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## ABSTRACT

This note reviews the history of the "residual," from its earliest articulation in Copeland (1937) to its codification in Solow (1957), describing the various earlier contributions by Tinbergen, Stigler, Schmookler, Fabricant, Kendrick, Abramovitz and others.

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## THE DISCOVERY OF THE RESIDUAL: A HISTORICAL NOTE

The concept of total productivity and the notion that labor is not the only factor of production, that other factors such as capital and land should also be taken into account in a wealth of the nation calculation or a measure of its productivity, were discussed repeatedly in the literature of the 1930's. Two major strands of research came together, ultimately, in what was to become total factor productivity measurement and growth "accounting": The first developed out of the national income measurement tradition, based largely on the work of NBER and what was later to become the BEA.[1] The second was influenced by Douglas's work on production functions. His work had been largely cross-sectional, but as time series data became available it was an obvious generalization to add trend like terms to the function and allow it shift over time.[2] This research tradition had a more econometric background and did not accept, necessarily, the various compromises and assumptions embedded in the national income accounts. It found a fertile soil in agricultural economics, spurred by the presence and teaching of Gerhard Tintner at Iowa State University (Tintner, 1944) and the later work of Heady (see Heady and Dillon 1961, Chapter 1, for an early review of this literature). The two traditions came together in the work of Solow (1957), in some of my own early papers (Griliches 1960 and 1963), and especially in Jorgenson and Griliches (1967). But they have also kept drifting apart.

The first mention of what might be called an output-over-input index that I can find appears in Copeland (1937).[3] Once one started thinking about "real" national income and worrying how to deflate it, it was a relatively short step to the idea that the two different sides of these accounts (product receipts and factor payments) could be deflated separately, with their own specific deflators, yielding measures of real product and real input, and an associated measure of economic efficiency.[4] A year later the idea was much more fleshed out in Copeland and Martin (1938) who said:

> "...construct an index of the physical volume of wealth used in production each year and weight it by the total property income in the period selected for the determination of weights. This weighted plant-hour series might then be added to a correspondingly weighted man-hour series to measure physical input for the economic system."
>
> "..(a) divergence is likely to appear between the movements of a series representing the physical volume of output (and input), and ... this divergence is a rough measure of changes in the efficiency of our economic system" (p.112).

In commenting on this paper, Milton Friedman, in one of his earliest appearances on the scene, interprets the authors as saying "that a comparison of the two types of indices (output and input) provides a basis for estimating the degree of technical change" (p.126).[5] Hicks, while considering a similar approach to "efficiency" measurement in 1940, added an explicit discussion of index number biases, noting that the problem "is much worse when we allow for increasing returns and imperfect competition" (p.121). He also asked whether taxes should be included in the weights (yes for output indexes, no for input).

Actual measurement was initiated by several people, working independently but subject to the same intellectual milieu. Whether they measured it as a shifter of the production function (Tinbergen, 1942; Tintner, 1944; Johnson, 1950; Solow, 1957) or as an output-over-total-input index (Stigler, 1947; Barton and Cooper, 1948; Schmookler, 1952; Fabricant, 1954; Ruttan, 1954 and 1956; Kendrick, 1955 and 1956; Abramovitz, 1956), they did not claim any particular originality for it. They were making illuminating calculations for a concept that was obviously already there. Credit for the earliest explicit calculation belongs clearly to Tinbergen, who in a 1942 paper, published in German, generalizes the Cobb-Douglas production function by adding an exponential trend to it, intended to represent various "technical developments".[6] He computed the average value of this trend component, calling it a measure of "efficiency", for four countries: Germany, Great Britain, France, and the U.S., using the formula $t = y - 2/3n - 1/3k$, where y, n, and k are the average growth rates of output, labor, and capital respectively and the weights are taken explicitly from Douglas. Note how close this is to Solow who will let these weights change, shifting the index number formula from a fixed-weight geometric to an approximate Divisia form. Nobody seems to have been aware of Tinbergen's paper in the U.S. until much later.[7]

The developments in the U.S. originated primarily at the NBER where a program of constructing national income and "real" output series under the leadership of Simon Kuznets was expanded also to include capital series for major sectors of the economy, with contributions by Creamer, Fabricant, Goldsmith, Tostlebe and others. It seemed reasonably obvious to try and use such capital numbers in a more general productivity calculation.[8] The first such published calculation appears in 1947 in Stigler's book on *Trends in Output and Employment* where, after working pretty hard on the output and employment data, he presents (on p.49), off-handedly, what looks like a "back of the envelope" calculation of efficiency, which he says "is usually defined as Output/(Input of Labor+Input of Other Resources)", for 12 manufacturing industries. In 1952, Jacob Schmookler, who had been Kuznets' student at the University of Pennsylvania, publishes a detailed article on the "Changing Efficiency of the American Economy" in which he constructs an output over total input index with the intent "to describe the pattern and magnitude of technical change".[9]

