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WHY DIDN'T ECONOMISTS PREDICT THE GREAT DEPRESSION?

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ABSTRACT

Economists failed to forecast the Great Depression, perhaps because they had lacked reason to theorize enough about business cycles. Since theory is a public good, the market produces too little of it. The prospect of *ex post* fame may induce theory; but fame comes from explaining famous events, not from averting adverse events. Also, learning-by-doing induces theory by cutting its cost, favoring the first theories to be developed. These dealt with markets – not business cycles – in the decades before the Depression. (Keywords: Great Depression, theory of business cycles, history of macroeconomic thought, marketplace of ideas, learning by doing. *JEL* classifications: B10, E32)

I. INTRODUCTION

The Great Depression bemused economists of the day. “The slump in trade and employment and the business losses which are being incurred,” John Maynard Keynes (1963, p. 148) said in 1931, “are as bad as the worst which have ever occurred in the modern history of the world.”

Economists were caught off-guard. In late October 1929, the Harvard Economic Society had announced that “despite its severity, we believe that the slump in stock prices will prove an intermediate movement and not the precursor of a business depression such as would entail prolonged further liquidation” (Allen 2000, p. 280-1). Irving Fisher had said on October 17 that New York stock prices were on “what looks like a permanently high plateau” (Allen 2000, p. 281).

The lack of explanations for the Depression troubled economists as much as did the magnitude of the event. “Most monetary theorists,” wrote Lionel Robbins (1931, p. xii),

seem to have failed utterly to apprehend correctly the nature of the forces operative in America before the coming of depression, thinking apparently that the relative stability of the price level indicated a state of affairs necessarily free from injurious monetary influences.

Milton Friedman (1968, p. 1) noted that, before Keynes, work on monetary policy amounted to “theory by aphorism.” Friedrich Hayek (1931, p. 2) also blamed shallow theories:

...If it were asked whether understanding of the connection between money and prices has made great progress during these years [of the Great Depression], at any rate until very recently, or whether the generally accepted doctrines on this point have progressed far beyond what was generally known a hundred years ago, I should be inclined to answer in the negative.

As for employment, “the fundamental theory underlying it has been deemed so simple and obvious that it has received, at the most, a bare mention,” wrote Keynes (1991, pp. 4-5) in 1936. One thinks of John Stuart Mill’s warning in *On liberty* that undebated ideas devolve into meaningless shibboleths (Bentham and Mill 1973). To Keynes (1991, p. 7) in 1936, “the only detailed account of the classical theory of employment” was Arthur Cecil Pigou’s *Theory of unemployment*.

Six years earlier, Keynes (1950: pp. 405-6) had written the *Treatise on money* out of discontent with the usual monetary explanation of the depression unfolding:

...When we turn to the work of economists, whose proper business is ‘the abstract thinking of the world,’ it is

noticeable how little serious writing on monetary theory there is to be found anywhere, prior to the stirrings of the last few years....Half-baked theory is not of much value in practice, though it may be half-way towards final perfection.

Some blame attached to Keynes' former teacher Alfred Marshall, who "in his anxiety to push economic theory on to the point where it regains contact with the real world, was a little disposed sometimes to camouflage the essentially static nature of his equilibrium theory..." (Keynes 1950, pp. 406-7). Most economists of the day assumed a static equilibrium, treating fluctuations as essentially random (Schumpeter 1994, pp. 1132-3). But the business cycle *was* fluctuation. "...As long as we do not see much ground for believing that the economic system produces general fluctuations by virtue of its own logic, we may easily conclude that these fluctuations arise simply whenever something of sufficient importance goes wrong, no matter for what reason," wrote Schumpeter (page 1134).

People demanded business-cycle theory only after events suggested that existing models were inadequate. Mainstream economists had predicted that the falling prices of the Depression would revive demand for goods, re-creating jobs. But the price level in the United States fell by a third over four years without triggering strong recovery. When Keynes' critique of classical economics, *The general theory of employment, interest and money*, came out in 1936, graduate students of economics snapped up copies. Its "success...was merely symptomatic of, or perhaps helped decisively, the displacement of

what is called microeconomics with macroeconomics,” Hayek (1995, p. 60) wrote in 1963.

