

The Macroeconomic Effects of Federal Regulation

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Abstract

We introduce a new measure of the extent of federal regulation in the U.S. and use it to investigate the relationship between federal regulation and macroeconomic performance. We find that regulation has statistically and economically significant effects on aggregate output and the factors that produce it—total factor productivity (TFP), physical capital, and labor. Regulation has caused substantial reductions in the growth rates of both output and TFP and has had effects on the trends in capital and labor that vary over time in both sign and magnitude. Regulation also affects deviations about the trends in output and its factors of production, and the effects differ across dependent variables. Regulation changes the way output is produced by changing the mix of inputs. Changes in regulation and marginal tax rates also offer a straightforward explanation for the productivity slowdown of the 1970s.

Keywords: Regulation; macroeconomic performance; economic growth; productivity slowdown.

JEL classification: E20; L50; O40

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1. Introduction

Macroeconomists typically divide government economic activity into four broad classes: spending, taxation, deficits, and monetary policy. There is, however, a fifth class of activity that may well have important effects on economic activity but that nevertheless has received little attention in the macroeconomic literature: regulation. Although microeconomists have joined politicians and pressure groups in debating the effects and desirability of regulation, macroeconomists traditionally have devoted no attention to regulation. Recent years have seen a change, with several empirical studies suggesting that regulation indeed does have significant macroeconomic effects. Goff (1996) apparently was the pioneer, using factor analysis to construct a measure of total regulation in the United States and finding a type of Granger-causality effect of regulation on the path of output. Subsequently, development of several excellent sets of regulation data in cross-sections and panels of countries has led to many new studies of regulation's economic impact; see Nicoletti, Bassanini, Ernst, Jean, Santiago, and Swaim (2001), Bassanini and Ernst (2002), Djankov, LaPorta, Lopez-de-Silanes, and Shleifer (2002), Nicoletti and Scarpetta (2003), Djankov, McLiesh, and Ramalho (2005), and Loayza, Oviedo, and Serven (2004, 2005) for cross-section studies and Alesina, Ardagna, Nicoletti, and Schiantarelli (2003) and Kaufman, Kraay, and Mastruzzi (2003) for panel studies. These studies all conclude that regulation generally has deleterious effects on economic activity.

Existing measures of regulation have two important limitations, however, that restrict their usefulness in quantifying regulation's effects on the macroeconomy: (1) restriction to a small subset of regulations and (2) a short time dimension. For example, the OECD data set, used in several of the studies cited above, considers only product market and employment protection regulation; see Nicoletti, Scarpetta, and Boylaud (2000). The cross-section data, of course, have no time dimension at all, and the panel data sets of Nicoletti et al. (2001) and Kaufman et al. cover only 20 years. Restricting attention to a subset of regulations is problematic because the included types of regulations are often highly correlated both contemporaneously and intertemporally with those that have been ignored (as we document below), leading to an omitted variables bias in interpreting regression coefficients. A short time dimension makes analysis of dynamics difficult or impossible.

In the present paper, we construct a new measure of federal regulation in the US that overcomes these limitations, and we use our measure to analyze the macrodynamic effects of regulation. Our measure is the number of pages in the *Code of Federal Regulations* (hereafter, CFR). Although other researchers have proposed related

measures, ours is more precise and covers a much longer time span.¹ The CFR contains literally every federal regulation in existence during a given year, and it has a time span of more than 50 years. The main advantages of our measure compared to those mentioned above are its comprehensiveness and its time span; its relative limitations are that it contains no direct measures of the vigor of enforcement or the quality of regulations and it covers only a single country. It is complementary to the existing measures, covering different dimensions of the body of regulation and useful for addressing different types of questions. Our measure is designed for time series analysis and thus is particularly well-suited to examining the impact of regulation on macroeconomic dynamics.

We use our series in an equation derived from endogenous growth theory to examine regulation's effect on the time paths of output and total factor productivity (TFP) and secondarily on the paths of labor and capital services. Regulation grows most of the time, but there is great variation in the growth rate. That variation allows us to perform tests of the relation between regulation and the other variables. We estimate reduced-form models in which regulation affects the trends as well as the levels of the dependent variables. Regulation added over the last fifty years has reduced the trend in aggregate output and through that effect also has reduced output itself. The reduction in output's trend varies over time with the amount of regulation, but on average it has been about eight-tenths of a percentage point over our sample period. As usual with the compound effect of growth rates, the accumulated effect of a moderate change in the growth rate leads to large effects on the level over time. In particular, our estimates indicate that output now is about 62 percent lower than it would have been had regulation remained at its 1949 level. Regulation also affects the dynamic adjustment paths of all variables, altering both the trend and level of each variable and usually having both contemporaneous and lagged effects. The effect of regulation on TFP is especially noteworthy. Increases in regulation, together with changes in marginal tax rates, explain much or all of the TFP slowdown of the 1970s. Regulation's effects differ for output, TFP, capital, and labor, implying that regulation alters the allocation of resources. Where our findings are comparable with those of previous cross-section and panel studies, they generally are consistent with them.

¹Friedman and Friedman (1979), Becker and Mulligan (1999), and Mulligan and Shleifer (2003).

2. The Code of Federal Regulations and Measures of Federal Regulation

We begin with a brief description of the *Code of Federal Regulations*, the measure of regulation we extract from it, and a comparison of our measure with predecessors. A more complete discussion appears in the Appendix.

2.1. Brief History of the CFR

The CFR contains all regulations issued by the federal government. It was first published in 1938 and was divided into 50 “titles,” each pertaining to a major division of regulation, such as agriculture, banking, environment, labor, and shipping; the structure of 50 titles continues to this day. The next complete edition of the CFR was published in 1949. Annual supplements were published between 1938 and 1949, listing changes in regulations. Because of the way the annual supplements were done, it is difficult to use them to update the 1938 edition of the CFR to obtain annual page counts. After 1949, pocket supplements replaced the annual supplements, and updated versions of entire titles were published increasingly often. The pocket supplements were done differently than the annual supplements; together with the intermittent revised titles, they make it possible to construct annual page counts for the CFR between 1949 and 1969.² Starting in 1969, the complete CFR has been published annually.

2.2. Overview of the CFR Page Count Series

Figures 1 and 2 show the time paths for the level and growth rate of the total page count of the CFR from 1949 to 2002, and Table 1 presents some basic statistics of the series. Over the sample period, the CFR page count increased by more than six-fold, from 19335 pages in 1949 to 123772 in 2002. Regulation grows almost all the time, but its growth rate varies a great deal. The growth rate has a mean of 0.035 and a standard deviation of 0.04. Periods of negative growth are infrequent, and, when they do occur, the absolute value of the growth rate is small. By far, the fastest percentage growth occurred in the early 1950s. High growth also occurred in the 1970s, even though there was extensive deregulation in transportation, telecommunications, and energy. Deregulation in that period was more than offset by increased regulation in other areas, notably pertaining to the environment and occupational safety, as Hopkins (1991) has noted. The Reagan administration of the 1980s promoted deregulation as a national priority, and growth in the number of CFR pages slowed in the early and late 1980s. Nevertheless, total pages decreased in only one year, 1985. The 1990s witnessed the largest reduction in pages of regulation in the

²See the Appendix for details on the method of construction.

history of the CFR, with three consecutive years of decline. This reduction coincides with the Clinton administration's "reinventing government" initiative that sought reduced regulation in general and a reduction in the number of pages in the CFR in particular. (Interestingly, the greatest percentage reduction in the CFR did not occur during either the Reagan or Clinton administrations but rather in the first year of the Kennedy administration, 1961.) There thus are several major segments in regulation's time path, with corresponding breaks in trend (dates are approximate): (1) 1949 to 1960 (fast growth), (2) 1960 to 1972 (slow growth), (3) 1972 to 1981 (fast growth), (4) 1981 to 1985 (slightly negative to slow growth), (5) 1985 to 1993 (fast growth), and (6) 1993 to 2002 (slightly positive growth). As we will see, these segments correspond to behavior of the aggregate variables of interest.

