How saline is the Solow Residual?
Debating Real Business Cycles in the 1980s and 1990s

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1. Introduction

In a 1957 paper, Robert Solow exploited the mathematical properties of the aggregate production function to isolate the role of disembodied "technical change" in economic growth.[1] Solow's method allowed us to disentangle the role of technical change from that of production factors, with the residual serving as a measure of total factor productivity growth. Solow concluded that more than 87 percent of the growth rate could be attributed to technical change. His method and results were met equally with praise and criticism, some of which focused on the use of an aggregate production function, the residual composition, and measurement errors. The interrogations around the residual—considered by Moses Abramovitz (1956, 11) as "some sort of measure of our ignorance"—gave rise to an abundant literature from the late 1950s which made it possible to improve the technique of calculation and refine the results.

Long studied in the 1960s and 1970s as "the most important source of economic growth" (Griliches 1963, 331), the Solow residual was recycled in the mid-1980s within the debates on the short-term sources of impulses of economic fluctuations. Two distinct but eventually irreconcilable research programs carried out by Edward Prescott and Robert Hall opposed each other. Prescott, with Finn Kydland, held the Solow residual as a good measure of total factor productivity and believed that using the Solow residual to calibrate their model would provide clear evidence that technological shocks were driving the business cycle in an economy governed by perfect competition and constant returns to scale. Hall initially used the residual in an altogether different context to identify econometrically markup, that is, firms' ability to set prices above the marginal costs. This, he concluded, showed both that there was no such thing as perfect competition in the overall U.S. economy, and that the Solow residual captured measures of market power and increasing

[1] See Boianovsky and Hoover 2009 and Halsmayer 2014 for a history of Solow's 1956 and 1957 papers, and the ensuing debates.

returns to scale in addition to technical progress. According to Hall, Prescott's theory of real business cycles and empirical strategy were both flawed. Those antagonistic conclusions resulted in a flow of research aimed at discussing the empirical plausibility of the real business cycle hypothesis. Those debates often boiled down to competing interpretations of what the Solow residual captured. Its interpretation was closely tied to how economists explained the procyclicality of labor productivity, that is, why employees seemed to work more efficiently when the economy expanded. Supply-side and a variety of demand-side explanations, including not only imperfect competition but also labor hoarding, were offered.

In this article, I argue that the 1980s debates were not essentially different from those which took place in the 1950s and 1960s (regarding measurement issues, increasing returns to scale, and procyclical productivity). They both have blossomed within the National Bureau of Economic Research (NBER), they both highlight and are central to the macroeconomics controversies of the decade and its resolution in a 1990s consensus (as mentioned by Duarte 2015, 31). However, these debates—exhaustively studied by historians of economics (e.g., Duarte 2012; De Vroey 2016; Sergi 2017)—have been accompanied by a change in the "epistemic status of shocks" in economics (Duarte and Hoover 2009, 228), which redesigned the Solow residual from a source of secular growth to be quantified to the initial impulse of short-term economic fluctuations. In contrast to Louçã and Mata 2001 presenting the residual as a "black box," I allege that it was the ability to decompose the residual theoretically and empirically that made it a weapon in the war between those believed the business cycle was driven by supply-side factors versus demand-side factors:

> As a gross oversimplification, current thought can be divided into two schools. The freshwater view holds that fluctuations are largely attributable to supply shifts and that the government is essentially incapable of affecting the level of economic activity. The saltwater view holds shifts in demand responsible for fluctuations and thinks government policies (at least monetary policy) is capable of affecting demand. Needless to say, individual contributors vary across a spectrum of salinity. [Hall 1976, 1]

The stakes were thus high, as these alternative models of the business cycle were paired with radically diverging resulting policy prescriptions. Paradoxically, the way macroeconomists used the Solow residual also highlights heterogeneities in how "new Keynesian" economists understood friction-driven business cycles and the related propagation mechanisms. And it shows that Hall, who himself coined the freshwater/saltwater distinction, fit neither category, not unlike the sunspot theorists who tried to find a middle position on the saline spectrum (Cherrier and Saïdi 2018). Opposing economists' positions substantially converged in the 1990s with freshwater economists becoming more saline over time, integrating typical demand-side elements into their models, and saltwater gradually adopting calibration methods introduced by Kydland and Prescott (1982). As this "new neoclassical synthesis" became more widely adopted, debates around the procyclical residual and productivity gradually faded away and gave way to issues relating to imperfect competition, increasing returns, and real and nominal rigidities.

