Volume Title: Wesley Clair Mitchell: The Economic Scientist

Volume Author/Editor: Arthur F. Burns, ed.

Volume Publisher: NBER

Volume ISBN: 0-87014-052-3

Volume URL: http://www.nber.org/books/burn52-1

Publication Date: 1952

Chapter Title: The Economic Naturalist

Chapter Author: Edwin B. Wilson

Chapter URL: http://www.nber.org/chapters/c3103

Chapter pages in book: (p. 283 - 291)

The Economic Naturalist*

By Edwin B. Wilson

It is always difficult, almost to the point of impossibility, adequately to review a monograph by a great naturalist, whether it be by Roland Thaxter on Laboulbeniaceae or by W. M. Wheeler on ants or by Mitchell on business cycles. The difficulty is increased when the monograph is but one of a series, as yet incomplete, stretching over a large fraction of the working lifetime of the author and on into that of his successor. Arthur F. Burns, whose name Mitchell put first in the collaboration for the dual reason that the major part of the detail had fallen upon him and that he would have to carry on ever more independently in the future, already has had to assume his greater responsibilities with the death of Mitchell coming before the completion of a subsequent monograph announced as well along in the works before Mitchell died. We wish him Godspeed. May he never become a slave to the fulfillment of the business cycle project; for if he should, he would prove unworthy of the great confidence Mitchell placed in him, and untrue to the Mitchell tradition of independence in co-operation toward the major aim of advancing man's knowledge.

If in writing of this monograph I choose to refer chiefly to Mitchell, I am sure that Burns will consider it no slight but merely a privilege assumed by one who first came to know in some detail of the work on measuring business cycles when at Mitchell's request he spent an afternoon some twenty years ago in Mitchell's study on Twelfth Street, New York, going over, in only the superficial way one might in one afternoon, a part of the great mass of data already by that time collected and partly processed. It was the extent of the collection of data and Mitchell's attitude toward its treatment that convinced me that he was in this matter essen-

* Originally published in the *Quarterly Journal of Economics,* May 1950, as a review of *Measuring Business Cycles* by Arthur F. Burns and Wesley C. Mitchell. Here reprinted, with only minor changes, by permission of the author and publisher.

tially a naturalist—one who is at great pains to go out into the world of concrete and detailed fact, to "look see" what phenomena of a certain sort are really like, to find out in nature something that is new to knowledge. Few of us academic folk, held to schedules of Monday, Wednesday, Friday, 11-12 and 4-6, or the like, fully realize the trouble to which the naturalist will go to get his material and to put it into some sort of order suggested to the experienced eye by the similarities and differences among its items. Indeed such pains as are thus exemplified in this monograph are widely regarded as pathologic by those who take the "lazier way" of a priori theory[1] or of exhortation to social action.

The ways of the naturalist may be read in the works of von Humboldt or of Darwin, they may be glimpsed secondhand in writings of Victor von Hagen, or they may be seen today in the travels of the dean of American botanists, Liberty Hyde Bailey, who undertaking in the ninth and tenth decades of his life to write a monograph on the palms must be off at ninety to the Orinoco, to the Amazon, or to the Congo to see the palms in their natural habitat and to verify the accuracy of the designations of the dried specimens in the herbaria before he could be content to finish and release the volume he has in preparation. I realized that Mitchell was a naturalist as, years earlier, I had perceived the naturalist in William Graham Sumner when he showed me part of that great collection of over one hundred thousand items which he had culled from the literature (as he was not free to spend his time in wide-flung travels in the field) for use with his classes, for writing his *Folkways*, and for the subsequent extensive work on the Science of Society which he never lived to write, no more than Mitchell lived to complete his series on the business cycle. Better luck to L. H. Bailey.

The emphasis upon Mitchell as a naturalist is necessary; it is the key to this work, though not to all his work, for he knew economic theory and economic history and had a reasonable experience in the affairs of government.