2.3. Comparison with Other Measures of Regulation

There are several differences between our measure of regulation and earlier measures.³ We first describe some of the differences and then discuss their implications.

2.3.A. Description. Our measure covers one country over 54 years; earlier measures cover many countries over much shorter periods of time. Some of the earlier measures are purely cross-sectional, applying to a single year; others cover more years and so are panel data. The longest time span of the panel sets is 20 years.

Our measure is more comprehensive than any of its predecessors. Federal law requires that all federal regulations be published in the CFR, so our measure includes literally every regulation issued by the federal government. No other measure of regulation comes close to that extent of coverage. For example, the most widely used of the earlier data sets is the OECD cross-section measure described by Nicoletti et al. (2000) and extended in part to a 20-year panel by Nicoletti et al. (2001). The cross-section data are restricted to product market and employment protection regulation; other types, such as environmental or occupational health and safety regulation, are ignored. The panel extension is restricted further to a small subset of seven non-manufacturing industries: gas, electricity, post, telecommunications, passenger air transport, railways and road freight. Types of regulations considered also are limited, with data availability varying by industry: barriers to entry (available for all industries), public ownership (all industries except road freight), vertical integration (gas, electricity and railways), market

³See Nicoletti et al. (2000), Nicoletti et al. (2001), Djankov et al. (2002), Kaufman, Kraay, and Mastruzzi (2003), Nicoletti and Scarpetta (2003), Loayza et al. (2004), and Djankov et al. (2005) for detailed descriptions of these alternative measures.

structure (gas, telecommunications and railways), and price controls (only road freight).

All measures of regulation including ours are aggregate indices. Our index is more highly aggregated than any of the others simply because it covers the full array of regulations, but all are aggregates. None simply reports a quantitative measure of the magnitude or effect of a single regulation. The OECD measure, for example, collects answers to about 1300 questions and combines them into an index through a multistep aggregation procedure. The methods of aggregation differ substantially across indices. Our method is to weight each regulation by its number of pages in the CFR, which captures at least partially the complexity of the regulation, as we discuss below. Many other indices are constructed as simple averages of basic data, with no attempt to weight by the importance or complexity of the regulations included. The OECD uses a multistep procedure, in which the OECD staff creates a collection of categorical sub-aggregates mostly as simple averages of basic data and then uses factor analysis to aggregate those into its final indices.

All measures except ours are based at least in part on survey data, typically obtained from questionnaires sent to government officials (OECD), market participants (Kaufman et al.), and/or lawyers (Djankov, 2002). Our measure is based solely on the page count of the CFR.

2.3.B. Evaluation. Our measure is a pure time series covering a long time span. The earlier measures of regulation have short to non-existent time spans, the longest being 20 years and the shortest 1 year. Such data cannot be used to study regulation's effects on macroeconomic dynamic adjustment paths, which requires following the evolution of variables through time. There is more hope of studying regulation's effects on average growth rates by using the cross-sectional dimension of the data to overcome the inadequate time dimension, which is precisely what several of the previous studies do. However, growth is an intertemporal phenomenon, so it would be useful to have time series estimates of regulation's effects on it, especially in light of Ventura's (1997) demonstration that the interpretation of cross-country growth regressions is confounded by the effects of foreign trade. Our measure, with its comparatively long time dimension, allows us to study both the long-run growth and short-run dynamic adjustment effects of regulation. The earlier studies, with their strong cross-section element but weak intertemporal element, are better suited for cross-sectional issues.

Our measure also is more comprehensive than the earlier measures, none of which encompasses the total set of regulations in any country. Incomplete coverage leads to two problems: (1) omitted variables bias, and, in any

time series study, (2) divergence between the time series behavior of subsets of regulation on the one hand and of total regulation on the other.

Table 1 shows that the page counts of the various titles of the CFR are highly correlated with one another, whether measured in levels or growth rates. The mean correlation among levels is 0.60, with an even higher median of 0.77. The maximum correlation in levels is 0.99, and the minimum correlation is -0.76. The correlations in growth rates are much lower, of course, with a mean of only 0.16 (median of 0.15), but there still are quite a few correlations of substantial magnitude, with the maximum and minimum being 0.74 and -0.63, respectively.⁴ Such high correlations show that including just one type of regulation in a statistical analysis is likely to be misleading because of multicollinearity and consequent omitted variables problems. The problem is even more severe when addressing issues of macroeconomic dynamics. The correlations in Table 1 are all contemporaneous; for analyzing time series behavior, we also want to know the dynamic relations among various types of regulations. Granger-causality tests show the intertemporal dependence of one series on another after accounting for the first series's dependence on its own lagged values. Table 2 summarizes Granger-causality test results for two titles of the CFR related to the kinds of regulations studied in previous analyses—regulation of entry and regulation of labor markets. Title 16 of the CFR pertains to Commercial Practices, and Title 29 pertains to Labor. Table 2 shows that the page counts of those titles both Granger-cause and are Granger-caused by the page counts of other titles, some apparently quite unrelated in content to the subjects of titles 16 and 29. Similar results hold for most of the other titles of the CFR. These Granger-causality relations among CFR titles show that there are temporal orderings in the statistical relations among the types of regulation and provide strong evidence that a time series analysis restricted to a subset of regulations is likely to suffer from serious omitted variables bias.

The foregoing remarks have greatest force when applied to attempts to study the economic effects of a particular type of regulation. If one is interested in the impact of total regulation, the high correlations among the different types might actually be considered good news because they suggest that a subset of regulations may capture the behavior of the whole. Indeed, Nicoletti et al. (2001), who have perhaps the most restricted measure of all, interpret their indicators as “a proxy for the overall regulatory policies followed by OECD countries over the sample

⁴Similarly, Loyaza et al. (2005) found very high correlations among their 7 indices of regulation.

period (p. 43).”

Unfortunately, examination of the data shows this hope to be ill-founded. Nicoletti et al.'s (2001) measure spans 1978-98 and shows a 66% decline over that period. Subsets of CFR titles corresponding to Nicoletti et al.'s measure behave similarly. For example, titles 23 (Highways), 46 (Shipping), and 49 (Transportation) of the CFR encompass regulation of air transport, railways, and road freight, one Nicoletti et al.'s regulation groups. The page count of titles 23, 46, and 49 drops from a total of 8400 in 1978 to 8261 in 1998, which is qualitatively the same behavior as Nicoletti et al.'s measure. Nevertheless, the page count of the total CFR displays the opposite behavior, rising 47% over 1978-98. The inescapable implication is that subsets of regulation are not reliable proxies for total regulation.

