restrict the functional form of f(.) are very weak:

1. if $\forall j\ \Delta p_j \gtreqless 0$, then $\Delta p \gtreqless 0$.
2. $\Delta p \leq \max\{\Delta p_1, \Delta p_2, \ldots, \Delta p_n,\}$.
3. $\Delta p \geq \min\{\Delta p_1, \Delta p_2, \ldots, \Delta p_n,\}$.

Conditions 2 and 3 together imply an obvious corrollary:

4. if $\Delta p_1 = \Delta p_2 = \ldots = \Delta p_n$, then $\Delta p = \Delta p_j$ for any j.

Condition 1 says that if each price increases (decreases) the general price level must itself increase (decrease), and that the general price level cannot change if no individual price changes. Conditions 2 and 3 say that

the general price level cannot increase by more than the largest nor decrease by less than the smallest individual price change. Condition 2 says that if every individual price changes equiproportionally, so must the general price level. An infinite number of functions fulfill these conditions, and the range of consistent changes in the general price level, given a fixed set of underlying price changes is wide. In practice, price indices are generally linear

$$\Delta p = \sum_{j=1}^{m} w_j \Delta p_j, \text{ where } \sum_{j=1}^{m} w_j = 1. \tag{2}$$

For $m < n$, this formulation recognizes the practical fact that price indices are based on samples of selected goods. The weights w_j in these indices are chosen in practice to capture the pre-scientific sense of the amount of a price rise. Two common weighting schemes with rationales in microeconomic consumer theory are the Laspeyres index, which chooses the weights to reflect the share of each good in base period consumption, so that the general price level effectively measures the changing cost of a fixed bundle of goods, and the Paasche index, which chooses weights to reflect the share of each good in current period consumption, so that some compensation is made for substitution from relatively more expensive to relatively cheaper goods in the face of changes in relative prices.

Neither index is "correct"; there are an infinite number of indices lying between the two; and economists have from time to time argued the case for other indices with different weighting schemes.[7] The non-uniqueness of the price index is important for the point of this essay. It is a fundamental property. A price index is an attempt to quantify the pre-scientific insight that the value of money changes. The different admissible price indices are not, however, approximations to some true underlying general price level. The general price level is in some fundamental sense non-scalar, although there is no currently acceptable scientific refinement that captures that fact.[8] No similar indefiniteness attaches to any of the prices of the underlying individual goods.

The change in the general price level, p, may be integrated over time to generate a price level ($p_{t+n} = \int_t^{t+n} \Delta p dt = p'_{t+n} + c$). The constant of integration (c) permits us to choose an arbitrary base usually $P_t = 1$ or

[7] Generally one expects the Laspeyres index to be greater than the Paasche index, but this can be guaranteed only if certain regularity conditions are imposed on preferences that may not always hold for individual agents.

[8] This may be an area in which the theory of fuzzy sets would be helpful. The use of scalar indices may account for some portion of the apparently irreducible randomness in estimated macroeconomic relations.

$P_t = 100$, where $P = \exp(p)$, for some desired base period t (other base values are less common, but not unknown). P differs from the particular price of, say, a Volvo, not just in its intrinsic indefiniteness, but also in its dimensions. The dimensions of the price of a Volvo are dollars/Volvo; the dimensions of P are period-t dollars/base-period dollars. The dimensions of P are not the dimensions of the price of any good. They appear to be the inverse dimensions of the price of money, taking base-period money to be the numeraire. Given the indefiniteness of the price index, however, it is evident that the price of money is unlike the price of other goods, and represents a substantial departure from pre-theoretic notions of price. The price index is used to normalize the price of particular goods, thereby to decompose individual price changes into a common or general element and a "real" or relative (to the index) element. That this operation is not obvious to common sense will be evident to anyone who has taught elementary economics or read policy analysis by non-economists.