The question posed in 2008, about the financial crash, would also have resonated in 1933: Why hadn’t economists anticipated the most important downturn of the century? I will argue that economists shied away from producing macroeconomic theory because they did not expect to capture enough operating benefits to cover fixed costs. Also, theory is subject to scale economies: The more ideas available, the easier to create another, by building upon existing ideas (Schumpeter 1962). Early breakthroughs in microeconomics had attracted theorists away from macro.

Spillovers and scale economies affect all theory. The failure to predict the Great Depression in particular was also due in part to a self-unfulfilling prophecy: Had the works of economists anticipated the Depression, chances would have increased for averting it.

Here’s a preview of the rest of the paper. A brief history of macroeconomic thought indicates the importance of two factors in the supply of theory: The number of theorists and the amount of effort by each (Section II; Appendix A offers a mathematical restatement). Both factors may be inhibited by the nature of theory as a public good. A nonmarket incentive for theorizing – the prospect of fame – may increase the supply of theory for likely events but decrease it for unlikely ones (Section III). Scale economies in theorizing may also reallocate effort away from models of unlikely events (Appendix B provides the math). Likely events attract theorists in the beginning, because of the prospect of earning fame. The growing corpus of theory about those events lowers the

marginal cost of contributing to it, thus attracting more work. Finally, market power in the theory-producing industry may discourage entrants (Section IV).

II. HISTORICAL BACKGROUND

The reluctance to innovate in theory is of long standing and may help explain why, by the late 19th century, macroeconomic theory had become the neglected stepchild of economics. Early economists focused on economic growth or crises, not on business cycles. The lack of a widely accepted theory of cycles by the late 1800s prevented economists then from synthesizing the macroeconomic insights of the first half of the century. Meanwhile, the more rapid development of microeconomics was diverting theorists away from macroeconomics.

A narrative of narratives

Early economists took more interest in economic growth than in the business cycle, perhaps partly because of the growing populations of the day. Adam Smith (1976), who had devoted the brief Book III of *The wealth of nations* to “the different Opulence of Progress in different Nations,” regarded real investment as a spur to growth. François Quesnay (1948, p. 341), writing in 18th-century France, attributed growth of a national economy to agriculture since this was the only industry that could generate a net product. A contemporary, Anne Robert Jacques Turgot (1948, p. 353), was even more explicit

about the economic primacy of land. In 1817, Ricardo (1973, p. 95) focused on economic growth, manifest in

the increase of population...the extension of agriculture...the increase of shipping and manufactures...the building of docks...the opening of numerous canals, as well as...many other expensive undertakings; all denoting an increase both of capital and of annual production.

The focus of early growth theorists on particular inputs might have disposed later work toward microeconomics.

When economists of the late 18th century studied recessions and recoveries, they focused on commercial crises and precipitating incidents rather than on business cycles and general causes (Miller 1924, pp. 294-5). Changes in the price level mattered little to households of the mid-1700s since they produced largely for themselves (Miller 1924, pp. 313-4). This may have decreased demand at that time for theories of inflation.

Concerning business cycles, 18th-century economists did offer scattered insights about methodology, fallacies, and specific topics – especially money – that macroeconomists would exploit more than a century later.

Methodology. Physical analogies to cycles date back at least to Quesnay's analysis in 1758 of a stable equilibrium, which had drawn upon William Harvey's studies of blood circulation a century before (Schumpeter 1994, p. 240). (Quesnay had been a surgeon

(Monroe 1948, p. 340).) A few years later, Turgot (1948, pp. 365 and 371) likened the circulation of money in the economy to that of blood in the body; and the interest earned, to a thermometer.

Eighteenth-century macroeconomists also foreshadowed dynamic analysis. Quesnay (1948) roughed out the income multiplier that would become a staple of Keynesian models nearly two centuries later.² Turgot (1948, pp. 352 and 362-4) saw in 1766 that lengthy production required advances to input owners.

Anticipated fallacies. Early economists identified some errors in macroeconomic reasoning that would persist into the 20th century. In a 1752 attack on mercantilism, David Hume (1948, pp. 330) described a fallacy of composition that would become familiar in distinctions between changes in relative prices and those in the price level. He was criticizing the precept that a nation grows wealthier by accumulating gold and silver money:

We fancy, because an individual would be much richer, were his stock of money doubled, that the same good effect would follow, were the money of every one increased; not considering, that this would raise as much the price of every commodity, and reduce every man, in time, to the same condition as before.