Our measure of regulation is the only measure constructed by a completely objective method. Our measure consists of the page counts in the CFR, a number requiring no judgement to obtain. Subjectivity enters all other measures in two ways. First, as remarked above, all other measures are based at least in part on survey data. As Nicoletti et al. (2000) note, the people completing the surveys have some latitude in interpreting the survey questions and may respond idiosyncratically. Second, the survey must be designed and the responses must be combined, processes that involve the judgement of the investigator. For example, the OECD index begins with responses to about 1300 survey questions. The responses usually are Yes or No. Groups of these responses are combined by averaging to create categorical variables with values from 0 to 6. The procedure used for measuring the scope of public enterprise illustrates the issues. Respondents are asked if there are “national, state or provincial government controls in at least one of” 24 industries chosen by the OECD. Some of the industries chosen are 2-digit ISIC (e.g., wholesale trade, financial institutions), some are 3-digit (e.g., tobacco manufactures), and some are 4-digit (e.g., electricity, motion picture distribution and projection). Despite the differences in size and importance, all industries have been assigned the same weight of 1 in the construction of the categorical variables. See Table A2.1.1 in Nicoletti et al. (2000), p.60, for details.⁵ Other measures of regulation are generally more subjective than the OECD's. An oddity that results from subjectively deciding what is and is not regulation is that two of the published

⁵Nicoletti and Pryor (2001) argue that the OECD measure is objective, but it clearly is not. Indeed, in describing its construction, Nicoletti et al. (2000) say that 90% of the data underlying the OECD measure is “survey data” (p.11) and that both questionnaire responses and the procedure for scoring them involved “subjective judgement” (p.16).

measures of regulation contain elements that have nothing to do with regulation. The OECD measure includes data on publicly-owned enterprises, a form of government intervention but not regulation. Loayza et al.'s (2005) measure includes data on spending and taxation and on the fraction of the workforce that is unionized, neither of which pertains to regulation. These measures thus confound regulation with other government and even non-government activities.

Some indices of regulation attempt to measure the burden imposed by the component regulations by including quantitative and/or qualitative data pertaining to the regulations included. Examples include the number of procedures a new firm must go through to start operation (Djankov et al., 2002) and regulatory complexity (OECD; Nicoletti et al., 2000). Our measure contains no such direct measures but nonetheless controls for regulatory burden to some extent. It seems reasonable to suppose that, on average at least, the more complex a regulation, the more pages it will require. Indeed, the OECD measures complexity by the presence or absence of a long list of regulatory requirements. The larger the number of requirements, the more the pages of regulations necessary to describe them, which is precisely what our measure captures. Indeed, our approach may give a more complete picture of regulatory burden than the OECD's measure because page counts indicate not only the presence or absence of particular provisions but also their complexity.

2.3.C. Summary. Our page count measure of the extent of regulation compares well with other measures. Although limited to a single country, it has a much longer time span than any other measure. It is unique in being totally comprehensive.⁶ It also is unique in being 100% objective both in the data underlying it and in the method of constructing the index. It offers indicators at two levels of aggregation—one final index for total regulation and many sub-indices for the different classes (“titles”) of regulation. It is easily replicated and easily updated. Finally, CFR page counts are a more precise measure of regulation than page counts of other federal publications, such as the *Federal Register* or the *U. S. Code*, suggested by others.⁷ The *Federal Register* contains proposed regulations and other irrelevant material; the *U. S. Code* contains all federal laws, not just regulations.

⁶At least for federal regulations. Our measure does not include state and local regulations.

⁷Friedman and Friedman (1979) use the number of pages in the *Federal Register* to measure the growth of regulation. Becker and Mulligan (1999) use pages in the *U.S. Code* as a measure of growth in the size of government. Mulligan and Shleifer (2003) use kilobytes of unannotated state law, where 1kb approximately equals one printed page, to study the causes of regulation.

3. Theory

We divide theories of regulation into two categories: micro and macro, which we discuss separately.

3.1. Microeconomic Theory of Regulation

The microeconomic theory of regulation also divides into two types: those about the effects of regulation and those about its origins. A full discussion of either is far beyond the scope of the present paper, and in any case theory's implications for macroeconomic aggregates always are ambiguous. We therefore present only the briefest of summaries.

Even at the micro level, regulation's effects often are not straightforward. For example, regulating the rate of return earned by public utilities seemingly should make the utility less profitable and so reduce its capital stock. However, in a well-known article, Averch and Johnson (1962) show that capital may rise. Even when regulation's effect on a firm is clear, the effect on the market often is not. Smokestack emission regulations may require a firm to invest in new capital, implying that capital should rise in response to the regulation, but some firms may close in the face of the new regulatory costs, reducing capital. The net effect on aggregate capital is ambiguous. Effects on production costs and thus output are even more difficult to predict. Effluent regulations increase the cost of business for the polluter and reduce his output but have the opposite effects on producers downstream. Again, the aggregate output effect is ambiguous. Moreover, types of regulation interact with each other and with the market structure of the regulated industry, typically leading to ambiguous effects. See Alesina et al. (2003) for a more extended discussion. Regulatory effects on labor also are complex; see Blanchard and Giavazzi (2003) for one treatment.

The origins of regulation are studied in a branch of the public choice literature. Djankov et al. (2002) present an excellent discussion of the literature, which we briefly summarize here. Pigou (1938) argues that regulation arises from government's attempt to improve social welfare by correcting market failures. Stigler (1971) proposes a much less benign theory of regulatory capture, in which the regulated firms gain control of the regulatory agency and use it to their advantage. McChesney (1987) offers the related idea that regulations are created for the benefit of politicians and regulators. Though interesting, none of these theories seems to have any implication for the relation between regulation and macro variables. Even the completely benign view of Pigou does not predict whether regulation will increase or decrease measured output. For example, regulation may lower measured output (many environmental regulations probably do so) or raise it (e.g., trust-busting). However, it is important to note

that none of the theories of regulatory origin gives any reason to believe that regulations arise in response to *macroeconomic* conditions. In other words, existing theories of regulatory origin suggest that regulation is macroeconomically exogenous. That seems consistent with common sense. It is easy to imagine that regulations have significant effects on macroeconomic variables, but it is hard to imagine that regulations arise because of macroeconomic conditions. We return to this point in our empirical analysis.

3.2. Regulation and the Macroeconomy

We are aware of no theory that addresses the relation between regulation on the one hand and macroeconomic behavior on the other. However, Peretto (2007) uses endogenous growth theory to analyze the effects of various kinds of taxes on economic growth, and his approach can be adapted to study regulation's macroeconomic effects. We present a distilled version of his model to motivate the equation that we use for estimation. We restrict attention to a bare bones description of the important components of the model and present the solution without derivation. Readers interested in the details should consult Peretto.

Households maximize intertemporal utility

$$U(t) = \int_t^{\infty} \log u(s) e^{-(\rho-\lambda)(s-t)} ds \quad 0 < \lambda < \rho$$

subject to the usual flow budget constraint

$$\dot{A} = rA + WL - C$$

where ρ is the discount rate, λ is the population growth rate, A is assets, r is rate of return on financial assets, W is the wage rate, $L_t = L_0 e^{\lambda t}$ with $L_0=0$ is population (which also is labor supply), and C is consumption expenditure.

The household has instantaneous preferences over a continuum of differentiated goods:

$$\log u = \log \left[\int_0^N \left(\frac{X_i}{L} \right)^{\frac{\epsilon-1}{\epsilon}} di \right]^{\frac{\epsilon}{\epsilon-1}} \quad 1 < \epsilon$$

where ϵ is the elasticity of product substitution, X_i is the household's purchase of good i , and N is the mass of goods (= mass of firms) at time t .

