

distribution, from which we can think of the points as arising, has a density with elliptical contours. Two of the features of the point clouds are the ratio of the lengths of the minor and the major axes and the area. Subjects might be using either to judge association.

The ratio of the minor axis to the major axis of a contour of the associated bivariate normal distribution is $\sqrt{(1-r)/(1+r)}$, since the standard deviations of $x_i(k)$ and $y_i(k)$ are equal and the scales on the horizontal and vertical axes of each scatterplot are the same. If subjects were judging association by judging the ratio of the axes of the point cloud, then the judged scale would be $g(r)$, which, as described earlier, is shown in Fig. 2.

The area of an elliptical contour of the associated bivariate normal distribution divided by the area of a rectangle with sides parallel to the horizontal and vertical axes of the plot is equal to $.25\pi\sqrt{1-r^2}$. If subjects were judging association by judging the areas of the point clouds relative to a circumscribed rectangle, the judged scale would be $w(r)$, which, as described earlier, is shown in Fig. 2.

Neither of the curves $w(r)$ and $g(r)$ appears to describe the judged association (15). It could be, however, that one of the two geometrical tasks—judging axis ratios or judging areas—is being carried out but that there are biases in the judgments that alter the perceived association. For example, it is known that judgments of area and length tend to be proportional not to the physical quantity but rather to the physical quantity to a power less than 1 (16). New experiments are needed in order to better understand the perceptual mechanism that people use in judging association (17).

WILLIAM S. CLEVELAND
Bell Laboratories,
Murray Hill, New Jersey 07974

PERSI DIACONIS
Stanford University,
Stanford, California 94305

ROBERT MCGILL
Bell Laboratories, Murray Hill

References and Notes

1. G. W. Snedecor and W. G. Cochran, *Statistical Methods* (Iowa State Univ. Press, Ames, 1967).
2. S. J. Devlin, R. Gnanadesikan, J. R. Kettenring, *Biometrika* **62**, 531 (1975); F. J. Anscombe, *Am. Stat.* **27**, 17 (1973).
3. The written instructions to subjects included: "This is an experiment to find out how people such as you assess the association of two variables from a scatterplot. We will measure association on a scale of 0 to 100. Zero means no association and 100 means perfect linear association" and "We are going to show you scatterplots and ask you to rate on a scale of 0 to 100 what your subjective assessment of the association is. There is no right answer. We are inter-

ested in what the association appears to be to you."

4. Let q_i for $i = 1, \dots, 200$ be equally spaced quantiles of the normal distribution so that $\Phi(q_i) = (i - .5)/200$. Let u_i be the q_i divided by their standard error. The values portrayed on the horizontal axis of the k th scatterplot are $x_i(k) = \alpha(k) + \beta u_i$ for $i = 1, \dots, 200$. Let $v_i(k)$ be a random permutation of the u_i ; linearly regress $v_i(k)$ on u_i and let $w_i(k)$ be the residuals divided by their standard error. Let r_k be the desired correlation coefficient of the k th scatterplot. The values portrayed on the vertical axis of the plot are $y_i(k) = \alpha(k) + \beta[r_k u_i + (1 - (r_k)^2)^{-1/2} w_i(k)]$. Both $x_i(k)$ and $y_i(k)$ have standard deviation β , and their correlation is $r(k)$. A different permutation is used for each scatterplot. The $\alpha(k)$ are chosen so that the center of gravity of the point cloud is at the center of the plotting frame; β has a value that places the extremes of the point cloud for the smallest scale value just inside the plotting frame.
5. F. Mosteller and J. W. Tukey, *Data Analysis and Regression* (Addison-Wesley, Reading, Mass., 1977).
6. P. J. Huber, *Robust Statistics* (Wiley, New York, 1980); F. R. Hampel, *J. Am. Stat. Assoc.* **69**, 383 (1974).
7. J. W. Tukey and D. H. McLaughlin, *Sankhya Ser. A* **25**, 331 (1963).
8. The data for individual subjects generally followed the pattern of the average behavior but was noisier. A few subjects deviated radically, which suggested using 10 percent trimmed means, but the results do not change dramatically if means and their standard errors are used.
9. A referee suggests that the bigger circles in this

figure get greater visual weight. This could be investigated by experiments similar to those described here.