I recall that, when Mitchell and I were discussing his material and its treatment, the question of how to handle such masses of data arose. The answer seemed to be that one had to try a

[1] With apologies to F. S. Oliver in his life of *Alexander Hamilton* (Putnam's Sons, 1906), pp. 239 and 247.

variety of methods and see what turned up rather than rely upon any particular choice in advance. Should one smooth the data and work from the smoothed series or should one work directly from the raw data? How tell, except by trying both ways? Should one undertake to prove by some mathematical method applied to the various series that there was extractable from them some general underlying cyclic phenomenon or should one take some simple average as a general underlying reference cyclic series and treat the individual or specific cycles relative thereto? Let us consider for a moment just these two items—smoothing and the selection of a reference average.

When in a given empirical series

$$\cdots, u_{-n}, \cdots, u_{-1}, u_0, u_1, \cdots, u_n, \cdots$$

each element as u_0 is replaced by a new value u'_0 by a formula of the general type

$$u'_0 = a_0u_0 + a_1(u_1 + u_{-1}) + \cdots + a_n(u_n + u_{-n}) \quad (1)$$

with

$$a_0 + 2a_1 + \cdots + 2a_n = 1\,, \quad (2)$$

these three things happen:

(1) A part of any term-by-term random element in the individual terms u is eliminated and does not appear in the terms u'. This elimination is statistical in the sense that if σ^2 be the variance (supposed uniform) of the random part of u, there will remain in the series u' only the reduced amount of random variance σ'^2 where

$$E^2 = \sigma'^2/\sigma^2 = a_0^2 + 2a_1^2 + \cdots + 2a_n^2. \quad (3)$$

The quantity E, the ratio of the standard deviations of the residual of the random element and of the original random element itself, may be called the coefficient of elimination of the smoothing process (1). That average which eliminates the greatest possible amount of the term-by-term random element of the original series u is the simple moving average, for which $a_i = 1/(2n + 1)$ and $E = 1/\sqrt{2n + 1}$.

(2) The residual randomness, with variance $E^2\sigma^2 = \sigma'^2$, left in the derived series u' is spread over $2n + 1$ consecutive terms of that

series so that the randomness is no longer termwise. One may best see in detail what happens by taking a series of purely random numbers (with zero mean) and applying to it the process (1) with a considerable value of n, say a simple moving average of 25 terms or Macaulay's ingenious 43-term which was in the works at the time.[2] The result will be a relatively smooth wavy curve which will show cycles, and thus may be something of a liability as well as something of an asset. The usual actuarial test for smoothness involves the ratio of the variances of the third differences of the derived series u' and of the original series u as

$$S^2 = \sigma^2(\Delta^3 u')/\sigma^2(\Delta^3 u), \tag{4}$$

where S is by definition the smoothing coefficient. For theoretical purposes the value of S is calculated from the smoothing process (1) in terms of the coefficients a_i; i.e., it is calculated for the random part of u; one must not expect to check the value of S by computing the ratio of the standard deviations of the third differences of u and u' unless u has been tabulated so closely that the third differences of its regular part are negligible.

(3) The regular part of the series u, i.e., the law set up by the series when abstraction is made from the termwise random element, will be deformed. Indeed if for the moment u denote merely the regular part, D denote differentiation, and u_i be expanded about u_0 by a Maclaurin series,

$$\begin{aligned} u_{\pm i} &= u_0 \pm iDu_0 + \frac{i^2}{2} D^2 u_0 \pm \cdots = e^{\pm iD} u_0 \\ u'_0 &= a_0 u_0 + a_1(e^D + e^{-D})u_0 + \cdots + a_n(e^{nD} + e^{-nD})u_0 \\ &= u_0 + (a_1 + 2^2 a_2 + \cdots + n^2 a_n) D^2 u_0 \\ &\quad + \frac{1}{12}(a_1 + 2^4 a_2 + \cdots + n^4 a_n) D^4 u_0 + \cdots . \end{aligned} \tag{5}$$

Thus the regular part of u will not be reproduced exactly by the application of any smoothing process (1), but will be little deformed if

$$(a_1 + 2^2 a_2 + \cdots + n^2 a_n) D^2 u_0$$

is small and u is tabulated at such frequent intervals of time that the higher terms in the development are negligible. If this latter condition holds, there may be said to be practically no deforma-

[2] F. R. Macaulay, *The Smoothing of Time Series* (National Bureau, 1931).