Each firm produces one differentiated consumption good with the technology

$$X_i = Z_i^{\theta} (L_{X_i} - \phi) \quad 0 < \theta < 1, 0 < \phi$$

where X_i is output of the good, L_{X_i} is production employment, ϕ is a fixed labor cost, Z_i^{θ} is the firm's total factor

productivity, which is a function of firm-specific knowledge Z_i . The firm accumulates knowledge through R&D with technology

$$\dot{Z}_i = \mu KL_{Z_i} \quad 0 < \mu$$

where L_{Z_i} is R&D employment and K is the stock of public knowledge. Public knowledge accumulates from spillovers. When a firm generates firm-specific knowledge, it also generates general purpose knowledge that is not excludable. We suppose that public knowledge is just the average of firm-specific knowledge:

$$K = \int_0^N \frac{1}{N} Z_i di$$

The manufacturing firm maximizes present value:

$$V_i(t) = \int_t^{\infty} \pi_{X_i}(s) e^{-\int_t^s r(v) dv} ds$$

We assume that an entrepreneur can enter the industry by paying a sunk cost of $\sigma P_i X_i$, where P_i is the price of X_i . Upon entering, the entrepreneur begins a firm whose productivity equals the industry average. A free entry equilibrium then requires that $V_i = \sigma P_i X_i$.

We thus have a model in which monopolistically competitive firms do R&D to generate new knowledge. Accumulation of knowledge reduces costs, and lower costs raise profit and present value V . That in turn stimulates entry, which increases the number of varieties of goods and thus drives growth of output per person. The general equilibrium solution to the model leads to an output growth equation of the form

$$\ln y_t = a(\lambda, \rho, \sigma) + B(\epsilon, \theta, \lambda, \mu, \rho, \sigma, \phi)t + h(\Delta, \epsilon, \theta, \mu, \rho, \sigma, \phi)$$

where y is output per person, $a(\cdot)$, $B(\cdot)$, and $h(\cdot)$ are functions of the indicated parameters, Δ is the initial deviation from the balanced growth path. Exponentiating gives

$$y = A(\lambda, \rho, \sigma) H(\epsilon, \theta, \mu, \rho, \sigma, \phi) e^{B(\epsilon, \theta, \lambda, \mu, \rho, \sigma, \phi)t}$$

where $A = e^a$ and $H = e^h$. Both the intercept AH and the growth rate B are functions of underlying parameters.

Peretto uses the foregoing model to study the effects of taxes on the path of output. Taxes enter the model as new parameters that modify the underlying parameters in some way. Our interest here is regulation, and it is clear that regulation will enter the model in a similar way as taxes, that is, by modifying underlying parameters. There are

seven fundamental parameters: ϵ , θ , λ , μ , ρ , σ , and ϕ . Various regulations certainly affect at least total factor productivity in production of goods θ (e.g., Title 7: Agriculture; Title 12: Banks and Banking; Title 15: Commerce and Foreign Trade; Title 30: Mineral Resources; Title 49: Transportation), total factor productivity in production of knowledge μ (e.g., Title 9: Animals and Animal Products; Title 21: Food and Drugs; Title 37: Patents, Trademarks, and Copyrights; Title 40: Protection of the Environment), entry costs σ (e.g., Title 16: Commercial Practices), and fixed labor cost ϕ (e.g., Title 20: Employees' Benefits; Title 29: Labor; Title 38: Pensions, Bonuses, and Veterans' Benefits). It is quite possible that regulations also affect the consumption elasticity of substitution ϵ (by changing suppliers' incentives to affect willingness to pay through advertising and other means; see Sa, 2007) and the rate of time preference ρ (by affecting the probability of dying which enters the household maximization problem as a component of the rate of time preference). We thus have reason to search for effects of regulation on the long-run intercept term A , the transition dynamics intercept term H , and the growth rate B , using an extended form of the foregoing equation that includes regulation:

$$(1) \quad y = A(\lambda, \rho, \sigma, R_t)H(\epsilon, \theta, \mu, \rho, \sigma, \phi, R_t)e^{B(\epsilon, \theta, \lambda, \mu, \rho, \sigma, \phi, R_t)t}$$

where R_t is regulation.

An important aspect of (1) is its implication for the treatment of trend. The consensus among macroeconometricians is that the best model of the non-stationarity of aggregate data is a deterministic trend with breaks (Perron, 1989; Lumsdaine and Papell, 1997; Murray and Nelson, 2000).⁸ Equation (1) is a trend-break model derived from a tight theory of endogenous growth and thus dovetails nicely with current econometric practice. Equation (1) differs from the standard econometric trend-break models in explaining trend breaks as consequences of government policy changes rather than as unexplained random phenomena. Nonetheless, it implies a trend-break structure, so we will use that approach by adopting equation (1) as the basis for most of our empirical work.

4. Estimation

We begin our empirical investigation with a discussion of the variables to be examined and then turn to the econometric analysis.

⁸Fractional integration models have been shown to be observationally equivalent to trend-break models (Diebold and Inoue, 2001).

4.1. Variables To Be Examined

We want to study the effect that federal regulation has on macroeconomic activity. The obvious macroeconomic variable to examine is real aggregate output, and indeed that is the focus of our study. However, regulation presumably affects the economy in complex ways. It therefore seems worthwhile to examine how regulation affects not just output but also the determinants of output. If we suppose a Cobb-Douglas production function, then output Y_t is given by

$$Y_t = A_t K_t^\zeta N_t^{1-\zeta}$$

where A_t is total factor productivity (hereafter, TFP), K_t is capital services, and N_t is labor services. In what follows, we examine how regulation affects A , K , and N in addition to Y .

4.2. Data

Regulatory activity (R) is the total page count of the CFR, discussed above. Real output in the private business sector (Y), private capital service flows (K), and hours of labor services (N) are from the *Monthly Labor Review*. Output (Y) is real output in the private business sector, which is gross domestic product less output produced by the government, private households, and non-profit institutions. Capital (K) is service flows of equipment, structures, inventories, and land, computed as a Tornqvist aggregate of capital stocks using rental prices as weights. Labor (N) is hours worked by all persons in the private business sector, computed as a Tornqvist aggregate of hours of all persons using hourly compensation as weights. TFP is the Solow residual from a Cobb-Douglas production function assuming a capital share of thirty percent. Our regression analysis includes two explanatory variables other than regulation: total government purchases (G) and the federal marginal tax rate on personal income (T). Government purchases is the sum of government consumption and government investment and is from NIPA; the marginal tax rate is from Stephenson (1998) and includes both the federal personal income tax and the Social Security tax. Figures 3 and 4 show the time series for G and T . All data are annual observations for the U.S. beginning in 1949. The marginal tax rate data end in 1995; consequently, we end all other series there, too.

We explored the effect of performing our statistical analysis over a longer sample with marginal tax rates omitted. The results were qualitatively the same as those reported below, but the magnitude of regulation's effects was larger, reflecting the bias arising from omitting tax rates, which are correlated with regulation. Because the longer sample gives qualitatively similar results and because we are interested here in quantifying regulation's

effects, we restrict attention in what follows to the shorter sample period with tax rates included in the analysis.