The "raw-materials" assembled by the NBER were used repeatedly in subsequent studies and in 1953 Kendrick was asked to systematize and develop this line of measurement more explicitly. Calculations based on his preliminary work were made by Fabricant in 1954, who may have been the first to emphasize loudly that most of the growth in output per unit of input has not been explained and hence, "it follows that the major source of our economic advance has been a vastly improved efficiency" (p.8). In 1956, a much more detailed analysis of basically the same data was published by Abramovitz, who identified productivity with his computed index of "output per unit of utilized resources" and observed that "the lopsided importance which it appears to give to productivity increase" (in accounting for the growth in output per manhour) "should be... sobering, if not discouraging, to students of economic growth" and labeled the resulting index "a measure of our ignorance". Kendrick's own similar results were already reported in the 1955 NBER Annual Report (Kendrick 1955, pp. 44-47), but his magnum opus did not come out until 1961 (Kendrick, 1961) and in the end he was over-shadowed and did not get enough credit, in my opinion, either for providing the data to his "interpreters" or for the detailed data construction effort behind it. Some of these computations, and the parallel ones made for agriculture, are summarized in Table 1.

At the same time that the NBER was assembling data for the U.S. economy as a whole and for several of its major sectors, a parallel measurement effort was proceeding at the Bureau of Agricultural Economics of the USDA directed at the measurement of farm output and efficiency. In 1948, Barton and Cooper published a fully articulated and detailed output per unit of input index for the U.S. agriculture. Without computing such an index explicitly, Johnson (1950) used their data to estimate the magnitude of the shifts in the aggregate agricultural production function in different periods, based on weights from estimated production functions, both linear and linear in the logarithms of the variables.[10] Schultz (1953) used the Barton and Cooper index to compute the return to public investments in agricultural research. Ruttan (1954), as part of his dissertation at the University of Chicago, constructed linear and geometric output over input indexes for meat packing plants, interpreting these indexes as approximations to shifts in the underlying production function. He used base and end period weights to bound them, and included a reasonably complete discussion of the potential biases in such a construction. An extension of this work to the measurement of "The Contribution of Technological Progress to Farm Output" was published in Ruttan (1956).

Against this historical background the 1957 paper by Solow may appear to be less original than it really was. Neither the question, the data, nor the conclusion were new. Nor did using a geometric input index with shifting weights affect the results all that much. What was new and opportune in it, the "new wrinkle" (p. 312), was the explicit integration of economic theory into such a calculation and the use of calculus, which by then was being taught to most graduate students. It showed that one need not assume stable production function coefficients to make such calculations and provided an approximation formula for any constant returns production function and, by implication, also an interpretation of the earlier work that did not use this formula. This clarified the meaning of what were heretofore relatively arcane index number calculations and brought the subject from the periphery of the field to the center. It also connected it, indirectly, to Solow (1956) and growth theory and macroeconomics as it was to develop subsequently, and had an immense influence on subsequent work in both macro and micro economics.

All of the pioneers of this subject were quite clear about the tenuousness of such calculations and that it may be misleading to identify the results as "pure" measures of technical progress. Abramovitz worried about possible measurement errors in his labor and capital series, especially the omission of intangible capital accumulation through education, nutrition, and R&D, and also about not allowing for increasing returns to scale. Kendrick (1956) noted the omission of intangible capital, such as R&D, from his total input construction. Solow emphasized that he used "the phrase 'technical change' for **any kind of shift** in the production function" (emphasis in the original). He commented on the absence of good measures of capital utilization and he credited Schultz for "a heightened awareness that a lot of what appears as shifts in the production function must represent improvement in the quality of labor input, and therefore a result of real capital formation of an important kind" (fn.6). Schultz, as mentioned earlier, actually attributed such numbers to public R&D and used them to compute a rate of return to them, influencing my subsequent work on returns to hybrid corn research (Griliches, 1958). Most writers echoed Abramovitz's conclusion that such calculations should be interpreted, primarily, as providing an "indication of where we need to concentrate our attention."

At this point the gauntlet had been thrown: even though it had been named "efficiency", "technical change", or most accurately a "measure of our ignorance", much of observed economic growth remained unexplained. It was now the turn of the explainers.[11]

**Notes**

1.This tradition and the data bases it developed, together with the development of Keynsian economics, also contributed to the rise of growth theory in the works of Harrod, Domar, and Solow. But that is a different story.

2. Douglas had been criticized earlier for not allowing for some kind of trend factor in his estimation equation, especially by Mendershausen (1938), a criticism that Tinbergen endorsed in his *Econometrie* textbook, published in Dutch in 1941. Given the increasing availability of time series data and the general advice of that time to use "de-trended" data, it was not long before trend variables began to appear in production function estimation. The first one do so, as far as I can tell, was Victor Smith in 1940, in his Northwestern Ph.D. dissertation on the productivity of the steel industry, followed by Tintner in a number of papers based on data for the U.S. agriculture (Tintner 1944 and 1946).

3. More thorough research may unearth even earlier references.

4. "Income derived from an area may be deflated to show changes in the physical volume of services of labor and wealth employed by the economic system.....the deflated distributive shares may be compared with the deflated consumed and saved income to show changes in the efficiency of operation of the economic system" (Copeland 1937, p. 31).