10. R. F. Strahan and C. J. Hansen, *Appl. Psychol. Meas.* **2**, 543 (1978); P. Bobko and R. Karren, *Personnel Psychol.* **32**, 313 (1979).
11. D. L. Jennings, T. Amabile, L. Ross, *Judgment Under Uncertainty: Heuristics and Biases*, A. Tversky and D. Kahneman, Eds. (Cambridge Univ. Press, New York, 1980).
12. F. Mosteller, A. F. Siegel, E. Trapido, C. Youtz, *Am. Stat.* **35**, 150 (1981).
13. H. Wainer and D. Thission, *Appl. Psychol. Meas.* **3**, 543 (1979).
14. M. B. Wilk, *Bell Laboratories Technical Memorandum* (Bell Laboratories, Murray Hill, N.J., 1966).
15. We fit to the data in Fig. 2 a two-parameter family of curves that includes $g(r)$, $w(r)$, r , and r^2 . The family is $1 - (1 - r)^2(1 + r)^2$. Estimates of the unknown α and β and their standard errors are $.71 \pm .04$ and $.66 \pm .11$, respectively.
16. S. S. Stevens, *Psychophysics: Introduction to Its Perceptual, Neural, and Social Prospects* (Wiley, New York, 1975).
17. P. Tukey, S. Sternberg, and a referee have all pointed out that in the left panel of Fig. 1 the ratio of the plotting-character size relative to the point-cloud size is larger than in the right panel. Perhaps, as the referee has put it, "variables on scatterplots look more highly correlated when the point sizes are increased."
18. We are indebted to R. Gnanadesikan, C. Malows, S. Sternberg, W. Tapp, P. Tukey, and two referees for helpful comments on our manuscript.

11 December 1981; revised 15 March 1982

Economic Values and Embodied Energy

In his article "Embodied energy and economic valuation" (1), Costanza concludes that "With the appropriate boundaries, embodied energy values are accurate indicators of market values where markets exist. . . . they may also be used to determine 'market values' where markets do not exist" and that "the physical dimensions of economic activity are not separable from limitations of energy supply." He uses input-output analysis to support his conclusions and calculates embodied energy as the direct plus indirect energy required to produce goods and services in the U.S. economy.

Costanza makes two major changes in traditional input-output analysis. First, he expands the transaction matrix to include the household and government sectors. With this change, the total net output of the economy is not gross national product (GNP) but the sum of gross capital formation, net inventory change, and net exports. Costanza justifies this shift of system boundaries with Odum's argument (2) that primary factors of production (capital, labor, and natural resources) are not independent, but Odum's argument has no bearing on this shift of system boundaries. What this shift of boundaries does mean, for example, is that the household sector receives energy from other sectors in proportion to employee compensation (modified by indirect business taxes and

so on). While it is acceptable to argue in a national income accounting sense that the market value of the goods and services received by the household sector is equal to the market value of the employee compensation received (with modification for indirect business taxes and so on), Costanza's tracing of Btu flows in this manner is highly debatable and is, in fact, double counting (although he argues it is not). This shift of system boundaries is the most debatable aspect of Costanza's argument and the most important.

The second major change Costanza makes in traditional input-output analysis is the addition of solar energy flows after correcting for the lower thermodynamic usefulness of direct sunlight in comparison with fossil fuels. He assumes that solar energy enters the economy through the agriculture, forestry, and fisheries sectors according to their relative land areas and admits that this crude approximation should be improved. While it is highly debatable whether solar and environmental services should be valued in terms of non-market-determined Btu flows and added to the market-determined Btu flows of traditional input-output analysis, Costanza's use of solar energy flows has little to do with the conclusions he reached. This in itself may be of interest, since he added a total of 51.5×10^{15} Btu's per year to represent the functional

Table 1. Summary of regressions from table 2 in Costanza (1).

Embodied energy alternative	Sectors 1 to 92*			Sectors 8 to 92		
	r^2	F	F -test significance level	r^2	F	F -test significance level
A	.0210	1.89	.1729	.5539	100.57	.0001
B	.0629	5.90	.1710	.2042	20.78	.0001
C	.7809	313.73	.0001	.9907	8633.95	.0001
D	.8535	512.74	.0001	.9454	1401.31	.0001

*Energy sectors 1 to 7 are (1) coal mining, (2) crude petroleum and natural gas, (3) petroleum refining, (4) electric utilities, (5) gas utilities, (6) miscellaneous agriculture products, and (7) forestry and fisheries.

fossil fuel equivalent of solar energy input—a value roughly equal to the total 1967 U.S. energy consumption of 56×10^{15} Btu's (3).