4.3. Granger Causality Tests

In the spirit of Hamilton's (1983) pioneering study of oil and the macroeconomy, we begin with the completely non-structural method of Granger causality tests. The results of bivariate Granger-causality tests between regulation and each of the macroeconomic variables are reported in Table 3 for the period 1949-1995. The tests are conducted on the first-differences of the logs in order to have stationary variables. Lower case variables denote the logs of the corresponding upper case variables. The notation $\Delta r \nrightarrow \Delta x$ refers to the test that Δr does not Granger-cause Δx . Test results stabilized at about 4 lags. The tests indicate unidirectional causality from R to Y and TFP and no causality in either direction between R on the one hand and N and K on the other. These causal orderings are consistent with the theoretical implications discussed above.

Hamilton (1983) concluded on the basis of similar Granger causality tests that oil prices played a major role in causing movements in US aggregate output. If we were to stop here, we would draw a similar conclusion about regulation.⁹ Rather than stop, however, we continue with an examination of reduced-form regressions to explore in more detail the relations suggested by our Granger causality tests.¹⁰

4.4. Regression Models

As remarked above, we use a trend-break model derived from endogenous growth theory for our estimation.

4.4.A. The Trend-Break Model. We start with a simple generic model to facilitate discussion. Let X be any macro variable of interest, and let Z be an exogenous explanatory variable for X . Consider the following simple trend-break model for X , explained momentarily:

$$(2) \quad X_t = \left(e^{[\beta + \sum_{j=0}^{J_1} \gamma_j Z_{t-j} + \sum_{j=0}^{J_2} \delta_j Z_{t-j}^2]t} \right) \left(e^{\alpha \left[\prod_{j=0}^{J_3} Z_{t-j}^{\omega_j} \right]} U_t \right)$$

or, upon taking logarithms and putting the terms in the usual order,

⁹As did Goff (1996, Chapter 7). He estimated an ARMA model for each of several macro variables and then introduced a few lags of his factor-analysis measure of regulation, effectively performing a Granger-causality test. He found that regulation causes the dependent variables.

¹⁰We also performed Granger-causality tests on the individual titles of the CFR. Few individual titles Granger-cause any of the macroeconomic variables considered here, but the whole set of titles is jointly significant. With nearly 50 individual titles and only 54 observations, the tests have few degrees of freedom, leaving the results uninformative. This problem with degrees of freedom arises again later; we discuss it in more detail there.

$$(3) \quad x_t = \alpha + [\beta + \sum_{j=0}^{J_1} \gamma_j Z_{t-j} + \sum_{j=0}^{J_2} \delta_j Z_{t-j}^2]t + \sum_{j=0}^{J_3} \omega_j Z_{t-j} + u_t$$

where, as before, lower-case variables are the logs of the corresponding upper-case variables, α , β , γ_j , δ_j , and ω_j are constants, J_i are lag lengths, and U is a log-normally distributed residual.

Equation (2) takes a structural approach to modeling trend breaks, consistent with the theory behind equation (1). The first term in parentheses is the trend term, corresponding to the term $e^{\beta t}$ in (1). The trend coefficient is a function of Z . We assume that function to be quadratic. The quadratic is simple and suffices for this first attempt at exploring the causal relation between regulation and trend breaks. It has some undesirable implications for extrapolation beyond the sample period, as we discuss below, so further research on functional forms for the trend coefficient would be useful. Note that (2) nests the simpler linearly detrended model with constant trend ($\gamma_i = 0$, $\delta_j = 0$ for all i, j).¹¹

The second large term in parentheses in (2) appears to be purely an intercept term, but in fact it, too, contains a trend element whose role must be understood. Suppose Z obeys the law of motion

$$Z_t = e^{\alpha_z} e^{\beta_z t} V_t$$

where α_z and β_z are constants, with β_z being the trend in Z , and V is a log-normally distributed residual. Consider a macro variable X whose “own” trend is β and whose “intercept” term depends on current Z :

$$\begin{aligned} X_t &= e^\alpha e^{\beta t} Z_t^\omega U_t \\ &= e^\alpha e^{\beta t} (e^{\alpha_z} e^{\beta_z t} V_t)^\omega U_t \\ &= A e^{[\beta + \omega \beta_z] t} W_t \end{aligned}$$

where ω and $A \equiv e^{\alpha + \alpha_z \omega}$ are constants, and U and $W \equiv UV^\omega$ are log-normally distributed. The total trend in X is $\beta + \omega \beta_z$. It is the estimate one would obtain for the trend by regressing the log of X on a constant and time with Z omitted; it would be reported in the newspapers as the growth rate of X . Note, therefore, that it is *not* the trend in X that would result if Z were held constant; that would be β , not $\beta + \omega \beta_z$. In some of what follows, we will discuss counterfactual paths that would have emerged if exogenous variables had been held constant. Doing that requires

¹¹That is, a model of the form $x_t = \alpha + \beta t + \sum \omega_i z_{t-i} + u_t$.

distinguishing among the quantities β , $\omega\beta_Z$, and $\beta+\omega\beta_Z$.

Applying these results to (2) leads to

$$(4) \quad X_t = \left(e^{[\beta + \beta_Z \sum_{j=0}^{J_3} \omega_j + \sum_{j=0}^{J_1} \gamma_j Z_{t-j} + \sum_{j=0}^{J_2} \delta_j Z_{t-j}^2]t} \right) \left(e^{\alpha + \alpha_Z \left[\prod_{j=0}^{J_3} V_{t-j}^{\omega_j} \right]} U_t \right) \\ = e^{B(Z)_t} \left(A \left[\prod_{j=0}^{J_3} V_{t-j}^{\omega_j} \right] U_t \right).$$

The trend is $B(Z)_t = \beta + \beta_Z \sum \omega_j + \sum \gamma_j Z_{t-j} + \sum \delta_j Z_{t-j}^2$, a quadratic function of Z . The first term β is constant and captures trend elements apart from any effects of Z . The last three terms collect the various effects of Z . In what follows, we refer to the first term β as the *trend-apart effect* (because it captures the trend that would be present if all the exogenous variables were trendless), the second term $\beta_Z \sum \omega_j$ as the *trend-intercept effect* of Z , the third term as the *trend-linear effect*, and the fourth term as the *trend-quadratic effect*. The trend-intercept effect is constant. At times, it is convenient to keep it separate from the trend-apart effect, which also is constant; at other times, it is convenient to group the two together. In the second line of (4), the term in large parentheses is the detrended intercept of X , consisting of two parts. The first part is the constant A , and the second part is the compound residual, $U(\Pi V^\omega)$. Because X is trend-stationary, the compound residual is purely transient, causing fluctuations about trend. The term ΠV^ω captures the part of the residual due to regulation's deviation from its trend; we refer to this transient component as the *cycle effect*.¹²

Transition dynamics appear in two places in (2) and (4): the lagged terms in the growth rate and the lagged terms in the cycle effect. The balanced growth rate is the value of the growth rate in (4) when Z is constant. When Z changes, the growth rate deviates from the balanced growth rate and produces transition dynamics. Changes in Z induce additional transition dynamics through the cycle effect. In (1), all transition effects are collected in the term H , whereas in (2) and (4) they are divided between two terms.