2. The Early Criticisms of the Solow Residual

When published in 1957, Robert Solow's seminal article introducing the so-called Solow residual contained no innovative results. Estimates of the contribution of technological progress to growth already existed (Crafts 2009), which concluded at proportions quite similar to what Solow found, almost 90 percent. The novelty of his approach was mainly due to the introduction into growth accounting of the aggregate production function, which he used to calculate the contribution to economic growth of the various production factors (restricted to capital K and labor L) and of disembodied "technical change" (A). Formally, assuming—as Solow did—that production is of the Cobb-Douglas type, the growth rate $\hat{Y}$ can be expressed as a weighted average of the growth rates of the two inputs, $\hat{K}$ and $\hat{L}$, and of the total factor productivity rate $\hat{A}$:

$$\hat{Y} = \hat{A} + \alpha\hat{K} + \beta\hat{L}, \tag{1}$$

where α and β stand for the output elasticities with respect to capital and labor respectively. To calculate the contribution of technical change (i.e. total factor productivity) to economic growth, Solow used two assumptions. The first concerned returns to scale, which were assumed to be constant, that is $\alpha + \beta = 1$. The second, relating to the competitive structure of markets, assumed that production factors were paid their marginal product and implies that coefficient α (resp. β) was measured by the share of labor (resp. capital) in national income. Under these two assumptions, Solow defined its residual as follows:

$$\varepsilon = \hat{Y} - \alpha\hat{K} - (1 - \alpha)\hat{L}. \tag{2}$$

If Solow's assumptions are correct and if the capital share is used to determine the value of α, the residual ε provides a good estimate of the total factor productivity growth $\hat{A}$.

This methodology, as well as its results, quickly attracted much criticism, initiated almost entirely by Warren Hogan (1958) in the *Review of Economics and Statistics*, where Solow published his article. A first series of criticisms was directed against the use of the aggregate production function. The reason the function used by Solow fit the empirical data so well, they concluded, was that his methodology was tantamount to regressing an national accounting identity.[2] The second series of criticisms, made by the defenders of Solow's methodology, many of whom were affiliated with the NBER and worked on productivity trends in the United States, challenged the leonine role conferred to technical progress in explaining economic growth.[3] They all considered the Solow residual a "catch-all" variable (an expression borrowed from, for example, Massell 1961 or Griliches 1963), which embraces any specification or estimation error that potentially impacts equation (2). Indeed, if the elasticities of the output are not wisely chosen, the Solow residual constitutes only a biased measure of technological progress. This is the case when there are economies of scale (Walters 1963), or when production factors are not paid their marginal product. This is also the case when a relevant variable is omitted from the set of potential sources of growth, or when the value of an input is not correctly measured. Debates around measurement issues mainly focused on capital (e.g., Jorgenson 1966), under the likely influence of the Cambridge controversy, even though questions relating to human capital or education were also discussed (Griliches 1963).[4]

This last series of criticisms led to a burst of work aimed at refining the measurement of technological progress. While some studies, such as Edward Denison's, did not substantially alter the conclusions drawn by Solow, others, such as Griliches and Jorgenson 1967, lead to technical progress being given a minority role in growth accounting once problems of data aggregation have been corrected. Criticisms relating to the production

[2] For a detailed survey of this literature and a presentation of the main arguments, see McCombie 2000.

[3] Abramovitz, Edward Denison, Solomon Fabricant, Zvi Griliches (who later became director of the Productivity and Technical Change program), Dale Jorgenson, John Kendrick, are part of these NBER researchers.

[4] The capital-embodied technological progress hypothesis, widely developed by Jorgenson (among many others), was immediately acknowledged by Solow in 1960.

function remained (with few exceptions) relatively ignored over time. By contrast, the second batch of criticisms has been flourishing. The total number of citations for Solow's 1957 paper has increased by fourteen per year on average throughout the 1960s, 1970s and in the first half of the 1980s.[5]

3. The Rise of Real Business Cycle Modeling and the Second Life of the Solow Residual

A notable resurgence of interest occurred in the mid-1980s, when the annual number of citations started oscillating between 20 and 37 (from 1986 to 1992), reaching 163 annual citations at the end of 2017. This rejuvenation of the Solow residual is the work of Kydland and Prescott that revamped the standard neoclassical growth model by introducing gestation in the production of new capital (time-to-build). In their stochastic version of the 1956 Solow model, the business cycle was driven only by "technological shock" affecting total factor productivity and causing shifts in the production function.[6] The shock was actually made of different components, including a constant parameter—capturing the growth rate of total factor productivity along the balanced growth path—and a serially correlated and highly persistent random process providing the necessary impulses to generate fluctuations around the long run trend.[7] This shock somewhat "combines growth and business cycle theory" according to Kydland and Prescott (1982, 1345), that gradually shifted the debate around the Solow residual (they used as a measure of the technology shocks) from the 1960s and 1970s questions about the sources of long-term growth to the controversies around the nature of short-term or high-frequency economic fluctuations (are they driven more by supply/real rather than demand/monetary components?).[8]

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[5] According to JSTOR, accessed August 2018. There were 14.1 citations on average for Solow's paper over the period 1961—70, as well as over the period 1971—80, and 14.6 citations per year on average over the period 1981—85. This number has fluctuated between 10 and 20 citations per year but remained relatively stable over the two and a half decades.