Topics. Early economists broke ground concerning an issue that was newsworthy in their time -- the nature of money and interest. Ricardo (2002, pp. 261-301) provided elements of the quantity theory of money in 1811. Henry Thornton, an English economist and member of Parliament, distinguished between real and nominal rates of

interest, as well as between the market rate of interest and the “natural” rate that equaled the expected rate of return to a dollar of physical investment (Blaug 1986, pp. 243-5).

Hume and Thornton anticipated the modern conclusion that money does not affect output in the long run because prices adjust until inputs are fully employed (Thornton 2010, pp. 216-9; Niehans 1990, pp. 52-6 and p. 110). Thornton (2010, p. 263) quoted Hume’s conclusion in the “Essay on the balance of trade” that prices would adjust so as to “preserve money nearly proportionate to the art and industry of each nation.” Thornton (2010, pp. 219-22) also presaged the New Classical conclusion that an unanticipated increase in money may deceive producers into supplying more output. James Mill adumbrated neoclassical macroeconomics by arguing that prices were flexible enough to render money neutral in even the short run (Niehans 1990, p. 114). “The ‘neoclassical synthesis’ of the 1950s could almost (though not quite) have been achieved around 1830,” remarked Jürg Niehans (1990, p. 105).

Early theories of cycles. Early economists did not pull together such elements into a theory of the business cycle, perhaps because they had not detected a cycle in the real world. Prominent analyses of business cycles did not appear until the early 19th century. Perhaps they were partly induced by the economic convulsions of the Napoleonic wars. In 1819, a political economist from Geneva, Jean-Charles-Léonard Simonde de Sismondi (1991, pp. 101-7 and 303-7), wrote about business cycles partly because, he said, he was “deeply affected by the commercial crisis which Europe had experienced of late...” (Mitchell 1927, p. 5).

Not until mid-century did economists commonly recognize cycles in national economies (Hansen 1964, p. 229). Joseph Schumpeter (1962, pp. 40-41) credited Karl

Marx with discovery of the business cycle. *The Communist manifesto* of 1848 anticipated growing destruction in the “periodical return” of “commercial crises” (Marx and Engels 2002, pp. 225-6). Marx was not alone: In *Principles of political economy*, the economics textbook of choice at that time, John Stuart Mill (2006, pp. 741-2) wrote of “commercial revulsions” coming on the heels of speculation and excess capital. In 1867, John Mills concluded that an excessive, and eventually inimical, demand for loans occurred in almost every decade (Hansen 1964, p. 220).

In Schumpeter’s reckoning, the first detailed analysis of business cycles appeared in 1860, in Clément Juglar’s *Des crises commerciales et de leur retour périodique en France, en Angleterre et aux États-Unis* (Hansen 1964, p. 218). Juglar was the “ancestor” of business-cycle analysis, due to his work with time series (Schumpeter 1994, pp. 1124). His business cycle passed through prosperity, crisis and liquidation because of how people saved and invested. When investment fell short of savings, firms would go bankrupt because households wouldn’t spend excess savings – an idea familiar from Sismondi (Hansen 1964, pp. 218-9). Marx, too, blamed lapses in aggregate demand. Innovation triggers rising output and income, but the resulting increase in wages induces capitalists to substitute machines for labor. This leads to unemployment, reductions in consumption, and to recession.

Early economic theory might have provided the framework for business-cycle analysis that fact-gathering lacked, had 19th-century economists developed these first glimmerings of intuition. But aside from the work of Juglar, Marx and Sismondi, general analyses were unusual until the destructive depressions of the early 20th century.

“...Strange to say,” Schumpeter (1994, pp. 746-7) remarked about the 19th-century fragments of cycle theory,

nobody seems to have known all these bricks or to have understood that they were bricks awaiting the hand that would combine them in a comprehensive structure – comprehensive though provisional – before the period was out.

In the second half of the 1800s, many mainstream economists, newly obsessed with price theory and loath to take on issues that seemed intractable, ignored the business cycle. Relative to Marshall’s work on microeconomics in 1890, “no comparable body of scientific work had been done on business cycles and the problem of economic depressions,” wrote Alvin H. Hansen (1964, p. 225). By 1914, macroeconomists agreed that instable production of capital goods was vital to business cycles (Schumpeter 1994, p. 1125). But they balked at developing a compelling general theory.