Costanza calculated embodied energy intensities for the 90-sector model of 1967 energy input and output maintained by the Energy Research Group at the University of Illinois. Embodied energy intensities (in Btu's per dollar) were calculated for 92 sectors (4) for four possible alternatives: (A) excluding labor and government energy costs and solar energy inputs; (B) including solar energy inputs; (C) including labor and government energy costs; and (D) including both labor and government energy costs and solar energy inputs. Costanza then regressed dollar value of total sector output on embodied Btu consumption by sector. Table 1 summarizes his regression results, on which he bases his conclusions. Note that a linear relation between sector dollar value of output and sector embodied Btu's also implies a constant embodied energy intensity per dollar across sectors. Costanza uses this to argue that one cannot decouple energy and GNP, since shifts in the mix of output by sector will not reduce embodied energy per dollar of output. His conclusion that GNP cannot grow without increased energy consumption is based on this constancy of embodied Btu's per dollar across sectors. It depends on the validity of the regression in Table 1, ignores the possibility of different rates of technological improvement among sectors, and is not borne out by the well-known fact that the Btu/GNP ratio has declined since 1974.

Reexamination of the regression results of Table 1 indicates that addition of solar energy (option B) adds little to Costanza's analysis since, in comparison to option A, the F -test significance level is reduced whether energy sectors 1 to 7 are included or excluded (5). Costanza's results are due totally to the change in system boundaries (option C) or to exclusion of energy sectors 1 to 7. His regression results for nonenergy sectors 8 to 92 indicate that embodied Btu's per dollar are relatively constant for non-

energy sectors, even under option A, which leaves open the possibility for decoupling GNP and energy by increasing the output of the nonenergy sectors relative to the energy sectors (6). For Costanza's analysis regarding decoupling to hold, Btu's per dollar must be constant for all sectors; hence his conclusions must derive entirely from the change in system boundaries (since option C produced a linear relation for all 92 sectors).

Once it is recognized that Costanza's conclusions are derived from the shift in system boundaries and not the addition of solar energy input, one can return to an assessment of the validity of this assumption. I argued above that Costanza's shift of system boundaries does not follow from Odum's argument and is double counting. The most important of these criticisms is that of double counting. Since the direct and indirect energy embodied in each sector's output is already counted once under option A, the readdition of this same energy via the household and government sectors under the changed system boundaries is clearly double counting. If the output of Costanza's redefined input-output system (sum of gross capital formation, net inventory change, and net exports) were exactly zero, the double counting would be exactly 2.0. Since the sum of these three items was 15 percent of GNP in 1967, the double counting amounted to 1.70. In other words, the total energy counted by Costanza under option C should be exactly 1.70 times that of option A (7). Traditional input-output analysis draws system boundaries that exclude the government and household sectors precisely to avoid such double counting. The total Btu's required to create GNP are not doubled by the fact that the output of Costanza's sectors 1 to 90 is consumed by his sectors 91 and 92 (government and households, respectively).

In national income accounts the net primary inputs (capital, labor, and natural resources) are value added. Each sector's value added (or Btu's transferred) to government and households

under option A is transferred right back (except for gross investment, net inventory change, and exports) in proportion to labor and government services under option C. It is for this reason that Costanza finds a nearly linear relation between the dollar value of a sector's output and its embodied energy consumption. Costanza notes that what he has done with energy could also be done with any of the other primary factors of production to support capital, labor, or government service theories of value, except that physical reality would not support the notion that "labor creates sunlight." But, as we have seen above, his results are not due to the addition of solar Btu's but rather to computations that could be reformulated to support many different theories of value. (It would be of interest to consider how his arguments would apply to a nuclear energy economy—that is, an economy where the use of fossil fuels is nonexistent.)