[6] Some earlier versions of the paper included monetary shocks, but Kydland and Prescott eventually found their explanatory power negligible. See Young 2014 for an exhaustive history of how the paper came to be.

[7] See Duarte and Hoover 2012 on the polysemous use of "shocks" in the economic literature.

[8] Both Louçã and Mata (2001, 340) and Hartley et al. (1997, 46—50) deny the legitimacy of the filiation with exogenous growth theory claimed by real business cycle theorists. The former highlight Solow's opposition to the new classical macroeconomics (and particularly to the representative agent and the principle of intertemporal substitution). Relying on Cogley and Nason's 1995 results, the latter argue that in absence of significant internal propagation mechanisms (that otherwise

In the early real business cycle literature, any reference to Solow was absent. The random shock distribution was not originally defined to match actual data relating to technical change but was artificially specified so that the variance of output in the model be equal to the actual variability of postwar US production. This restriction was challenged by Lucas in his May 1985 Yrjö Jahnsson Lectures, published two years later, for not resulting from estimation based on actual economic data. He suggested "using Solow's method for estimating 'technical change' . . . from time series on output, inputs and factor shares" (Lucas 1987, 72). Prescott formally responded to this criticism in a paper prepared for the November 1985 session of Karl Brunner and Alan Meltzer's Carnegie-Rochester Conference on Public Policy dedicated to the comparative importance of monetary, credit, and real impulses for business cycles. The paper was also presented at the summer meeting of the NBER program in Economic Fluctuations headed by John Taylor and Gregory Mankiw, in July 1986.[9] It was then discussed twice—by Kenneth Rogoff at Carnegie-Mellon and by Lawrence Summers in Cambridge, Massachusetts—and published (along with the respective discussions) in fall 1986 both in the *Carnegie-Rochester Conference Series* and in the *Federal Reserve Bank of Minneapolis Quarterly Review*.[10] The paper was an attempt to justify (or even consecrate) the real business cycles approach based on its ability to fit the data, and establish the real business cycle model as the new "paradigm for macro analysis" (Prescott 1986a, 9).[11]

Prescott applied Solow 1957 method for measuring technological change to quarterly (instead of yearly) data for the postwar US economy (1955-84), preserving the two standard assumptions of constant returns to scale and perfect competition. The computed standard deviation of technical change was much higher than the value Kydland and Prescott had chosen to replicate empirical observations in their 1982 simulation model. After some considerations about the opportunity to compute the variance of the residual from yearly

would replicate the secular movement of output) real business cycle theorists cannot pretend to model economic growth.

[9] At the time, Kydland and Prescott were not yet affiliated with the NBER. Prescott entered the program in September 1989 (Kydland almost ten years later).

[10] Young (2014, 140—52) traces the early discussions that have surrounded Kydland and Prescott's assumptions and methodology.

[11] Hoover and Salyer 1998, among others, do not contest the ability of real business cycle models to fit the data (or more precisely some handpicked stylized facts) but attribute it to statistical artifacts (notably related to the Hodrick-Prescott filter).

data and some comparisons of the properties of the technical change with those of a random walk, Prescott eventually came to the conclusion that the standard deviation of the residual was consistent with the value he and Kydland had chosen in the 1982 paper.

An early version of this approach had been previously outlined in the dissertation written by Prescott's graduate student, Gary Hansen.[12] The corresponding chapter on "indivisible labor" was published in 1985 in the *Journal of Monetary Economics* upon Robert King's solicitation (Hansen, pers. comm., March 21, 2017). However, Hansen explained, "it was very definitely Prescott's idea to use the Solow residual to calibrate the technology shock . . . At the time I was working on my dissertation, Prescott had come around to the view that . . . serially correlated shocks emerged naturally from the sum of a sequence of iid [independent and identically distributed] random variables up to some date t (the Slutsky idea). This is how he wanted to think about Solow's technology shock."[13]

The main criticisms against Prescott's article dealt with the notion of technological shock, considered the main source of economic fluctuations, and transcended the freshwater/saltwater divide.[14] Lawrence Summers, one of the staunchest opponents, pointed out the unrealistic nature of a "price-free economic analysis," which aimed to explain economic fluctuations without ever resorting to relative price changes or integrating coordination failures. "Extremely bad theories can predict remarkably well. Ptolemaic astronomy guided ships and scheduled harvests for two centuries," he added with a bit of Attic salt (Summers 1986, 25). On the freshwater side, Lucas (1987, 33) considered the model "a mistake" in that it "focuses exclusively on real (as opposed to monetary) neoclassical considerations" while fully endorsing Kydland and Prescott's calibration

[12] Hansen's dissertation was almost completed in 1984 and delivered in December 1986.