Real GDP is another important example of a synthetic aggregate. Considered as national income, nominal GDP adds up the incomes of each individual in the economy and is an obvious extension of the accounting framework for business or personal income. In a major innovation in economic analysis, the national accounting framework since the 1930s establishes the three-way identity between the sum of all incomes, the value-added in production, and the value of all final goods and services. That these other methods of computing GDP have obvious commonsense analogues is less clear. If final goods (i.e., goods that are not inputs into other production processes) are indicated by Q_j, then nominal GDP is

$$Y^N = \sum_{j=1}^{n} P_j Q_j \qquad j = 1, 2, \ldots n. \tag{3}$$

The dimension of income is dollars/unit time. Money provides the common unit that is essential if disparate goods are to be added.

If some or all of the prices of individual goods increase, it is obvious that nominal GDP could increase without any of the individual quantities changing. In the utilitarian framework that underlies economics this is an undesirable characteristic, because the measure of income has changed without the underlying utility, which is assumed to be generated by the quantities of the goods themselves, changing. It is clearly desirable to correct for changing prices. The usual way to do this is to compute real GDP as

$$Y^R = Y^N / P. \tag{4}$$

Real GDP is often treated as the analogue of an individual good. It does not, however, have the dimensions of a real good. Rather its dimension is base-period dollars (not dollars/unit good). Real GDP is a derivative measurement. One gets a different measurement for it for every different admissible price index. It inherits the fundamental fuzziness of the general price level.

The analogy of real GDP to an individual good is suggested to some by the possibility of perfect aggregation. If, for example, relative prices are constant (i.e., p_j/p_k is constant for all j and k), then $\sum_{j=1}^{n} P_{j,t} Q_{j,t}$ (where the t in the subscript indicates the base time period t) can be normalized by choosing the units for the $Q_{j,t}$s so that each $P_{j,t} = 1$. Then nominal GDP at time n can be written

$$\sum_{j=1}^{n} P_{j,t+n} Q_{j,t+n} = P_{t+n} \sum_{j=1}^{n} Q_{j,t+n}. \tag{5}$$

In this case, conditions 1 to 4 above insure that P is unique. Some conclude therefore that in this limited case, one can treat the summation in the right-hand side of equation (5) as a natural aggregate quantity analogous to an individual quantity. The conditions for constant relative prices are almost certainly never fulfilled, but even if they were the summation is not analogous to an individual quantity. The general price level P in (5) still has the dimension period-n dollars/period-t (i.e., base period) dollars. To sum heterogeneous goods, they must still be converted to a common denominator, and in this case, the summation still has the dimensions of period-t dollars. This would be more perspicacious if (5) were written as

$$\sum_{j=1}^{n} P_{j,t+n} Q_{j,t+n} = P_{t+n} \sum_{j=1}^{n} 1_{j,t+n} Q_{j,t+n}, \tag{6}$$

where the subscripted numeral 1 is a place holder for the dimensional conversion.

The general price level and real GDP are the most important aggregates in macroeconomics. There are many others. Each mixes the characteristics of simple and synthetic aggregates to different degrees. Average interest rates were cited above as an example of a simple aggregate, but when averaging is across nonhomogeneous maturities and risk classes, interest rates too are complicated by the fundamental problems of index numbers. Aggregation of employment across skill or quality levels faces similar considerations. There are other derivative quantities as well. The real rate of interest is defined to be the market interest rate less the percentage change in the general level of prices (Δp). Like real GDP, the real rate of interest inherits the fundamental fuzziness of the general price level.

The history of quantitative economics demonstrates that even the use of simple averages represented a difficult conceptual leap. On the best interpretation what is accepted to common sense is relative. To treat synthetic aggregates as mere extensions of commonsense notions appears in comparison to make a category mistake. Despite their deceptively related names, there is no simple analogy between the general price level and individual prices or between quantities of individual goods and real GDP.

3 The supervenience of macroeconomics on microeconomics

Synthetic aggregates, at least, are not direct extensions of folk ontology. It is clear, however, that, if their independent reality is to be demonstrated in a sense more fundamental than that one can always calculate them according to some algorithm, we must first show that such aggregates cannot be reduced to properties of individual economic actors. Aggregates are in fact calculated; they clearly do not exist in a separate Platonic realm; and ontological individualism has immediate appeal, because we all have first-hand experience as economic actors. Any account of the autonomy or nonreducibility of macroeconomic aggregates must account, therefore, for the relationship of the individual to the aggregate.