Costanza states that the idea of an energy theory of value is "summarily dismissed by neoclassical economists," including myself (8), and at several points quotes Georgescu-Roegen's statement (9) that an increase of capital implies an additional depletion of resources (10). He states that his approach represents a return to Leontief's concept of a closed system, yet ignores Georgescu-Roegen's conclusion (11) based on a Leontief system that "relative prices are indeterminate under net energy analysis or gross energy analysis." There are valid reasons for dismissing an energy theory of value.

In a recent review of energy modeling and the economic theory of exhaustible resources (12), I noted that the common assumption that energy is the ultimate limiting factor is not only unacceptable but also arbitrary. One can just as easily and legitimately view technological change as the ultimate limiting factor or argue that other resources could be depleted before energy exhaustion is reached. While there is a clear need to treat environmental and other physical constraints more realistically in economic modeling, the approach offered by Costanza is not the answer, nor does it support an energy theory of value or the policy conclusions stated. The questions to be answered require recognition of the fact that energy is but one constraint and that resource augmentation can be induced in various ways. Physical and environmental systems need to be incorporated more explicitly into economic models, and the linkages between growth and technological change deserve more attention. The insights of

energy and environmental analysts will reach their fullest potential if they are built into valid economic models so that interrelationships can be traced and analyzed.

DAVID A. HUETTNER

*Division of Economics,
University of Oklahoma, Norman 73019*

References and Notes

1. R. Costanza, *Science* **210**, 1219 (1980).
2. H. T. Odum, in *Energy Analysis: A New Public Policy Tool*, M. Gilliland, Ed. (Westview, Boulder, Colo., 1978), pp. 55-87.
3. Note that Costanza used the 1967 U.S. input-output data in his analysis.
4. Ninety sectors plus household and government.
5. This occurs despite the fact that Costanza added 51.5×10^{15} Btu's to represent functional solar energy input.
6. The value for $r^2 = .5539$ under option A in this case is hardly high enough to argue that embodied energy is a good indicator of value for nonenergy sectors.
7. Costanza could have divided his total energy usage under option C by 1.70 and recomputed the input-output coefficients, but the problems introduced by double counting would still remain since the coefficients would still be tainted by double counting (the process of matrix inversion automatically normalizes his coefficients so that the double counting is not obvious).
8. D. A. Huettner, *Science* **192**, 101 (1976).
9. N. Georgescu-Roegen, in *Scarcity and Growth Reconsidered*, V. K. Smith, Ed. (Johns Hopkins Press, Baltimore, 1979), pp. 95-105.
10. Note that Georgescu-Roegen's statement is valid in the absence of technological change.
11. N. Georgescu-Roegen, *Southern Econ. J.* **45**, 1023 (1979).
12. D. A. Huettner, *Energy J.* **2**, 123 (1981).

8 July 1981

I disagree with Huettner's contention that the modifications to the economic system boundaries that I employed in (1) are unacceptable, that they lead to double counting, and that the conclusions in my article are therefore invalid. Contrary to Huettner's interpretation, the method I used was not to first calculate embodied energies under option A and then simply add on the household and government energy costs. This approach would indeed be double counting. Rather, all options were based on separate calculations, made with the constraint that the total energy embodied in the net output from the system (sum of the energy intensities times the net outputs over all sectors) had to equal the total energy input to the system. This constraint eliminated the possibility of double counting regardless of the numerical value of the net output. Huettner's comment that "the double counting would be exactly 2.0" if net output were zero and would be equal to 1.7 with the net output values used is not correct in view of the way the calculations were performed. If net output were zero the system of equations would be indeterminate. In all cases, however, the total energy counted is equal to the total energy input.

The assertion that traditional input-output (I-O) analysis draws boundaries that exclude households and government in order to avoid double counting is not

correct. The boundaries in I-O studies are placed as a matter of convention and convenience in performing certain types of analysis, and there are several examples in the literature of I-O systems with endogenous household and/or government sectors (2). Inclusion of these sectors is necessary when the interdependencies between them and the rest of the economy are of interest, as they are in energy analysis, and it dramatically affects the results and conclusions.

Huettner notes that "the household sector receives energy from the other sectors in proportion to employee compensation" (EC). Figure 2 in (1) shows that households receive inputs in proportion to personal consumption expenditures (not EC) and provide labor services to other sectors in proportion to EC. Again, the contention that "Each sector's value added (or Btu's transferred) to government and households under option A is transferred right back . . . in proportion to labor and government services under option C" is not an accurate description of the modifications made.