4.4.B. Explanatory Variables and Exogeneity. We examine the sensitivity of our endogenous macro variables to three policy variables R , G , and T ; that is, the variable Z in (1) is the vector (R, G, T) and the parameters γ , δ , and ω are also vectors. We have not pursued the possibility of decomposing G into major parts (such as federal versus

¹²The U component of the compound residual can include any exogenous variable not subject to analysis. The trends in such variables are included in the trend-apart term β , so that U captures the transient components.

state and local, or national defense versus road building), even though different kinds of expenditure almost certainly have different effects on the economy and may interact with regulation in different ways. Similarly, we ignore government debt, which is the correct procedure under the assumption of Ricardian equivalence. Most of the empirical investigations of regulation cited earlier ignore G and T . We find that inclusion of T has quantitative though not qualitative effects on the estimates of regulation's macroeconomic impact.¹³

Before we can proceed to estimation, we must determine whether the explanatory variables are statistically exogenous. The Granger-causality tests for R , discussed above, show no causality running from any of the dependent variables to R , implying exogeneity of R . We performed additional Granger-causality tests (not reported) of the exogeneity of G and T . The tests stabilize at about 4 lags and indicate statistical exogeneity of T at the 5% level. In contrast, G frequently appears to be endogenous, with causality never running from G to the macro variables of interest but causality frequently running from them to G . In light of these results, we treat R and T as exogenous. Exploration of regressions that included G showed no important differences from regressions that omitted it, so henceforth we ignore G .¹⁴

4.4.C. Estimation Results. The equation to be estimated is

$$(5) \quad X_t = \left(e^{\left[\beta + \sum_{j=0}^{J_1^R} \gamma_j^R R_{t-j} + \sum_{j=0}^{J_2^R} \delta_j^R R_{t-j}^2 + \sum_{j=0}^{J_1^T} \gamma_j^T T_{t-j} + \sum_{j=0}^{J_2^T} \delta_j^T T_{t-j}^2 \right] t} \right) \left(e^{\alpha \left[\prod_{j=0}^{J_3^R} R_{t-j}^{\omega_j^R} \right] \left[\prod_{j=0}^{J_3^T} T_{t-j}^{\omega_j^T} \right]} U_t \right)$$

This form corresponds to equation (2), where the trends in the exogenous variables remain impounded in the variables themselves (that is, the trend-intercept terms do not appear in the trend expression inside the first set of parentheses). Estimation was performed on the log-linearized version of (5). The lag lengths J_x were chosen by imposing an initial value of 3 on all the J_x and searching, subject to two restrictions, over all possible smaller values to find that which minimized the Schwarz information criterion. The restrictions on the search procedure were (1) the constant always was retained and (2) no variable could be omitted unless all of its more-lagged values also were omitted. For example, even if the lowest SBC value was obtained with a model that excluded of Z_t but retained Z_{t-1} ,

¹³In this regard, our results differ from those in Alesina et al. (2003), whose estimates of regulatory impact are insensitive to inclusion or omission of fiscal policy variables.

¹⁴The irrelevance of G in the regressions is not surprising in light of the shape of G 's time path, shown in Figure 3. The path is essentially a trend with no substantial variation about the trend.

that model was not considered. Exclusion of Z_t would be allowed only if Z_{t-1} also was excluded. The reason for imposing this restriction was that, with annual data, it did not seem reasonable to suppose that a variable could have an effect only with a one-period lag. The residuals were serially correlated, so we used a Newey-West correction.¹⁵ Table 4 reports the estimation results for the five macro variables of interest. Part I of the table reports estimated values for all coefficients pertaining to regulation, and Part II reports all other parameter estimates. Very few lagged variables are significant, so to save space Table 4 is restricted to those lags that had significant variables in at least one equation.

Regulation has significant effects on all four dependent macro variables, entering with both trend and cycle terms. In some cases regulation enters with lags, indicating dynamic responses in the dependent variables. Also, the coefficient patterns and magnitudes differ across dependent variables, indicating compositional effects. As we have seen above, regulation can have two kinds of effects on a dependent variable's trend: a shift in the trend (the trend-intercept effect) and breaks in trend (the trend-linear and trend-quadratic effects). Our results indicate that both kinds of effects are present. The trend-intercept effect is the product of regulation's trend β_R and the sum of the ω_j^R coefficients. The latter are reported in Table 4, and the former is obtained by estimating the equation $r_t = \alpha_R + \beta_R t + v_t$. The estimated value of β_R is 0.0339. Estimating the analogous equation for the tax rate gives a trend in T of 0.0073.

4.4.C.1: Output. For output, there is only one ω_j^R coefficient, whose value is -0.447. Its product with β_R is -0.015, indicating that regulation shifts the trend in output down by one and a half percentage points. This shift, being a reduction in the intercept of the trend coefficient function $B(R_t)$, is uniform over time. In addition, regulation has time-varying effects on output's trend through the trend-linear and trend-quadratic terms of the coefficient function $B(R_t)$. The trend-linear coefficient is positive, causing output's trend to rise as regulation grows, and the trend-quadratic coefficients have a negative sum, indicating that the net trend-quadratic effect is negative and causes

¹⁵We tried two other estimation methods. Instead of using a Newey-West correction, we estimated subject to the following ARMA model for u_t :

$$u_t = \sum_{j=1}^J \phi_j u_{t-j} + e_t + \sum_{m=1}^M \theta_m e_{t-m}$$

where e is white noise. The results were essentially the same as those with the Newey-West correction. We also tried choosing lag lengths by dropping lag terms until we arrived at one that was individually significant. Again, the main conclusions were unchanged.

output's trend to fall as regulation grows. We thus have a non-linear effect of regulation on output's trend. Figure 5 shows the total effects of regulation on output's trend over time. The effect is always negative but is nonlinear. Growth in regulation raised output's trend (that is, made it less negative) until about 1980 and then reduced it. The average value of the negative effect is 0.008, or eight-tenths of a percentage point.

The large negative value of ω_0 indicates that regulation also has a substantial cycle effect on output, which obscures regulation's net effect on output. We can resolve the difficulty by using the parameter estimates and regulation data to calculate a counterfactual value of output that would have obtained had regulation stayed at its 1949 level.¹⁶ Figure 6 plots the ratio of actual Y to counterfactual Y . The ratio falls over the sample period, indicating that actual output fell relative to what it would have been had regulation not grown. The pattern is irregular. The ratio first falls until about 1960, then is constant for about 15 years, then falls again for about 20 years, and is roughly constant after about 1995. The overall decline in output relative to its counterfactual is large. By then end of the sample period, output is down to 62 percent of its counterfactual. This large effect arises from compounding the reduction in output's growth rate caused by regulation. As noted above, the average reduction in output's growth rate is about 0.008, so with a sample period of 54, we have $1/(1.008)^{54} = 0.65$. This value is nearly equal to the observed value of 0.62. The small discrepancy reflects the fact that the regulation-induced reduction in output's growth rate is not constant but rather varies over time, as shown in Figure 5.

4.4.C.2. Opportunity Cost of Regulation. Attempts in the literature to measure the cost of regulation have been confined to compliance cost. The estimates are large. For example, Crain and Hopkins (2001) estimate the cost of all federal regulation (not just post-1949 regulation) to be about 8 percent of current GDP, or about \$1 trillion in 2005.¹⁷ Our results suggest another element of cost that has not been considered previously: the opportunity cost

¹⁶For example, in the trend term we replace R_t with $(R_t - R_{1949}) + R_{1949} \equiv \Delta R_t + R_t$ and replace R_t^2 with $[(R_t - R_{1949}) + R_{1949}]^2 = (\Delta R_t)^2 + 2R_{1949}\Delta R_t + (R_t)^2$, multiply by the appropriate estimated trend coefficients (γ and δ), and collect all terms containing ΔR_t . Similarly, in the cycle term we replace R_t with $R_{1949}(R_t/R_{1949})$, raise to appropriate estimated power (ω), and collect all terms involving ratios of the form $(R_t/R_{1949})^\omega$. Finally, dividing actual output by the trend arising from the terms containing ΔR_t and by the cycle components $(R_t/R_{1949})^\omega$ is equivalent to setting ΔR to zero and $(R_t/R_{1949})^\omega$ to one, thus giving a counterfactual value of output under the restriction that regulation remained at its 1949 level.