Macroeconomic aggregates I believe *supervene* upon microeconomic reality. What this means is that even though macroeconomics cannot be reduced to microeconomics, if two parallel worlds possessed exactly the same configuration of microeconomic or individual economic elements, they would also possess exactly the same configuration of macroeconomic elements. It is not the case, however, that the same configuration of macroeconomic elements implies the same configuration of microeconomic elements.

Biology provides analogies and disanalogies for economics. Alexander Rosenberg (1985, ch. 4, section 8, ch. 6, section 3, *passim*) applies the notion of supervenience to the relationship of functional biology (macro) to molecular biology (micro). To take one example, hemoglobin is an element in functional explanations of the operations of the cardiopulmonary and circulatory systems of higher animals (Rosenberg 1985, ch. 4, section 2). At the molecular level, hemoglobin is not a single chemical, but a family of chemicals. To be hemoglobin at the macro level, a molecule must possess nine particular proteins at critical junctures in the molecular structure. Across different species the approxi-

mately 140 remaining proteins that the hemoglobin molecule comprises vary considerably. Similarly, Rosenberg argues that Mendelian genetics supervenes on a molecular base. Mendelian genetics uses a conceptual scheme that is not easily mapped onto molecular features, but nevertheless identical molecular configurations produce identical genetic behavior.

The notion of supervenience was initially suggested in the philosophy of mind as a method of retaining the dependence of the mental on the physical, while at the same time denying psychophysical laws (see Kim 1978, p. 153). Rosenberg draws on the analysis of Jaegwon Kim (1978). For Kim, supervenience is a relationship between two distinct realms of properties (or relations). Consider I_j, which is a conjuction of properties in the micro realm in which every one of the properties or its complement form one of the conjuncts.[9] Each I_j is then a complete characterization of a possible micro state, and the disjunction of every I_j defines every possible micro state. Consider the A_j, constructed *mutatis mutandis* for the macro state. A family of macro properties is supervenient on a family of micro properties when any objects which share the same micro properties necessarily share the same macro properties. Kim (1978, pp. 152–3) shows that one can derive the following relationship:

$$I_1 \vee I_2 \vee \ldots \vee I_n \rightarrow A_k, \text{ for any } A_k, \tag{7}$$

where the I_h, $h = 1, 2, \ldots, n$, are a subset of the I_j.[10] Of relation (7), Kim (1978, p. 153) says: "I don't see how such generalizations could fail to be lawlike."

Using Kim's analysis, Rosenberg argues against the autonomy of Mendelian genetics. The conceptual scheme of Mendelian genetics (the macro level) does not map easily into the conceptual scheme of molecular biology (the micro level). Mendelian genetics permits explanation of phenomena not easily explainable directly from the molecular level. However, Mendelian genetics fails to account for some phenomena within its scope. Rosenberg argues that Mendelian genetics supervenes on molecular biology, and that molecular biology is the more scientifically advanced, more fundamental and autonomous theory. Mendelian genetics is reducible in principle (that is the upshot of Kim's analysis in

[9] The account of Kim's analysis here omits most of the technical details (these are also reproduced in Rosenberg 1985, pp. 113–16), and changes his notation. The identification of the distinct realms of properties as "micro" and "macro" is my addition – literally *ad hoc* – and does not significantly affect Kim's analysis.

[10] Kim goes on to show that in some cases the implication in (7) can be strengthened to a biconditional.

(7) above), but it retains heuristic power, because something like the Cournot problem prevents the practicable application of molecular biology to some phenomena in which Mendelian genetics is relatively successful.

Can a similar argument be applied to economics? I do not think so. Rosenberg (1992, p. 129) himself has argued that the intentional character of microeconomics limits its scientific development: microeconomic "theory's prediction and explanation of the choices of individuals [cannot] exceed the precision and accuracy of commonsense explanations and predictions with which we have all been familiar since prehistory." In fact, macroeconomic explanation and prediction is not only often better, but may have more scope for improvability. An electric supplier could not say when Mary Smith will switch on her oven, but it may know pretty precisely how many kilowatts it must supply at a given time, based on an aggregate analysis of past behavior. Insurance companies know that whether an individual is, say, a smoker or obese matters probabilitistically to his chances of dying. But the company would go broke trying to predict individuals' precise dates of death.[11]