tion if (1) is such that

$$a_1 + 4a_2 + \cdots + n^2 a_n = 0 . \qquad (6)$$

What may happen in the attempt to make a satisfactory compromise between the inherently somewhat competitive requirements of getting a satisfactory diminution of the termwise random element and a satisfactorily smooth curve u' with a minimum distortion of the regular part of u may be illustrated by the following table for a seven-term process (1), i.e., with $n = 3$, where the coefficient of elimination E, the coefficient of smoothing S, and the coefficients of D^2 and D^4 in the expansion showing the distortional effects are set down, with corresponding results for Spencer's 21-term and Macaulay's 43-term formulas:[3]

	Coefficient E	Coefficient S	Coefficient of D^2	Coefficient of D^4
Best eliminator	.378	.111	2.0	1.17
Best smoother	.420	.024	1.17	.521
Best eliminator with (6)	.577	.194	0.0	−.429
Best smoother with (6)	.597	.115	0.0	−.294
Spencer's 21-term	.378	.0063	0.0	−12.6
Macaulay's 43-term	.333	.0020	0.084	32.9

It will be observed that for this simple case the running mean (best eliminator) reduces the random element little more than the best smoother while giving third differences four to five times as large and having a larger distortion. If we impose the condition that there be no distortion in the second order, we do not have such small coefficients of smoothing or elimination, as must be expected, but the distortion in the fourth order remains small. Note that Spencer's formula does not eliminate more of the random element than the plain 7-term moving average, has no second order but considerable fourth order distortion, and gives very smooth results. Macaulay's 43-term formula reduces the standard deviation of the random element to just ⅓ as would a 9-term moving average, and gives a very smooth result with very little distortion in the second order but a good deal in the fourth.

Realizing as we did the sorts of complication involved in theoretical considerations such as the above, and further taking into

[3] The results are taken from my lectures of a dozen years ago on topics in statistics of which advanced students of economics should have some awareness.

account the arithmetic work of smoothing, it was fairly clear to Mitchell and me, as our discussion progressed, that there was no way a priori to determine whether to smooth or not to smooth the series in studying methods of measuring business cycles; that the only scientific attitude was the experimental one of trying both, comparing the disadvantages and the advantages, and then deciding what to do. Ultimately, although Macaulay had developed his remarkable formula and it had been applied in detail to interest rates and somewhat to other series, the decision was, generally speaking, not to smooth but to work with the raw data. The discussion of this whole matter in Burns and Mitchell is excellent and highly germane to their problem; it could have been carried even further but probably not to any good purpose for most readers.

Let us now turn, and more briefly, to the consideration of eliciting, by some method akin to averaging, the general business cycle from the specific cycles. When I was talking with Mitchell some twenty years ago, I had just been much interested, and I may say intrigued, by the method of factor analysis on which Spearman and Godfrey Thomson and others had been writing and which has continued to this day to interest psychometrists. The problem was to isolate a general factor, the general intelligence, from various specific factors involved with it in a set of psychological tests. The method has since been modified and generalized in a variety of ways to the problem of isolating a number of general or group factors. Clearly, there must be at least a verbal analogy between general intelligence and general prosperity and between special abilities and the various specific economic activities.

One who was as much interested in factor analysis as I then was could not fail to suggest to Mitchell that it might be interesting, whether for the light that might be thrown on the economic series or for the contribution that might be made to the significance of the method itself, to try it out upon his series; but that is a very different thing from suggesting that the method be adopted and followed. There seems to be no trace of the method of factor analysis in Burns' and Mitchell's book—and perhaps that is just as well, for it would have taken a large effort adequately to try out the method, including the various modifications which have been suggested in the interim, and like as not the conclusion would have

been that its limitations for the problems in hand were too great. That was my judgment at the time and still remains my judgment, which I should still like to see substantiated or thrown out by trial which I still could not recommend as likely to be really worth the candle.[4]