[13] The "Slutsky idea" Hansen is referring to in his correspondence (as well as Prescott 1986, 10) is a stochastic method proposed by Slutsky in 1927 (translated from Russian in Slutsky 1937).

[14] It should be noted that when Hall built his classification in 1976, new Keynesian macroeconomics had not yet emerged. New classical macroeconomics coincided closely with the freshwater side of the saline spectrum. While these authors all considered that "monetary policy has no real effect" (Hall 1976, 1), only the advocates of a pure real business cycle theory went so far as to consider that money has no effect (which is not the case for economists like Lucas, at least at the time, or Sargent). Those still referred to as Keynesians could be advantageously reclassified on the saltwater side. So could monetarists (briefly cited by Hall in his article but never studied in depth). Between these two idealtypical categories, the so-called new Keynesian authors have been distributed to varying degrees along the saline spectrum. Some original real business cycle followers salinized themselves (like Eichembaum or Christiano); others went back and forth, like Rebelo, whose "brain is New Keynesian but . . . heart is RBC" (Eichembaum, pers. comm., 2018).

methodology.[15] Prescott's Minnesota colleague Thomas Sargent—corresponding to "distilled water" according to Hall's taxonomy (1976, n1, 1)—challenged the empirical strategy chosen in the paper (see also Rogoff 1986), that is, Prescott's breaking away from classical econometric testing to employ calibration.[16] As Prescott (1986a, 10) himself acknowledged:

> The models constructed within this theoretical framework are necessarily highly abstract. Consequently, they are necessarily false, and statistical hypothesis testing will reject them. This does not imply, however, that nothing can be learned from such quantitative theoretical exercises. [Prescott 1986a, 10; see also Lucas 1987, 45-47]

The preliminary parameterization phase recommended using micro or macro estimates from other models to estimate the parameter values of the model. Kydland and Prescott drew from the economic literature possible values for intertemporal elasticity of substitution or factor shares of output, and used the Solow residual to estimate the autocorrelation and standard deviation of their technological shock. As with the validation phase of the model, this parameterization procedure raised numerous technical and epistemological objections (Hoover 1995; Hartley, Hoover and Salyer 1997). Finally, the whole calibration exercise did not stifle debate but only shifted it to the choice of parameters.[17] Neither did the computation of the Solow residual Prescott had chosen to validate the existence of technological shocks escape close scrutiny. It quickly became, in King and Rebelo's (1999, 962) words, the "Achilles heel of the [real business cycle] literature," despite successive freshwater attempts to defend its supply-side nature (e.g., Prescott 1986b; Plosser 1989; or Kydland and Prescott 1990).

[15] Lucas (pers. comm., March 14, 2017) eventually changed his mind about the importance of monetary factors in business cycles. In the presidential address he delivered at the meeting of the American Economic Association in 2003, he considered that monetary factors were a minor source of instability for most of the postwar period, and very likely until 2008.

[16] The battles around which empirical methods to adopt in macroeconometrics pervaded the department of economics at Minnesota throughout the 1980s, as Cherrier 2018 explains.

[17] Summers (1986), for instance, argued that the value of elasticity of intertemporal substitution necessary to generate realistic labor movements was highly unrealistic. Since Kydland and Prescott 1982, all real business cycle theorists have attempted to fix this issue and generate large output fluctuations with small changes in total factor productivity and labor (see Plosser 1989, 68—69).

4. Hall Knocks over the Salt Cellar

If Prescott had hoped to win over freshwater economists to his method and conclusion through using the Solow residual, he was bound to fail from day one. For at the same time as he was working on his 1986 paper, Hall was independently trying to explain why labor productivity, one of the key determinants of the Solow residual, tended to rise when the economy expanded.[18] He investigated specification errors and problems in measuring labor input or effort, which he eventually found to account for a small fraction of procyclicality. Neither overtime labor (an explanation favored by freshwater economists) nor wage smoothing, adjustment costs, or price rigidity (explanations championed by saltwater researchers) passed Hall's tests, also rejected was Summers's 1986 assumption of labor hoarding.[19] A more convincing explanation for procyclical productivity, he thought, was the market power of firms, a feature in which he had developed a longstanding interest. And to test his hypothesis, he needed to work with the Solow residual.

After a 1967 MIT dissertation supervised by Robert Solow, aimed at comparing wealth allocation and intertemporal substitution effects in alternative growth models, Hall began considering labor markets. His interest was motivated by the economic situation–slowing productivity and rising inflation–and the associated discussion of unemployment and the Phillips curve held by the Brookings Institution panel he joined at the turn of the 1970s. The series of models he published in the *Brookings Papers for Economic Activity* incorporated both the labor market microfoundations outlined in Phelps *et al.* 1970–especially the chapters by Charles Holt, Edmund Phelps, Dale Mortensen, Lucas and Leonard Rapping's chapters–and the rational expectations hypothesis. In the book review of the Phelps volume he wrote for the *Journal of Economic Literature* in 1972, Hall acknowledged Phelps and Winter's "early contribution to the recent upsurge of interest in the mathematical theory of monopolistic competition." The discussion of market imperfections

[18] The research, initially circulated as a 1986 NBER working paper, was published in the *Journal of Political Economy* in 1988.