5. Friedman also raised a series of doubts about the empirical feasibility of this approach. After discussing the substitution bias that is implicit in any fixed weight index construction, he goes on to say:

.." Add to this (the substitution bias) the necessity of assuming 'constant tastes', if the comparison is to be meaningful, and the difficulty of obtaining an adequate measure of the quantity of capital,...., as well as the lesser difficulties with the other factors of production, and the possibility of actually employing the procedure suggested by .. Copeland and Martin seems exceedingly small.

The derivation of a measure of 'real input' that would provide an adequate basis for measuring changes in economic efficiency is even more complicated and difficult than the measurement of 'real output'; for the former involves the latter and other difficulties as well. ....We can....ask the question-to what extent is the change in output over some specified period a result of a change in the quantity of available resources,..(or) the way these resources are employed. (This separation is to a considerable extent artificial: technological change affects not only the way in which resources are employed but also the quantity and the character of the resources themselves.) In order to answer this question it would be necessary to determine the volume of 'real output' that **would** have been produced had the techniques remained unchanged. A comparison of this series with the actual 'real output' then provides a measure of the change in efficiency" (loc.cit., 126-7).

Copeland and Martin responded:

"The measurement difficulties about which Mr. Friedman is concerned do not seem to have deterred others to the same extent. Dr. Kuznets has already provided measures of deflated national income in an output sense. Dr. Kuznets' measures of capital formation necessarily involve measurements of the quantities of all kinds of capital...Moreover,..estimates of total man-years of employment have been developed. Thus...the two main elements for measurements of changes in social input (except..for non-reproducible wealth..) are admittedly at hand.

It must ..be conceded that (such) measurements .. are certain to be rough under present conditions. However, those who insist on a high degree of precision had best choose some field of activity other than estimating national wealth and income" (p.134).

Remember, this discussion is taking place in 1937!

6. The fact that an occupied Dutchman was publishing in Germany in 1942 caused some comment after the war. This was an example of trying to keep "science" going despite the circumstances. A position that was tenable in 1942 when much of what was happening was not yet widely known.

7. Valvanis-Vail (1955) mentions Tinbergen but, it is obvious from the context, not this paper. Following a suggestion attributed to Arnold Harberger, he uses average factor shares to compute a Cobb-Douglas type total input index and a residual which he then uses to estimate the trend coefficient for an aggregate production function, yielding an estimate of 0.75 percent per year for the 1869-1953 period. This is essentially equivalent to what Tinbergen did, but with weights based on income shares rather than estimated production function coefficients. Solow does reference Valvanis-Vail, but obviously neither of them is aware of Tinbergen (1942) at that point.

8. This was clearly foreshadowed in the earlier exchange between Copeland and Friedman quoted above.

9. There are two major components in Schmookler's dissertation on which this article was based. The primary task of the thesis was the assembly and examination of a consistent series on patenting in the U.S., interpreting it as a measure of inventive activity. This part was published as Schmookler (1954). It is clear that the intent was to bring these two parts together, with the patent series "explaining" the

growth in input-over-output series. In a third, unpublished, chapter Schmookler tries to do just that and gets nothing. It is interesting to note that as the result of this outcome he left the productivity measurement field and concentrated on the analysis of patent data, perhaps feeling that they are closer to and a more tangible reflection of the actual processes of invention and innovation that he wanted to study. A generation later I will pursue the same mirage (Griliches 1990).

10. Johnson credits Tintner with stimulating his interest in this approach (personal communication).

11. The search for "explanations" of such residual measures of productivity growth (technical change) will be the topic of the sequel to this note.

Table 1

Early Estimates of the "Residual" in U.S. Growth.

Percent of Growth Not Accounted for by Conventional Inputs

| Total Economy | | | | Agriculture | | |
|---|---|---|---|---|---|---|
| Source | Period | In Output | In Output per man or manhour | Source | Period | In Output |
| Tinbergen (1942) | 1870-1914 | 27 | 100 | Barton & Cooper (1948) | 1910-45 | 57 |
| Stigler (1947), Selected manuf. ind's. | 1904-1937 | n.a. | median 89 | Johnson (1950) | 1900-20<br>1923-29<br>1940-48 | 24<br>50<br>50 |
| | | | | Ruttan (1956) | 1910-50<br>beg. wts.<br>end wts. | <br>88<br>71 |
| Schmookler (1952) | 1869-1938 | 37 | n.a. | | | |
| manuf. | 1869-1928 | 31 | 88 | | | |
| Fabricant (1954) | 1870-1950 | n.a. | 92 | | | |
| Kendrick (1955) manuf. | 1899-1948 | | 87 | | | |
| Abramovitz (1956) | 1869-1878 to 1944-53 | 48 | 86 | | | |
| Solow (1957) | 1909-1949 | 52 | 88 | | | |

n.a. -- not available
beg. wts -- beginning period weights
end wts. -- end of period weights

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