¹⁷This 8 percent excludes the cost of tax compliance, which Crain and Hopkins included. We exclude tax compliance cost because taxes generally are not considered regulations. Tax compliance cost amounts to about one half of one percent of GDP.

arising from reduced GDP. The magnitude is several times larger than the compliance cost. The ratio of actual to counterfactual output was 0.62 in 2002. If we assume that the same ratio applies today, we can calculate immediately the opportunity cost of regulation added since 1949. In 2006, nominal GDP was \$13.5 trillion. Had regulation remained at its 1949 level, current GDP would have been about \$21.77 trillion, an increase of \$8.27 trillion. It amounts to about \$61,250 per household or \$27,600 per person. Another way to put it, perhaps a little too dramatically, is that each page of federal regulation added since 1949 today costs the economy roughly \$79.2 million in foregone output. Furthermore, our estimates indicate that the opportunity cost will grow at a rate of about 0.87 percent a year (the amount by which output's trend was reduced in 2002), *if* regulation is merely kept at its 2002 level and not increased further.¹⁸

Several aspects of the output opportunity cost are noteworthy. First, our figures are net costs. They are based on the change in total product caused by regulation and so include positive as well as negative effects. Our results thus indicate that whatever positive effects regulation may have on *measured* output are outweighed by the negative effects. Second, the foregone output does not measure the total opportunity cost because, as we have seen, regulation has increased employment and therefore reduced leisure, and because some current output may be devoted to avoiding regulation and thus may not be inherently valuable. Third, the opportunity cost is over eight times as large as the compliance costs. Fourth, our measure does not include non-production benefits of regulation. The non-production benefits could be both large and growing. Pollution, for example, presumably grows as unregulated industrialization expands. The cost of pollution may grow non-linearly. Those costs have been reduced by environmental regulation, and if regulation has reduced the growth rate of pollution, then it correspondingly has introduced a growing benefit that is not included in measured output. We do not attempt to measure such benefits here, confining our analysis strictly to measured output. Consequently, we emphasize that our results offer no conclusion on whether regulation is a net social benefit. They do, however, make clear that the cost of regulation is

¹⁸Of course, because we have restricted our functional form to a quadratic, ridiculous results can be obtained by extrapolating far beyond the sample period. If regulation continues to grow, the negative terms take over and eventually output growth becomes negative, driving output toward zero as time passes. Such behavior obviously would not be tolerated by society, and the process governing the evolution of regulation would change. The problem is exactly the same as using a quadratic utility function to approximate the true function: it can work quite well locally but will give nonsensical results if abused. These problems of extrapolation are not relevant to our discussion here, which is confined to behavior within the sample period.

substantial and must be taken seriously in any evaluation of regulation's net social benefit. Fifth, our estimated opportunity cost pertains only to regulation added since 1949. We have no way to measure the opportunity cost associated with regulation up to 1949. It seems certain that some regulation has a negative opportunity cost, that is, a net positive effect on GDP. Surely GDP were lower in the absence of traffic regulations or organized patent procedures. However, most of those most basic regulations were in place well before 1949, so for our work their benefits are simply a given impounded in the intercept.

4.4.C.3: TFP and the Productivity Slowdown. Figures 7 and 8 plot the growth rates of output and TFP. It is clear from the Hodrick-Prescott filtered series that the two variables move closely together with closely matched turning points. It therefore seems especially worthwhile to examine in some detail the effects that regulation has on TFP. Comparing Figure 8 with Figures 1, 2, and 4 suggests that regulation and taxes had something to do with TFP's behavior. TFP growth starts falling sometime in the mid 1960s, stops falling in the early 1980s, and grows slowly after that. The marginal tax rate and the growth rate of regulation began rising at almost exactly the same time as TFP's growth rate turned down in the mid 1960s. Regulation's growth peaked in the second half of the 1970s, a little before TFP turned back up, and even became negative in the mid-1990s (see Figure 2). Tax rates peaked at almost exactly the same time that TFP bottomed out, stopping their rise around 1980 and falling somewhat afterward. Clearly, major changes in TFP correspond to major changes in R and T.

We can use our parameter estimates to quantify this impressionistic visual analysis. The second column of Table 4 reports the TFP estimates. Using those in the same way as we did for output, we can calculate the effects of regulation and taxes on TFP. Figure 9 shows the effect of regulation on TFP's trend. The effect is negative throughout the sample period, but there is sharp increase in the steepness of the slope in the early 1970s, corresponding to the large increase in environmental and occupational, health, and safety regulation. Regulation has no cycle effect on TFP (all the ω coefficients are zero), the trend effect is the only effect. As a result, the ratio of actual TFP to counterfactual TFP, shown in Figure 10, has essentially the same shape as the trend effect in Figure 9.

Taxes also play a role in explaining TFP growth. Taxes enter the TFP equation only through the cycle effect, indicating that they have a trend-intercept effect on TFP's trend but no time-varying linear or quadratic effects. Figure 11 plots the ratio of actual TFP to counterfactual TFP obtained by holding tax rates at their 1949 level. The slope becomes substantially more negative in the mid-1960 and then becomes positive in the early 1980s,

matching the changes in TFP slope.

These two sets of results suggest that changes in regulation and tax rates go a long way toward explaining the famous TFP slowdown of the 1970s.

This explanation for the TFP slowdown is quite different from that offered by Greenwood and Yorukoglu (1997), who suggest that the TFP slowdown resulted somewhat paradoxically from an improvement in technology that required a period of learning before it could be used to full effect. Their theory is quite a complicated story, whereas our story is quite straightforward: high marginal tax rates and heavy regulation reduced returns to knowledge accumulation and so reduced the accumulation itself. Greenwood and Yorukoglu's theory is in no way incompatible with our proposed explanation, and both could be operative. It is clear from the data, however, that Greenwood and Yorukoglu's theory cannot be the complete explanation because the timing is wrong. Greenwood and Yorukoglu base their theory on the large amount of investment expenditure on information technology that began in the late 1970s. The TFP slowdown, however, began about a decade earlier, as shown in Figure 8.¹⁹ That timing corresponds closely to changes in regulation and marginal tax rates. Our proposed explanation is at least as plausible as Greenwood and Yorukoglu's on empirical grounds and is far simpler theoretically.

4.4.C.4: Consistency with Cross-Section Studies. Our time series results are consistent with most findings of earlier cross-sectional and panel studies. For example, Djankov et al. (2005) and Loayza et al. (2004, 2005) report a negative relation between economic growth and the extent and/or “quality” of regulation. Nicoletti and Scarpetta (2003) find a negative relation between TFP and regulations that restrict competition or entry, and Bassanini and Ernst (2002) find a similar relation for R&D, which presumably drives TFP. The only suggestion of a discrepancy between our results and those of earlier studies pertains to employment. Nicoletti et al. (2001) find that labor market regulations reduced employment in the OECD cross-section of countries, implying a negative effect on measured trend in employment, whereas we find that total regulation reduced U.S. employment's trend at first but later increased it.

4.5. Other Models Explored.

¹⁹Nordhaus's (2004) explanation has the same problem. He argues that the productivity slowdown resulted from the OPEC oil shock of the early 1970s, but again the timing is wrong because the slowdown started at least five years earlier than that.