[19] Summers's 1986 argument relied on Fay and Medoff 1985. With particular reference to Hall 1980, they provided an early study of the divergence between hours measured and labor effort, that is regularly quoted in the new Keynesian literature on efficiency wage. Measurement errors were commonly acknowledged by economists from various schools of thought. They reduced the part of procyclicality to be explained but did not pointed toward a specific theory.

became increasingly important in his work on labor market in the 1970s and first half of the 1980s.[20]

In a couple of 1986 papers, Hall tried to demonstrate econometrically that the U.S. economy could not be considered perfectly competitive. He regressed the Solow residual on the growth rate, whose coefficient β could be used as a measure to interpret the price-to-marginal cost ratio (equal to $1/(1-\beta)$). Perfect competition meant that price would equal marginal cost and the coefficient β would be 0, which Hall (1986a) rejected both for the economy as a whole and for a large number of sectors.[21] But then, Hall faced a paradox: market power should result in substantial profits for some firms, yet excess profits were nowhere to be found in the US economy. This apparent paradox, he believed (1986b), resulted from the behavior of US industries, many of which did not minimize their costs but rather operated with chronic excess capacity (thus limiting their profit).

As at the turn of the 1960s, the bottom line was that imperfect competition and market power affected the Solow residual. The residual thus provided only a biased measured of total factor productivity, in that it also captured market power which could result from increasing returns to scale.[22] Hall was the first to quantify such bias formally and provide an appropriate measure of true productivity.[23] He further pointed out that the Solow residual, to effectively measure technical progress, must be uncorrelated with any variable itself uncorrelated with the growth rate of total factor productivity (the so-called

[20] Hall discussed the issues of "externalities operating in the labor market," "natural monopoly" of the placement industry that operates at "increasing returns to scale," "market power," among others.

[21] The residual was expressed in terms of value added (rather than gross output), an approach that provoked severe criticisms in the subsequent literature. See McCombie 2000 for a technical discussion.

[22] First rejected as an unconvincing candidate (Hall 1986a, 1986c), Hall rehabilitated the hypothesis of increasing returns to scale in a 1988 mimeo. Increasing returns to scale is a plausible alternative to chronic excess capacity in order to explain the absence of large pure profits under monopolistic competition.

[23] By contrast with Solow's 1957 estimation of the elasticity of output with respect to labor, β in equation (1), with the labor share in total revenue, Hall 1986b used the labor share in total cost. Under the assumption of perfect competition, the two shares should be identical (which proved to be questionable in the empirical data). In the case of market power, the revenue share understates the elasticity of output then overestimates (resp. underestimates) true productivity in booms (resp. busts). As shown by Hall, using the cost share can correct this procyclical behavior unless there is chronic excess capacity: while "a firm with procyclical Solow productivity has market power," he concluded, "one where the productivity based on the cost share is also procyclical has excess capacity" (1986b, 16).

"invariance property"). But in the postwar US data, the residual appeared to be correlated to variables such as military spending, world oil price or the political party of the US president, which again pointed to market power, increasing returns and external technical complementarities (Hall 1989).[24] His dismissal of constant returns to scale gradually morphed into a more direct criticism of the real business cycle theory. Hall (1986c) made it clear in an article published in the *Brookings Papers* that competing explanations for procyclicality of the Solow residual were underpinned by alternative models of business cycles. Either technological shocks were correlated with the business cycles (the real business cycle hypothesis), or market structure was not competitive.[25] Hall's work then promoted heterodox theories (consistent with his chronic excess capacity hypothesis) for which he intended to provide an empirical basis:[26]

> Recent authors have built theoretical models in which market power implies that the equilibrium of the economy occurs at a point with unused labor. Some of these models have multiple equilibria. However, there is still a large gap between the theoretical models and empirical work. [Hall 1986c, 287]

While Hall was the one who worked out the freshwater/saltwater opposition, the above summary of his work shows he fitted neither category. Throughout the 1970s, he opposed Keynesians and real business cycle economists alike, playing them one against the other. During a 1979 Carnegie-Rochester conference, he championed the neoclassical model of intertemporal substitution introduced by Lucas and Rapping ten years before. While he usually endorsed the rational expectations hypothesis and the Lucas critique, he did not retain Lucas's imperfect information framework, which he considered to be an "artifact." Nor did he accept the alternative offered by new Keynesian economists, that is the "inability to write complete contracts." Rejecting the "dominant line of thought" and retaining only the basic mechanics applied to labor arbitrations, he concluded that "the intertemporal substitution hypothesis is neither Keynesian nor classical" (Hall 1979, 3), and that "nothing in

[24] These instruments, expected to be the cause of important movements in output and employment, have not effect on productivity shifts.