It is important to remember that it is not macroeconomic theory that supervenes on microeconomic theory, but macroeconomic reality that supervenes on microeconomic reality. The disabilities of microeconomic theory thus prove, at most, that there can be no automatic presumption that microeconomics is more basic, because more successful, and that macroeconomics is merely heuristic. The critical relationship is the reducibility in principle suggested by (7) above. To begin to undermine reducibility in the case of macroeconomics, it helps to note a crucial disanalogy with biology. Reduction appeals to biologists because it removes scientifically suspect teleological explanation common in evolutionary biology and other functional accounts. The aim of reduction in economics, however, is precisely the opposite: macroeconomics appears mechanical and dehumanized, and the point of the program of microfoundations is reintroduce human decision-making as an explanatory element. The point is to recover intentionality.

Kim's analysis posits two levels of properties that are (semantically at least) distinct and then investigates how they must be related if one set is supervenient on the other. Intentionality at the microeconomic level undermines the distinctness of microeconomic properties from macroeconomic properties. Levy's argument (see section 1 above) that individual economic actors will invariably make reference to social wholes and aggregates is even more fundamental than he imagines. In evaluating

[11] Both these examples are repeated verbatim from Hoover (1995a).

the future individuals must form expectations about real prices and real quantities. Independently of the uncertainty of the future, the Cournot problem implies that it is impracticable to solve good-by-good, price-by-price, period-by-period planning problems in all their fine detail. The best that one is practically able to do is to work with aggregates. The information on which these are based is fundamentally monetary. Economic actors must use estimates and expectations of the general price level and real interest rates to form any practical assessment of their situations.

Hayek (1979, p. 62) writes:

> in the social sciences it is necessary to draw a distinction between those ideas which are *constitutive* of the phenomena we want to explain and the ideas which either we ourselves or the very people whose actions we have to explain may have formed *about* these phenomena and which are not the cause of, but theories about, the social structures.

What Levy's argument demonstrates is that Hayek is mistaken, that how people theorize about the economy *is* constitutive of macroeconomic phenomena.[12] Since people cannot theorize about certain sorts of phenomena without appealing to macroeconomic categories – that are not themselves reducible to microeconomic categories – the Cournot problem introduces analytical constraints, not only in practice, but in principle as well. The distinctiveness of the properties at the microeconomic and macroeconomic levels is breached, undermining Kim's analysis, because complete characterizations of the microeconomic must include characterizations of the macroeconomic on the part of individual agents.

To challenge the applicability to economics of the reductionism in principle, implicit in Kim's analysis and in Rosenberg's application of it to biology, does not challenge the notion that macroeconomics supervenes on microeconomics. Kim's analysis is epistemological: it argues that there must be laws that would permit us to draw connections between the micro and macro levels. The point here is ontological: even though macroeconomics cannot be reduced to microeconomics as the program of microfoundations suggests, the elements of macroeconomics could not exist without the substrate of microeconomic individuals.

[12] In contrast to Hayek, his fellow Austrian-school economist Mises (1943, p. 252) argues that knowledge of economic theory can prevent the mistaken investments that fuel the business cycle.

4 Two arguments for the reality of macroeconomics

So far we have argued that the ontological status of macroeconomic entities is problematic in the sense that, like other entities posited by scientific theories, they are not part of our commonsense ontic furniture. Furthermore, the nature of the relationship through which the elements of macroeconomics supervene on the elements of microeconomics precludes direct reduction of the macroeconomic to the microeconomic, even in principle. If macroeconomic entities exist, they cannot be said therefore to exist only derivatively, despite their supervenience on microeconomic entities. It remains to argue directly for the existence of macroeconomic entities.

The first argument is based on the argument from manipulability championed by Ian Hacking (1983, esp. pp. 22–4): "If you can spray them, then they are real." Hacking argues that convincing evidence of the reality of the electron is found in experiments aimed at detecting the existence of free quarks, in which niobium balls are charged by "spraying" them with electrons. The general point is that an entity defined by a scientific theory has real existence when procedures that are best understood as using the entity referred to by the theory as a tool to manipulate other parts of the world, such as in laboratory experiments. I take this argument to be related to the "no-miracles" argument for the reality of scientific entities.[13] The best explanation of why theories are predictively successful, including successfully predicting the consequences of using them to design experimental or engineering manipulations of the world is that the entities posited by them in fact exist – anything else would be an inexplicable miracle.