In sum, until the Great Depression, no one theory of the business cycle dominated. Much work focused on excess supply (Overstone, John Stuart Mill, Marx, Tugan-Baranovski), and some on deficient demand (Sismondi and Juglar) and financial mistakes (John Mills, Spiethoff). “Upon high economic authority we have been told that the prevailing depression is due to over-consumption,” wrote Henry George in his *Progress and poverty* of 1879, “[and] upon equally high authority, that it is due to over-production...” (Hansen 1964, p. 221).

To some intellectual historians at the time, macroeconomic theory early in the 20th century had added little to the offerings of a century before. Schumpeter (1994, pp. 1122-3) wrote that “*all* the essential facts and ideas about business-cycle analysis had emerged by 1914...” (emphasis in the original). In Hayek’s eyes (1931, p. 2), any student of the monetary issues of the first half of the nineteenth century understood that “hardly any idea in contemporary monetary theory...was not known to one or more writers of that period.”

Monetary theory did not improve until global depression kindled interest. “When I look back to the early 1930s,” said Hayek (1995, p. 49) in 1963, “they appear to me much the most exciting period in the development of economic theory during this century” – not least, surely, because of his running controversy with Keynes. But Piero Sraffa (1932, p. 42) remarked about Hayek’s *Prices and production* that “there is one respect in which the lectures collected in this volume fully uphold the tradition which modern writers on money are rapidly establishing, that of unintelligibility.”

Conclusions

It took more than a century for macroeconomic theories of business cycles to catch on. Why? Schumpeter blamed “the personal element.”

Fairly satisfactory synthesis that would have left no major fact unaccounted for and would have constituted an excellent basis for further research was ‘objectively’

possible by [1914]. Why was it not attempted? The answer seems to be that objective possibility is one thing and its realization quite another thing: No more than any other history can the history of research afford to neglect the personal element. Entangled in controversy that was often petty, enamored of their own ideas and particular emphasis, economists plodded along successfully enough. But nobody rose to what would indeed have been a most difficult feat of leadership (Schumpeter 1994, p. 1135).

A difficulty in Schumpeter's argument may lie in explaining how a "personal element" can thwart research for decades.

I will consider two institutional reasons for the lack of progress in theory. First, theory is a public good, so markets will provide too little of it. Theory thus limits itself: It is hard to build upon ideas too scarce to find. Also, producing theory is subject to scale economies. Once microeconomics had accumulated critical mass, theorists would prefer it to macro, increasing the relative disadvantage of the latter.

III. THEORY AS A PUBLIC GOOD

Spillovers

A conventional market won't produce enough ideas, for reasons additional to the usual ones for public goods.¹ To sell an idea, its producer must normally disclose enticing features of it to the prospective buyer. This reduces its sales value which is based on its expected addition to the buyer's human capital *after* the purchase.

The buyer of an idea may be able to convey its essence to other people, preventing the producer from selling to them at a high price. The magnitude of this problem depends on the complexity of the idea. A film may be too intricate to convey in conversation.

Like any monopolist, the theorist may try to protect his rents by raising the fixed costs that rivals would incur in replicating his product. A model that uses advanced techniques – the mastery of which involves a time cost -- may reduce the number of rivals who otherwise could divert rents to themselves by replicating the analysis. Two characteristics of a good idea – clarity and simplicity – will quickly destroy its private rents by making it easy to understand and hence disseminate. Traits of a product that make it cheap to replicate may reduce the incentive to invent it.³

Even theorists who prefer influence to income may fail to capture spillovers.

Suppose that compensation is in the form of citations. The inventor of an enduring idea

¹ The producer cannot collect revenues from all consumers, in part because a unit of the good will not depreciate and thus limit its number of users, and in part because the producer cannot exclude non-paying consumers.

may find that these diminish over time as her concept becomes so universal as to be regarded public property.

In summary, the potential producer of a worthwhile idea may anticipate the loss of so much private value that she will decide not to develop it.

Reputation

To the problem of under-providing ideas, fame may be a non-market solution.⁴ The theorist's repute can reduce the amount of information that she must disclose to sell her idea. If she doesn't peddle the idea herself, then her fame may still enable her to command a higher salary from a college that will sell the information for her. The college has an incentive to act as an intermediary because its fame, which derives from that of its faculty, enables it to charge a premium to students who judge the quality of its education by its renown.