[25] His findings were also presented at the Carnegie-Rochester conference and published in a 1987 paper. In this paper, Hall is less affirmative about the alternative to be retained (even if one implicitly understands that he does not place himself on the side of the real business cycle theory).

[26] These models Hall was referring to when he was concerned with multiple equilibria have authors like Peter Diamond, Oliver Hart, John Bryant or John Roberts, among others. For a complete survey of the "strategic complementarities" literature, see Cooper and John 1988.

[his] paper can be said to confirm the intertemporal substitution model as against classical or Keynesian alternatives" (Hall 1979, 7). Adopting the conclusions of the former (role of money in fluctuations, effect of current income on consumption) and the concepts of the latter (especially the permanent income hypothesis), it would seem appropriate to rank him among the authors of the new Keynesian economics. Yet Hall seemed to claim a more balanced position, acknowledged by Robert Gordon's (1989, 181) own depiction of the saline spectrum. According to the latter, the Carnegie-Rochester conference was tilted toward the freshwater side, Brookings toward the saltwater side, while the NBER program on Economic Fluctuations, chaired by Robert Hall from its creation in 1977 (until 2013), was standing right in the middle.[27]

5. The NBER Weaponizes the Solow Residual

Hall's challenge to Prescott mixed questions of measurement (what does the Solow residual really capture?), theoretical argument (what explains total factor productivity?) and econometrics (should hypothesis testing be preferred to calibration?). As Hall (1986a, 37) himself confessed, his work did not bring anything new to the question of the Solow residual; the objections he raised were already raised in the 1950s and 1960s. Nevertheless, the originality of his econometric approach and the change of perspective he adopted with respect to Kydland and Prescott sparked a renewed interest in the Solow residual within the NBER program on Economic Fluctuations and finished to shift the residual from a logic of secular growth to the problem of short-term economic fluctuations. Glenn Hubbard, who joined the program in 1983, coauthored with Ian Domowitz and Bruce Petersen in December 1986 a working paper based on Hall's results and in which he explicitly supported his conclusions against the real business cycle literature (even if the estimated markups were somewhat lower). Although a Northwestern economist, he highly relied on the new Keynesian imperfect competition literature (that of Oliver Hart, Gregory Mankiw, George Akerlof, Janett Yellen, Olivier Blanchard, Julio Rotemberg, and so forth).

[27] Between 1980 and 2001, the year before the *Carnegie-Rochester Conference Series on Public Policy* was merged with the *Journal of Monetary Economics*, Hall had published five times both in the *Carnegie-Rochester Conference Series* and the *Brookings Papers*. His attempts to reach a balanced view were mirrored in his nomination strategy, though some economists including Lucas found it too restrictive (see Cherrier and Saïdi 2018, 458).

Some economists also adopted Hall's econometric methods, but only to back Prescott's analysis. In two 1987 NBER working papers, for instance, Matthew Shapiro, then a young assistant professor of economics at Yale University, used annual data to confirm Hall's findings that the two measures of productivity (revenue-based and cost-based) differed from each other but argued it was a consequence of specification and measurement errors rather than a sign of monopolistic competition (Shapiro 1987a). With Prescott, he maintained that the residual as calculated by Solow was a good indicator of true productivity and confined Keynesian explanations of procyclical productivity (adjustment cost and labor hoarding) to higher frequency data, in which demand shocks were assumed to operate. Shapiro (1987b) acknowledged that market power was found in numerous industries, but he did not believed this conclusion challenged Kydland and Prescott's conclusion that supply shocks were more important than demand shocks in explaining fluctuations. In doing so, he contributed to establish the interpretation of the Solow residual as the fault line between proponents and opponents of real business cycle theory.[28]

Another consequence of Hall's research line is that it contributed to shift the Keynesian agenda from opposing supply-side business cycle rationales to also challenging demand-side explanations. Rotemberg and Summers (1988), for instance, argued that the Solow residual was more procyclical in industries in which labor hoarding is stronger. Associated to nominal rigidities, labor hoarding was considered by the two Cambridge (Massachusetts) economists as a way to explain procyclical labor productivity that offered an alternative to Hall's increasing returns and market power assumptions. They received support from another NBER Economic Fluctuations team member. Ben Bernanke, together with Martin Parkinson, set to replicate estimates *à la* Hall by directly estimating the coefficients of the log-linearized production function (Bernanke and Parkinson 1990). They strongly rejected the technological shocks hypothesis, yet were unable to conclude whether non-competitive market structure with increasing returns and constant returns of scale was a better explanation than labor hoarding of procyclicality of labor productivity.[29]

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[28] He rallied econometrician Mark Watson to his cause the following year to conclude that real and monetary aggregate demand shocks can affect output only in the short run.