It is common to denigrate the empirical success of economics (see e.g., Rosenberg 1992, pp. 18, 56, 112, 238, *passim*). It is true that economics does not have the precision of physics or chemistry, although it is less clearly inferior to meteorology, geology, climatology, and parts of biology – to name just a few of the less exact, but nevertheless scientific disciplines. The reputation of economics for predicting poorly arises partly because people seek unconditional forecasts ("what will happen tomorrow?") while economic theories typically predict only conditionally

[13] Mäki (1996, section 6) argues that the no-miracles argument and other arguments from manipulability cannot be successfully applied to economics, even if they apply to physical sciences. (There may, of course, be other arguments – and Mäki supplies some – for existential beliefs about economic entities.) The essence of my position is that *macro*economics shares characteristics with physical sciences that microeconomics *may* not.

("tomorrow X will happen if Y happens"). Quantified economic relations are at best locally stable: the precise estimate of the price elasticity of demand for Volvos changes with changes in the range of alternative brands and models, with changes in the proportion of academics to the total population, and with changes in other background conditions. Nevertheless, qualitatively stable relations are well established: e.g., demand curves slope down (i.e., when the price of Volvos rises, sales of Volvos fall). And there is often enough local stability that useful quantitative assessments are possible. Can irreducible macroeconomic aggregates be manipulated as well?

The answer seems to be clearly yes. Consider the following example. Almost every macroeconomic theory predicts that sufficiently large expansions of government expenditure will change (probably increase) nominal GDP and the general level of prices.[14] Different theories differ in their precise understanding of the mechanisms. Similarly, no macroeconomic theory disputes the ability of the Federal Reserve to use its ability to supply or remove reserves from the banking system to set the level of the Federal funds rate (the rate at which one commerical bank borrows from another overnight). The empirical evidence in support of these propositions is also overwhelming. Now consider two irreducibly macroeconomic aggregate entities: the real rate of interest (i.e., the market rate of interest less the percentage change in the aggregate price level (p)) and the yield curve (an aggregate relation portrayed as a graph of market interest rates against time to maturity of the associated bonds). Both the real rate of interest and the yield curve are synthetic aggregates and both are entities with causal powers in some economic theories. Every macroeconomic theory that I know predicts that actions that increase the general price level or the Federal funds rate will shift the yield curve upwards in the short-run. And, at least if the changes are unanticipated, increases in the general price level will reduce the level of the real interest rate. The empirical evidence for these effects is overwhelming, and indeed are easily confirmed by anyone willing to read the *Wall Street Journal* regularly for a month. Just like the electron, some macroeconomic aggregates not only can be controlled, but can be used to manipulate other macroeconomic aggregates.

The second argument is related to the first. Nowak (1980) and others

[14] The caveat "almost" and the ambiguity over the direction both hinge on the financial market. If the interest elasticity of money demand were zero (empirically a false supposition), there would be no change in prices; if the substitution effect on money demand induced by an increase in government bonds financing an increase in government expenditure were large enough (again unlikely), the price level could fall.

have argued that the principal method of constructing scientific theories is *idealization*. Nowak's (1980, p. 29) paradigm idealization statement is:

$$\text{If } G(x) \text{ and } p_1(x) = 0 \text{ and } \ldots p_{k-1} = 0 \text{ and } p_k = 0 \text{ then } F(x) = f_k(H_1(x), \ldots, H_n(x)) \quad (8)$$

where H_i $(i = 1, \ldots, n)$ denote *primary factors* and the p_j $(j = 1, \ldots, k)$ denote *secondary factors*. An idealized theory is one which picks out the primary factors by setting the secondary factors to extreme values: zero or, represented here, without loss of generality, as $p_j = 0$.