Fame depends on the size of the audience. In the short run, policy advocacy may interest more people than would more general analysis even if the latter includes the relevant news event as a special case. Schumpeter (1994, p. 185) regretted that he was not like Adam Smith, whose "argument and material were enlivened by advocacy which is after all what attracts a wider public...." For example, in 1802, Thornton wrote *An inquiry into the nature and effects of the paper credit of Great Britain* to criticize a much-bruited proposal to contract the money supply in order to reduce the gap by which the market price of gold exceeded the mint price (Thornton 2001, p. 3). If the national economy fades from the headlines, then macroeconomic theory may languish.

Theory that seeks to prevent some event – call it “pre-emptive theory” -- destroys its own reward. An accurate theory of economic collapse may help prevent that event. If the collapse never occurs, then it cannot make its theorist famous. A potential theorist aware of this paradox may pursue other lines more likely to pay off, such as advocating policies for events already in the news.

She need not be aware of the paradox for it to exist. Theorists who model events that will never occur shall be sidelined by analysts of actual events, since the latter attract more attention.

The university may be able to avoid this paradox by certifying the value of its faculty members for its customers (students and funding agencies), in exchange for an increase in tuition and overhead as well as a cut in faculty salaries. With its considerable experience, the university may be able to judge the quality of the pre-emptive theorist by various means, while the student can judge researchers only with respect to their work on events that he has heard of.⁵

Contemporary analyses of the Industrial Revolution illustrate how policy affects economic thought. After Parliament repealed the Corn Laws in 1846, macroeconomics gradually gave way to microeconomic analysis. How would market prices direct resources? “...During the last quarter of the 19th century and beyond,” writes Donald Winch (1973, pp. xvi-xvii), “there was a definite movement away from classical macro-dynamics towards the microeconomic problems of allocation and efficient use of resources.” It was as if macroeconomists, having freed international trade to spur growth, felt no need to study another aspect of the national economy, since none seemed as pragmatic.

IV. SCALE ECONOMIES IN DOING THEORY

As an industry, macroeconomic theory may have failed to attract producers because of high barriers to entry and of scale economies. The extended interest in microeconomics over most of the 19th century created a corpus of work that was relatively easy to supplement. This may have led theorists to prefer microeconomics to macro, increasing the gap between the marginal costs of creating the two. Scale economies may occur in the production of ideas, because adding an idea to a stock of $S - 1$ ideas generates $S - 1$ more (excluding the S th idea itself) in the form of new connections that themselves are ideas (Appendix A). For example, the differential analysis of Stanley Jevons and Carl Menger led to additional applications of calculus to microeconomics. How to extend the calculus to macroeconomics may have been less obvious.

Modeling techniques devised for microeconomics did not always suit macroeconomics. For example, optimization seemed more sensible for individuals than for organizations. Was it meaningful to claim that a government sought to maximize utility (or anything else)? Schumpeter (1908-9) criticized analysis of social welfare for presuming that an aggregate of individual preferences meant anything, since the preferences of one person could not be compared to those of another. In general, without a credible theory of organizational behavior, macroeconomic theorists would have been reduced to arbitrary assumptions – a requirement that some theorists might have found too distasteful to accept.

The structure of the economics market. Another reason for the lagged response of theory to events may have been that the market for economic ideas was an oligopoly. In the 1920s, 1930s and early 1940s, the four most-often-cited macroeconomists accounted for more than half of all citations by macro articles in journals. In the late 1930s, Keynes alone accounted for a third of the citations.⁶

Since theorists learn by doing, veterans have an edge. Enjoying scale economies, they may have felt little competitive pressure to address the relatively difficult issues of macroeconomics (difficult, because unexplored). Young macroeconomists may have spent much time searching for existing ideas. To Hayek (1995, p. 50), Keynes was just one of the “eager young men [who] were trying hard to find what of the work of other schools they could usefully incorporate into their local tradition” at not only Cambridge but also at “London, Harvard,...Vienna, and Stockholm, and a few Italian, French, and German universities” in the Twenties. All were improvising on the “great founders” – Marshall, Walras, Pareto and Carl Menger.

That a few economists dominated theory may have magnified the impact on it of random events affecting them. In 1895, Marshall had meant to put macro on a firm footing, as he had done for micro. The second volume of the *Principles of economics* would have addressed credit and employment. But by 1923, when he wrote *Money, credit and commerce*, the 80-year-old’s health was failing (Keynes 1951, pp. 209-10 and 214-5). He said his project, initially to be of three volumes, was “heavy, and achievement has been slow: Therefore it has seemed best to publish without further delay the present volume, which aims at accomplishing one-half of the task. A little progress has been made in regard to the second half...” (Marshall 2003, p. 6). Less than two

years later, Marshall died. Few, aside from his former students Keynes and Pigou, took up his macroeconomic cudgels.