[29] They favored the labor hoarding hypothesis, contrasting with econometric works conducted separately at the same time by Hall's former PhD student Valerie Ramey and Robert Chirinko, supporting the presence of non-convexities (whether due to increasing returns or market power).

It was through his collaboration with Michael Woodford that Rotemberg achieved a kind of synthesis whereby market power and nominal rigidities were combined to explain business cycles. Increasing returns were obtained through the introduction of overhead labor based fixed costs instead of declining marginal costs (Rotemberg and Woodford 1989, 1990). The market structure—borrowed from previous work with Garth Saloner—was similar to monopolistic competition in that firms implicitly collude, resulting in a sluggish adjustment of prices, a moderate level of profits (consistent with empirical data) and a countercyclical markup.[30] But the main novelty of Rotemberg and Woodford's 1989 approach did not lie in the mechanism designed to explain the business cycle, but in its empirical methodology. For they calibrated both an oligopolistic and a competitive version of their model, so that they could easily compare the predictive power and properties of the competing versions by how closely they were able to mimic postwar US data impulse responses (related to military expenditures). Rotemberg and Woodford accordingly concluded that their "implicit collusion model" performed better than the standard real business cycle model with regard to replicating employment, productivity and real wage movements. The following year, they tested the implicit collusion hypothesis against two alternative oligopolistic competition models. Following Hall's computational method, they reprocessed the Solow residual to account for imperfect competition and increasing returns, and provided evidence that the data were most consistent with a countercyclical markup. Rotemberg and Woodford's move represented an important step toward the development of dynamic stochastic general equilibrium (DSGE) models in the next decades.[31] They established a new pattern whereby so-called saltwater economists would accept the modeling and empirical standards championed by their opponents, in the hope of demonstrating that models intended at replicating the business cycles needed those kind of frictions they were pushing for.

At the same time, a stream of freshwater contributions explored which hypotheses needed to qualify real business cycles models, initiating a bridging with the saltwater school. Prescott's student Andreas Hornstein (1993), for instance, came to endorse the ideas that

[30] Markup is countercyclical for firms have less incentive to deviate from the collusive equilibrium in lean times. Thus, costs increase and decrease faster than prices. Nominal rigidities are needed to generate procyclical real wages that move in the opposite direction to the markup.

[31] Saltwater economist Jordi Galí, for instance, endorsed writing imperfect competition models which could be calibrated in the 1990s and investigated structural VAR modeling.

RBC economists' computation of the Solow residual overestimated the fluctuation of productivity and that monopolistic competition and increasing returns to scale offered much more powerful mechanisms for the propagation of technological shocks, while acknowledging that the latter could still account for a "substantial fraction of observed output volatility." Robert King, one of the pioneers of real business cycle modelling, in a joint paper with Marianne Baxter (1991), introduced increasing returns and preference shocks in the real business cycle literature and calibrated an otherwise standard model using military spending as measure of public expenditure (as suggested by Hall) and the Solow residual to measure shifts of technology. They managed to replicate the weak correlation between hours and wages found in the data. Another early proponent of real business cycles, Martin Eichenbaum, began to sprinkle salt on their models. Eichenbaum 1990 challenged Kydland and Prescott's 1989 claim that technological shocks could account for 70 percent of output fluctuations, then developed fiscal and monetary explanations of business cycles with Lawrence Christiano and Charles Evans and agreed that the Solow residual also captured labor hoarding effects. As Eichenbaum (pers. comm., April 30, 2018) later reflected, "my advisor Tom Sargent once told me that the real divide between people in economics was really between people who interacted with data to learn about the world and people who used data to support their a priori views. I hope I belong to the first group. There is no doubt that the data pushed me in the direction of 'salt water.'"[32]

Finally, another line of research centered around the interpretation and estimation of the Solow residual, initiated by Benhabib and Farmer 1994, endorsed calibration but totally dispensed with the idea that the business cycle was, to some extent, driven by technological shocks. It built on a 1989 NBER working paper, authored by Caballero and Lyons who concluded that the increasing returns they estimated for aggregate U.S. industries disappeared at a more disaggregate level and may result from "external effects." Benhabib and Farmer demonstrated that under increasing returns to scale consistent with the upper bound of Caballero and Lyons' estimates, they could generate purely expectation-driven business cycles, as the equilibrium would be indeterminate. Roger Farmer (pers. comm.,

[32] While Christiano's trajectory along the saline spectrum was quite similar to that of Eichenbaum, Evans's position has always been critical of real business cycle theory: his 1992 article in which he demonstrates that the Solow residual is not an exogenous impulse in the Granger-Sims sense but is rather Granger-caused by money, interest rates and government spending remains a prominent charge against real business cycle theory in favor of a monetary explanation of cycles.