Huettner states that Odum's argument (that "primary" factors of production are interdependent) has no bearing on the shift of system boundaries. However, incorporation of these interdependencies into a valid economic model was the intent and the effect of shifting the boundaries.

The point that the Btu/GNP ratio has declined since 1974 is correct, but this decline could hardly be called significant in light of the long-term relation between these variables and the problems with measuring and interpreting aggregate real GNP and energy (3). More important, as I pointed out in (1), some measure of total investment would be more appropriate than GNP as a measure of net system output with the revised boundaries. In general, within the range of uncertainty in the data, Btu/GNP and Btu/total investment time series (4) do bear out my conclusions.

Huettner calls the assumption that energy is the ultimate limiting factor arbitrary, and states that "one can just as easily and legitimately view technological change as the ultimate limiting factor. . . ." There are, in fact, some good reasons for considering available energy the ultimate limiting factor. It is the only input that is both necessary for all productive activities and impossible to create internally or recycle. It must be supplied from outside the system and can only be dissipated internally. The same cannot be said for the other "intermediate" factors of production, land, labor, capital, and technology. Technological

change is certainly an important characteristic of our economic growth, but it is no more independent of direct and indirect energy costs than any other component of the economy. Most technological change in the past century has been aimed at increasing the intensiveness of fossil fuel use (5), and it is debatable whether technological change has improved the overall "efficiency" of economic production (6). We can expect technological change to help us adapt to new energy sources, but it cannot create available energy.

Embodied energy is a fairly new concept that deserves further theoretical and empirical development before any final conclusions are drawn. Although Huettner concludes that "There are valid reasons for dismissing an energy theory of value," there is significant theoretical and empirical support for at least some versions of such a theory. One can hope that the "clear need to treat environmental and other physical constraints more realistically in economic modeling" that Huettner acknowledges will induce economists to assist in the further development and scientific testing of what may turn out to be a very useful theory.

ROBERT COSTANZA

*Coastal Ecology Laboratory, Center
for Wetland Resources, Louisiana
State University, Baton Rouge 70803*

References and Notes

1. R. Costanza, *Science* **210**, 1219 (1980).
2. W. W. Leontief's original exposition of input-output analysis [*The Structure of American Economy, 1919, 1929: An Empirical Application of Equilibrium Analysis* (Harvard Univ. Press, Cambridge, Mass., 1941)] contained endogenous household and government sectors. J. B. Penn, B. McCarl, L. Brink, and G. D. Irwin [*Am. J. Agric. Econ.* **58**, 663 (1976)] reported an I-O model that included an endogenous household sector "due to the interest in economic interdependence including the induced effects of households." Their method of incorporating the household sector was essentially the same as that used in my article.
3. The Btu/GNP ratio for the United States has declined in roughly the same magnitude over the 1958-1966, 1947-1954, and 1940-1944 periods. Although there is a strong statistical relation between energy consumption and GNP [J. Darmstadter, J. Dunkerly, J. Alterman, *How Industrial Societies Use Energy* (Johns Hopkins Press, Baltimore, 1977)] and differences in Btu consumption "explain" a large percentage of the variation in GNP (about 85 percent worldwide and 95 percent for U.S. time series), there is disagreement about how the residual is interpreted and whether Btu consumption and GNP can be decoupled.
4. J. W. Kendrick [*The Formation and Stocks of Total Capital* (National Bureau of Economic Research, New York, 1976)] and R. Eisner, E. R. Simons, P. J. Pieper, and S. Bender [*Total Incomes in the United States, 1946 to 1976: A Summary Report* (International Association for Research in Income and Wealth, Gouvieux, France, 1981)] estimated time series for total investment in the U.S. economy.
5. M. Boretsky, *Am. Sci.* **63**, 70 (1975).
6. A. Griliches and D. W. Jorgenson, *Rev. Econ. Stud.* **34**, 249 (1967).
7. I thank B. Hannon, R. Herendeen, M. Gilliland, H. Daly, L. Bahr, J. Bartholomew, and C. Cleveland for reviewing these comments and providing helpful suggestions.

22 February 1982