Were $G(x)$ a known and exhaustively complete theory of the phenomenon within its explanatory range such that one could accurately specify each of the secondary factors that were set aside, then the distinction between primary and secondary factors would in fact be be unclear, because our complete knowledge of $G(x)$ would allow us for example to replace (8) with

$$\text{If } G(x) \text{ and } H_1(x) = 0 \text{ and } \ldots H_{n-1} = 0 \text{ and } H_n = 0 \text{ then } F(x) = f_n(p_1(x), \ldots, p_k(x)). \quad (9)$$

In the case of either (8) or (9), releasing the idealizing conditions ($p_k = 0$ or $H_i = 0$) allows us to recover the complete theory, $G(x)$. Idealization has been reduced to a fancy name for an arbitrary selection of *ceteris paribus* conditions or to a formal nesting relationship for theories.

Hoover (1994) argues that in an empirical context, the method of idealization has power only if we recognize that not all of the idealizing conditions can be explicitly stated. The claim to distinguish between primary and secondary factors is then a claim that the primary factors are the *essence* of the matter. Idealized theories thus aim to identify, isolate and relate the real essences or causally effective capacities of economic reality.[15] The success of such an idealized theory then amounts to an ontological claim for its primary factors.

That Keynesian macroecononomics could be cast as an idealization that employs macroeconomic aggregates essentially is beyond doubt. The

[15] Cartwright (1983, 1989) argues for realism with respect to causal capacities, but for an instrumentalist interpretation of scientific laws. Laws are either literally false ("the laws of physics lie" – to quote the title of Cartwright's (1983) earlier book) or are merely *phenomenal* – i.e., atheoretic regularities. Hoover (1994a) argues that if idealized models represent essences, then phenomenal laws are necessary bridges to take the place of those secondary factors that cannot in fact be identified explicitly. Mäki (1992) argues that Nowak conflates idealization with *isolation*, which comprises idealization, omission, and other techniques. To apply Mäki's account we would have to say that the omission of secondary factors amounts to a claim that retained primary factors are the essence of the matter.

major competitor to Keynesian macroeconomics today, new classical macroeconomics, trades on an explicitly microfoundational approach. There is, however, less here than meets the eye. The currently most popular new classical macroeconomic theory is embodied in the real-business-cycle model (see Hoover 1995b and forthcoming). The proponents of this representative-agent model would like it to be interpreted as an idealization from a complete Walrasian general equilibrium model of the economy in which distributional issues are idealized out of the model so that what remains is a one-agent, one-good, one-price representation of the economy. This would work if the Walrasian model (the analogue for $G(x)$ in Nowak's schema) were both true and known in detail. At least the second condition is false, which undermines the evidential basis for the first condition.

Empirically, far from isolating a microeconomic core, real-business-cycle models, as with other representative-agent models, use macroeconomic aggregates for their testing and estimation. Thus, to the degree that such models are successful in explaining empirical phenomena, they point to the ontological centrality of macroeconomic and not microeconomic entities. The appeal to the methods of microeconomics does not in this case amount to the successful implementation of the program of microfoundations, for they are but the simulacrum of microeconomics. The relationship between models that are microeconomic in form and their macroeconomic empirical implementation is metaphorical. The nature of metaphorical connection deserves futher exploration. It is enough for the present purpose to understand that, at the empirical level, even the new classical representative-agent models are fundamentally macroeconomic in content.

5 The contingency of economic reality

Unless one is committed to a certain kind of apriorism, then what we judge to be real depends on experience, on the nature of our theorizing, and the success of our scientific and everyday practices. The best guess of a scientist in 1789 would have been that phlogiston was real, although most scientists would today say that it is not. The point is not that reality is constituted by our theorizing: for a realist it is not only the case that phlogiston is not real today, but that it *was* not real in 1789. The point is rather that our knowing whether or not something is real is a scientific fact like other scientific facts, which are established by argument and evidence, and about which we may be mistaken. There is, therefore, no

timeless, certain answer to the question in the title of this paper. What I have sought to show in this essay is that the nature of microeconomics and macroeconomics – as they are currently practiced – undermines the prospects for a reduction of macroeconomics to microeconomics. Both microeconomics and macroeconomics must refer to irreducible macroeconomic entities. These macroeconomic entities occupy ontologically independent places in economic theory. To the degree that such theories are empirically successful, the best account of these macroeconomic entities is that they are real.

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