The upshot of the efforts to put into some meaningful order the large number of ups and downs in a large number of economic series, domestic and foreign, seems to have been to use relatively simple graphical and arithmetical procedures with patience and circumspection. This is good scientific method in the descriptive, taxonomic, exploratory stage when one is trying to find out how to measure and what to measure. It is real statistics, for statistics has to do with description and the statistician is one who by some know-how has a feeling for masses of data and senses how he can make them talk to him. Whether they have told him what they will tell others who later try to make order out of such series, whether the relatively simple methods will continue to give results or whether they will have to be further simplified or rendered far more complicated are questions which only the future can decide. The authors do not claim much:

> The more we have studied business cycles the more we have become convinced of both the importance and the difficulty of determining reliably what cyclical behavior has been characteristic of different economic activities. Theorists sometimes wrangle about questions of fact as if they were problems in metaphysics. Whether and how wage rates conform to business cycles is a question of fact; so also with building construction, savings, interest rates, and other economic factors. No speculative solution can have any meaning, except as a hypothesis to be tested. To settle these questions of fact, statistics must be marshaled with scientific care, they must be analyzed with the aid of expert knowledge of business processes, and they must be tested for consistency with other leading facts[5] (pp. 506-507).

[4] The difficulty for the psychologist in interpreting the results of factor analysis as representing something in itself significant rather than merely suggestive may be seen by perusing Sir Godfrey Thomson's address "The Nature of the Mind's 'Factors' " as president of the section of psychology of the British Association for the Advancement of Science, 1949.

[5] In discussing method with Mitchell I made no reference to periodogram analysis, which was well known at the time, because I had no confidence that anything of value could be contributed by it to Mitchell's problem. And I made

It is of interest that the authors' pattern of cyclic behavior and Edwin Frickey's in his *Economic Fluctuations in the United States,* obtained independently and by somewhat different lines of attack, are largely corroborative (pp. 111-113). On the other hand it appears that Burns and Mitchell have failed to find close confirmation of the 3-cycle schema—of Kondratieffs of 57 years, Juglars of 9½ years and Kitchins of 3⅓ years, which Schumpeter puts at the basis of his two-volume work on *Business Cycles;* yet they do find some general resemblances in pattern (Ch. 11, Sec. IV). We seem to recall that Schumpeter closed his treatise a decade ago with a section on "the disappointing Juglar"; we wonder whether, had he come to close it only now, he would have had a section on "the premature Kondratieff" or whether he would take it that we are now in an overenthusiastic Juglar with the real Kondratieff peak still to come some quarter of a century hence.

The book under review will not please those who dislike or depreciate masses of statistical materials simply compared to reveal likenesses and dissimilarities, as the systematic naturalist may compare the varieties, species and genera with which he works, but who can admire only neat probabilistic models or can be intrigued only by elaborate systems of deductive theory or consider that the only important contribution to economics is ethical. The book was not for them, its universe of discourse is not theirs—nor is it against them; if they can but bear with it, they will find scattered through its pages not a few observations, not a few suggestions on which they may profitably reflect. To the statistician who is not overwhelmed with a need for exercising advanced mathematics out of season, it is a mine of information with many a discussion as to what to do and what not to do in the treatment of such materials.

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no reference to correlogram analysis and autoregressive systems which had then recently been proposed by Yule as a method of analyzing time series because I had made no intensive study of Yule's approach. Later I applied periodogram analysis, with more circumspection than is usual, to Ayres' Index of American Business Activity, (*Quarterly Journal of Economics,* May 1934, pp. 375-417; *Science,* August 31, 1934, pp. 193-199); and came to the conclusion that although Ayres' Index undoubtedly showed cycles, it apparently did not show periods. This finding seems to me to be corroborated in a general way, though not specifically, by the findings of Burns and Mitchell in this monograph.

I began this review with the statement that to review a monograph of this sort was difficult almost to the point of impossibility. A perusal of the reviews I have seen justifies that statement. Many of the reviewers seem not to appreciate what the book is really about. It is therefore with special interest that I found in the February 1950 issue of the *Quarterly Journal of Economics* a critical analysis by J. A. Schumpeter of the work of Wesley Clair Mitchell in which, among many other matters, there is an excellent explanation of the significance of the volume in relation to Mitchell's whole lifework.[6]

We await the next monograph in the series with keen interest and in confidence that it, too, will be a masterly contribution.

[6] Schumpeter's paper is reprinted in this volume; see pp. 321-340.