May 1, 2017) remembers: “I taught those ideas in my graduate class. Jang-Ting [Guo], a graduate student at UCLA at the time, suggested to me that we put the indeterminacy idea together with the calibration techniques he’d learned from Gary Hansen [appointed assistant professor at UCLA in 1992].” Farmer and Guo 1994, published in a special issue of the *Journal of Economic Theory* on indeterminacy, was then the first paper to assess the quantitative plausibility of sunspots that were proved to mimic empirical data at least as well as the standard real business cycles models and fit closely the Solow residual fluctuations. The Benhabib-Farmer-Guo model was quickly challenged by NBER economist, Susanto Basu, and his former PhD colleague at Harvard, John Fernald. Based on Hall’s methodology, they argued that Caballero and Lyons’s findings were likely to result from composition biases and the use of value-added data. Once controlling for composition effects and using gross accounting data, they concluded on a small (or even negligible) amount of output spillovers and internal returns to scale close to constant, relegating the sunspot literature to the range of “theoretical curiosita” (Basu 1992, ix).[33] Soon joined by Shapiro, who worked with Ramey on this topic in the mid-1990s, they favored capacity utilization, adjustment costs (an explanation long defended by Mark Bils within NBER) and composition biases as the major source of procyclical productivity.[34] In order to rehabilitate the literature on indeterminacy, various authors—usually Benhabib and Farmer’s PhD students or co-authors—introduced additional features in the canonical model to minimize the level of increasing returns to scale: as they finally obtained indeterminacy for (almost) constant returns, the controversies around the Solow residual eventually moved into the debate on the degree of returns to scale.[35]

Many of those developments were nurtured within the Economic Fluctuations group of the NBER, the orientation of which was largely shape by Hall’s willingness to develop models which would transcend political divisions and eschew both extremities of the saline spectrum. In the process, the residual that was initially intended to legitimize the technological shocks approach was thus reappropriated by supporters of both schools, and

[33] Burnside, Eichenbaum and Rebelo (e.g., in 1995) also concluded that increasing returns, if there were any, would be small.

[34] Capital measurement may be affected by changes in capacity utilization. This aspect was already pointed out by Solow (1957). In more recent papers, Basu and Fernald, jointly with Shapiro, attached increasing importance to technological changes.

[35] See Benhabib and Farmer 1999 for a complete review of this literature.

the debate boiled down to quantifying the share of the residual due to technological shocks from that coming from variables relating to demand. As Eichenbaum attests:

> The NBER was an electric place 30 years ago . . . Each side was very leery of the other side. Lots of good hard arguments. The beginning of the 'great convergence' to the NK [New Keynesian] model started there, at least for me. Larry [Christiano], Julio [Rotemberg] and Mike Woodford found out that (i) we really liked each other as people, and (ii) had a lot in common in terms of how we viewed the world. [Eichenbaum, pers. comm., May 2, 2018]

In these debates, the residual was one of the numerous battlegrounds: it was a constantly redesigned construct, gradually purged of its various biases, tested economically or reused as part of the calibration exercise.

6. Concluding Remarks

The plasticity of the Solow residual, which was used as a bridge between various types of models and empirical techniques, shaped the debate over the explanation of the business cycles. Rather than choosing a camp, freshwater or saltwater, each promoting stabilized workhorse models which could never have been reconciled with each other, the Solow residual allowed economists to travel on the salinity spectrum by writing a variety of models in which technological shocks mattered alongside a host of demand-driven features. It was not a matter of all or nothing, but of how much technological shocks, market structure, increasing returns to scale, labor hoarding, adjustment costs, wage stickiness and later monetary and financial mechanisms each contributed to the Solow residual. Those various hypotheses were allowed to coexist in models. The underlying visions of the business cycle, its optimality and the adequate macroeconomic policies, may have been irreconcilable, but the Solow residual allowed economists to compare, even combine them in macro models.

In a 1989 article, Mankiw distinguished between "external" and "internal consistency," attributing the former to the real business cycle theory, that was able, he believed, to fit into the original classical theory, prolonging it without distorting it. On the contrary, he recognized in the new Keynesian economics an example of external consistency, a more frequent use of *ad hoc* assumptions but a stronger capacity to fit the facts. The Solow residual, introduced by Kydland and Prescott to justify empirically the technological shock hypothesis and used as a Trojan horse by their opponents to promote alternative hypotheses within the dominant framework, has largely made the distinction between internal and external consistency porous. Ten years later, while debates around the residual have largely faded in favor of the consensus proposed by the new neoclassical synthesis (De Vroey, 325-27), McCombie 2000 eventually closed the case. He concluded that the econometric methods used by the different strands of the saline spectrum were capable of formally deriving from the aggregate production function neither the degree of returns to scale nor the presence of externalities.

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