V. CONCLUSIONS AND REFLECTIONS

Before the 1930s, relatively little analysis in economics concerned unemployment that persisted despite changes in the price level. The lack of macroeconomic theory vital to predicting depressions occurring early in the 20th century may have been due in part to the relative ease of producing microeconomic theory instead, to the lack of competitive pressure in developing business-cycle theory, and to a reluctance to spend time modeling events that may never occur.

The financial crisis of 2008 was almost shocking as the Great Depression. Perhaps such surprises are inevitable. If economists fashion theory only after the fact, then they may not anticipate a fact. Conversely, the lack of theory over time may lead to major downfalls because of cumulative ignorance.

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VII: Appendix A

A mathematical abstract of the main question – why we lacked business-cycle theory – may suggest factors to analyze. This section models the probability that economists develop no such theory. The model is simple since, at this point, only general features of the problem interest us.

X_i measures the unsuccessful effort by economist i to develop a macroeconomic theory. We need not specify the dimension of X_i . It may refer to the number of journal rejections or of weeks spent on research. X_i is a random variable independent of X_j for all $j \neq i$.

All n economists have the same background and thus the same probability p that any unit of X_i will succeed in producing a viable theory. The economists work independently of one another.

The probability that any unit of effort succeeds is independent of other units. Consider economist i . If her first unit of effort has a 1% chance of success, then so does her second unit. Completing her first unit does not enhance the chances of success of her next unit.

The probability that $X_i = k$ follows a binomial distribution:

$$p_{x_i}(k) = (1 - p)^k.$$

The sum of all failed attempts by economists to develop the given theory is

$$W = \sum_{i=1}^n X_i.$$

The probability distribution for W follows directly:

$$p_w(W = nk) = (1 - p)^{nk}.$$

The term nk measures the total unsuccessful effort to develop the theory. If 10 economists each devote two weeks to research, then the total failed effort is 20 weeks.

The probability of at least one success among the nk efforts is

$$V(p, n, k) = 1 - (1 - p)^{nk}.$$

Equation 1

This equation has an economic interpretation. The parameter n reflects the degree of actual (not potential) competition among suppliers of theory. The probability p reflects the stock of published macroeconomic theory; a greater stock makes additions to it more likely, by providing theorists with more material. Also, p reflects the extent to which learning-by-doing generates macroeconomic theory. Finally, k reflects the attraction of alternative lines of research such as microeconomic theory. An increase in the opportunity cost of writing business-cycle theory reduces the effort k that the theorist is willing to devote to it.

Equation 1 raises the possibility that changes in n or k , the power terms, will affect the probability of no success more strongly than will p itself. The first derivatives of V bear this suspicion out in most cases (see below). Critical elements in providing macroeconomic theory may include the number of theorists and the amount of work that each would devote to business cycles. The attraction of an alternative endeavor may affect n and k and thus the amount of macro theory published. The stock itself of macro theory may play a secondary role.

The second partials of V show diminishing returns to p , n and k . Perhaps early choices of the three parameters affect V , and consequently the stock of macro theory, more powerfully than do later choices.

In sum, this toy model suggests that the number of business-cycle theorists, and the amount of effort that each is willing to devote to his work, may help explain the lack of theory. The model confirms common sense.

Function V. The first derivatives of V are

$$\begin{aligned}\frac{dV}{dp} &= nk(1-p)^{nk-1}, \\ \frac{dV}{dn} &= -k(1-p)^{nk} \ln(1-p), \\ \frac{dV}{dk} &= -n(1-p)^{nk} \ln(1-p).\end{aligned}$$

Unless nk is very large, the last two derivatives will exceed the first, since

$$1-p < e^{1-p}$$

for all p , implying that

$$\ln(1-p) > 1-p.$$

Crude simulations suggest that the chances of at least one success are good as long as $n > 1$ and $k > 1$ (Table 1 below; note from Equation 1 that n has the same effect on the probability of at least one success as does k).

Simulations for model of Depression theory

p	n	k	<i>Prob of success</i>
0.5	1	3	0.875
0.5	2	3	0.984
0.5	3	3	0.998
0.5	4	3	1.000

Table 1

A more sophisticated model of $p(t)$ is as a logistic function of $p(t-1)$, due to scale economies in producing theory and to truncations at 0 and 1.

