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Volume Author/Editor: Wesley Clair Mitchell

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Chapter Author: Wesley Clair Mitchell

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**Part I**

**AIMS, METHODS, AND MATERIALS**



## CHAPTER 1

### *The Task Essayed by the National Bureau*

Business cycles had been studied with increasing thoroughness for many years before the National Bureau entered the field. The basic observation that commercial crises recur at somewhat regular intervals was made at least as early as 1833. Historians had demonstrated that these crises have common features as well as innumerable differences of detail. The growth of statistical recording had provided fairly precise knowledge of changes in an expanding range of economic activities. In these time series several types of movements had been identified, and rough methods devised for segregating them. The original problem of explaining commercial crises had been reformulated to embrace the full cycle of changes in which a crisis is merely the most dramatic phase. But, despite these advances, theorists who sought to explain business cycles were insufficiently informed about the phenomena with which they were dealing, and had to reason for the most part from untested assumptions. The complexity of economic organization suggested to ingenious minds an embarrassing array of plausible hypotheses. No one could determine which among these jostling competitors was least inadequate, or whether any combination of them would account for what happens.

The study of business cycles lingered in the speculative stage, not because economists were averse to 'inductive verification', but because this type of testing was so hard to apply to a consensus among differing fluctuations in the many activities of a modern economy. Of the activities to be considered, relatively few were statistically recorded before 1900. What time series were available required laborious analysis before

they could be utilized. Economists were accustomed to work single handed, and no individual was able to collect the masses of raw data pertinent to the study of cyclical behavior, segregate the cyclical components from movements of other sorts, and assemble the findings to form a realistic model of business cycles by which explanations could be judged. A few investigators did what they could in this direction, but their best was inadequate.<sup>1</sup>

In its second year the National Bureau decided to devote part of its resources to this pressing task. We had a staff of several investigators with complementary skills, we could employ compilers and computers, we hoped to keep at the job long enough to learn how to do it. A group with these advantages should at least be able to perform more of the necessary spade work than any individual. Realizing full well that we could achieve no more than an approximation to the knowledge needed, we thought that anything we might learn concerning what happens during a business cycle should be useful to all who were trying to find out why nations practicing private enterprise fail so lamentably to make full use of their resources all of the time.

Yet we did not organize our program as a series of efforts to test current explanations of business cycles.<sup>2</sup> Our interest cen-

<sup>1</sup> As one of those who had tried to observe cyclical movements systematically, I can appreciate the prudence of men who relied upon common impressions, vague as they were. My own effort (*Business Cycles*, University of California Press, 1913) now seems to me sadly deficient on the factual side, and therefore of uncertain value theoretically.

<sup>2</sup> At least two projects of this sort have been started, but neither was completed. In 1926 Warren M. Persons began a "series of papers" in which he planned "to examine critically the theories of business fluctuations". An investigator so addicted to quantitative work presumably had statistical testing in mind. Unfortunately, Persons did not carry out his plan. Only the first paper appeared, and that was confined to the classification of 'theories'. ("Theories of Business Fluctuations", *Quarterly Journal of Economics*, XLI, Nov. 1926, pp. 94-128).

Ten years later the Economic Intelligence Service of the League of Nations undertook an extended inquiry "into the causes of the recurrence of periods of economic depression". With the aid of leading specialists from several countries, Gottfried Haberler analyzed "existing theories of the business

tered in the phenomena that should be explained. If our studies brought to light facts not previously known, or set dimly apprehended relations in clearer perspective, fresh explanations might be called for. A new theoretical structure founded on more exact and extensive knowledge would doubtless incorporate some familiar hypotheses; but we could not tell in advance what the ground plan of the new building would be. To us, the existing explanations were guides to research, not objects of research, and all the more useful because they pointed in so many directions.

That our undertaking grew as we worked on it will surprise no one familiar with empirical studies. After years of continuous effort, we have just reached a stage at which we venture to report some of our findings regarding the broad characteristics of business cycles. Even now what we can say is ill proportioned, tentative, and subject to change as the investigation proceeds.

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cycle" and derived from them "a synthetic account of the nature and possible causes of economic fluctuations". Haberler's *Prosperity and Depression: A Theoretical Analysis of Cyclical Movements* (Geneva, 1st edition, 1937) was an excellent beginning; but "the next stage in this investigation—the application, as far as possible, of quantitative tests to the various causal hypotheses" was a much more formidable undertaking. Jan Tinbergen's two monographs, *Statistical Testing of Business-Cycle Theories: A Method and Its Application to Investment Activity* and *Business Cycles in the United States of America, 1919–1932* (both Geneva, 1939), apply multiple correlation analysis to test several hypotheses concerning the interrelations among cyclical fluctuations in different activities. The work is notable for its blend of statistical skill with theoretical finesse, and the cautiously stated conclusions are highly suggestive. But Tinbergen could cover only a few hypotheses concerning a few activities during a few cycles. Further, he relied upon annual data, which are sadly deficient for his purpose.