The second partials of V are

$$\frac{\partial^2 V}{\partial p^2} = -(nk - 1)nk(1 - p)^{nk-2} < 0,$$

$$\frac{\partial^2 V}{\partial n^2} = -k^2 [\ln(1 - p)]^2 (1 - p)^{nk} < 0, \text{ and}$$

$$\frac{\partial^2 V}{\partial k^2} = -n^2 [\ln(1 - p)]^2 (1 - p)^{nk} < 0.$$

In a more general case, a theorist may have s publications and k failures. The probability that this combination would hold for each of n theorists is

$$\left[\frac{(k + s)!}{k!s!} (1 - p)^k p^s \right]^n.$$

VIII. APPENDIX B

This section offers a theoretical example of scale economies in producing ideas.

Define an “idea” to consist of k elements, where an increase in k indicates greater complexity of the idea. The stock of ideas has n elements, from which k may be drawn. Assume a constant cost C in adding an element to the stock of ideas.

The number of ideas with k elements that can be created from the stock of n elements is the combinatorial sum

$$\frac{n!}{k!(n - k)!} = \binom{n}{k}.$$

The number of ideas that can be created by adding another element to the stock is

$$\binom{n+1}{k} - \binom{n}{k}.$$

The marginal cost of an idea is approximately

$$\frac{C}{\binom{n+1}{k} - \binom{n}{k}}.$$

In this model, the marginal cost of an idea falls as the stock of elements increases.

To see this, let us establish that

$$\binom{n+1}{k} - \binom{n}{k} = \binom{n}{k-1}.$$

We have that

$$\begin{aligned}
\binom{n}{k} + \binom{n}{k-1} &= \frac{n!}{k!(n-k)!} + \frac{n!}{(k-1)!(n-k+1)!} = \\
&= n!(k-1)! \left(\frac{(n-k+1)! + k(n-k)!}{k!(n-k)!(k-1)!(n-k+1)!} \right) \\
&= n!(k-1)!(n-k)! \left(\frac{(n-k+1) + k}{k!(n-k)!(k-1)!(n-k+1)!} \right) = \\
&= (n+1)! \left(\frac{(k-1)!(n-k)!}{k!(n-k)!(k-1)!(n-k+1)!} \right) = \\
&= \frac{(n+1)!}{k!(n-k+1)!} = \binom{n+1}{k}.
\end{aligned}$$

(Larsen and Marx (2006) offer a more elegant proof.)

The marginal cost of an idea is approximately

$$\frac{C}{\binom{n}{k-1}}.$$

For example, when k equals 4, the number of new ideas increases at an accelerating pace, from 120 ($n = 10$) to 165 ($n = 11$) and from 364 ($n = 14$) to 455 ($n = 15$). Accordingly, approximate marginal cost falls.

¹ For related discussions, I thank John Dixon, Nikolai Povetkin, and participants in a KIMEP seminar.

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² In Quesnay's (1948, pp. 340-8) model, expenditures on consumer goods are "sterile" because they leak out of the economy. Expenditures on inputs are "productive" because they regenerate output. All expenditures are either sterile or productive. Quesnay implies that income Y sums all rounds of spending on inputs. Denote the share of sterile expenditures in all expenditures as s . Given an initial expenditure on

inputs of X , Quesnay suggests (nonmathematically) that $Y = X + (1-s)X + (1-s)^2X + \dots (1-s)^nX + \dots$

Summing the geometric series yields $Y = X/s$.

³ Most monopolists can replicate their product more easily than their rivals can and so may threaten to flood the market before rivals can recover fixed costs. The threat may be credible if rivals would give up someday rather than remain poised for entry forever. For the theorist, the threat of flooding the market may not be credible. Given the Web, replication may have no fixed costs. The rival can copy and paste as quickly as the inventor. If the latter anticipates this Mexican standoff, then she may not try to develop her theory.

⁴ Fame is a non-market product in the sense that it cannot be bought and sold directly. Indirect transactions are possible. For example, a college may acquire well-known researchers to brighten its own reputation.

⁵ A general discussion of intermediaries that certify is in Paul Belleflamme and Martin Peitz (2010, pp. 662-8).

⁶ These estimates are derived from Tables 1.1-1.4 in Brian Snowdon and Howard R. Vane (1997a, pp. 4-5). Snowdon and Vane attribute the data to P. Deutscher (1990).