We explored a few other models. First, we tried a difference-stationary ARMA model, extended to allow a time-varying drift rate:²⁰

$$(6) \quad \Delta x_t = \beta + \sum_{j=1}^{J_1} \gamma_j Z_{t-i} + \sum_{j=0}^{J_2} \delta_j Z_{t-j}^2 + \sum_{j=1}^{J_3} \eta_j \Delta \ln x_{t-j} + \sum_{j=1}^{J_4} \theta_j e_{t-j}.$$

As in the trend-break model, Z is the vector (R, T) and the coefficients γ and δ also are vectors. Lag lengths were chosen by minimizing the Schwarz information criterion, as for the trend-break model. The patterns and joint significance tests of the coefficient estimates lead to the same conclusions as those in the trend-break model.

We also explored cointegration among variables and the possibility of estimating an error-correction model. The results were uninformative. Johansen (1988, 1991) tests indicate the presence of multiple cointegrating vectors among the variables $Y, N, K, R, G,$ and T . This result suggests a long-run relationship exists between these variables, but interpretation of individual parameter estimates is unclear in the case of multiple cointegrating vectors. In addition, the number of cointegrating vectors is sensitive to assumptions about lag lengths, trends in the underlying data, the choice of variables included in the analysis, and the inclusion of constant and trend terms in the cointegrating relations themselves, making the choice between alternative models largely arbitrary. These results are not surprising if regulation affects the aggregate economy in the highly nonlinear fashion suggested by our trend-break model, for cointegration presumes a linear relation.

Furthermore, both the difference-stationary model and cointegration framework require the individual series to be unit-root processes. Although Phillips-Perron tests fail to reject the unit-root hypothesis in the series under examination, it is well known that conventional unit-root tests often fail to reject the unit-root null when the true data generating process is trend-break stationary (Perron, 1989). The underlying aggregate variables being trend-break stationary (as is the current consensus among macroeconomists) would be another reason for the poor performance of the difference-stationary and cointegration models.

Finally, we tried to isolate the CFR titles with significant impacts on our aggregate dependent variables.²¹

²⁰Phillips-Perron tests indicate that all variables have unit roots.

²¹Note that disaggregation by title is not the same as disaggregation by industry affected, nor does it necessarily capture all regulations of a general type. “Agriculture” and “Animals and Animal Products” are separate titles that both affect the agriculture industry. “Banks and Banking” is a title that may affect many industries. For some purposes, it might be preferable to measure some group of related regulations, such as all regulations pertaining to agriculture. This is the approach taken in some of the literature cited in the Introduction.

We regressed the five dependent variables on the set of page counts for the individual titles instead of the page count of all titles taken together. The large number of titles leaves us too few degrees of freedom to include even one lag in the titles or to include both level and trend effects, so we ran a simplified regression of the form

$$x_t = \alpha + \left[\beta + \sum_{w=7}^{50} \gamma_w r_w \right] t + u_t \quad \text{and} \quad x_t = \alpha + \beta t + \sum_{w=7}^{50} \omega_w r_w + u_t$$

where w indexes the CFR titles.

For each dependent variable, a small subset of the titles had coefficients individually significant at the 10 percent level or less. However, a joint test of the remaining titles always strongly rejected the null that the remaining titles were jointly insignificant (p-values of 0.000 in every case), implying that at least some of the individually insignificant titles are in fact significant. We thus could exclude no individual title and so could not identify which individual titles were significant for any dependent variable. This outcome is almost certainly due to the small number of degrees of freedom when all titles are included individually. Low degrees of freedom causes large standard errors and prevents inclusion of lagged regulation, which our previous analysis found to be highly significant and whose exclusion therefore biases the estimation.

5. Conclusion

We have presented a new time series measuring the extent of federal regulation in the United States, and we have used it to examine the effect of regulation on the macro dynamics of several aggregate variables of interest. We find that post-1949 regulation has statistically and economically significant effects on the time paths of output, total factor productivity, labor, physical capital, and investment. Regulation alters both trends and movements about the trends. The trend effects usually are complex and non-linear. The cycle effects have lag lengths and coefficient sign patterns that differ across the dependent variables. Regulation has allocative effects, changing the mix of factors used to produce output.

Regulation's overall effect on output's growth rate is negative and substantial. Federal regulations added over the past fifty years have reduced real output growth by about eight-tenths of a percentage point on average over the period 1949-2002. The main channel through which regulation has reduced output is TFP. We find that federal regulation, together with changes in marginal tax rates, can explain much of the famous and famously puzzling TFP

slowdown of the 1970s.

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Table 1
Basic Statistics for CFR Page Count Series

Number of Pages

Starting Year, 1949	19335
Ending Year, 2002	123772

Growth Rate of Number of Pages

Max	0.175	in 1950
Min	-0.052	in 1961
Mean	0.035	
Median	0.027	
Standard Deviation	0.040	

Correlations

In Number of Pages

Max	0.989	between titles 17 (Commodity and Securities Exchanges) & 36 (Parks, Forests, and Public Property)
Min	-0.764	between titles 26 (Internal Revenue) & 41 (Public Contracts and Property Management)
Mean	0.603	
Median	0.766	
Standard Deviation	0.398	

In Growth Rate of Number of Pages

Max	0.739	between titles 24 (Housing and Urban Development) & 43 (Public Lands: Interior)
Min	-0.628	between titles 16 (Commercial Practices) & 46 (Shipping)
Mean	0.160	
Median	0.153	
Standard Deviation	0.219	

Table 2
Granger Causality Examples: Growth Rates of Number of Pages

Title 16 - Commercial Practices

Granger-causes: 12 (Banks and Banking), 15 (Commerce and Foreign Trade), 17 (Commodity and Securities Exchanges), 18 (Conservation of Power and Water Resources), 20 (Employees' Benefits), 21 (Food and Drugs), 33 (Navigation and Navigable Waters), 38 (Pensions, Bonuses, and Veterans' Relief), 46 (Shipping), 47 (Telecommunication), 49 (Transportation), 50 (Wildlife and Fisheries)

Is Granger-caused by: 7 (Agriculture), 8 (Aliens and Nationality), 15 (Commerce and Foreign Trade), 21 (Food and Drugs), 22 (Foreign Relations), 24 (Housing and Urban Development), 30 (Mineral Resources), 36 (Parks, Forests, and Public Property), 38 (Pensions, Bonuses, and Veterans' Relief), 47 (Telecommunication)

Title 29 - Labor

Granger-causes: 13 (Business Credit and Assistance), 17 (Commodity and Securities Exchanges), 33 (Navigation and Navigable Waters)

Is Granger-caused by: 18 (Conservation of Power and Water Resources), 23 (Highways), 24 (Housing and Urban Development), 42 (Public Health)

Table 3
Bivariate Granger-Causality Tests, 1949-2002

Null Hypothesis	q	N	F	p -value
$\Delta r \nrightarrow \Delta y$	4	49	2.42	0.0643
$\Delta y \nrightarrow \Delta r$	4	49	0.55	0.7010
$\Delta r \nrightarrow \Delta tfp$	4	49	2.71	0.0434
$\Delta tfp \nrightarrow \Delta r$	4	49	1.03	0.4035
$\Delta r \nrightarrow \Delta n$	4	49	1.26	0.3026
$\Delta n \nrightarrow \Delta r$	4	49	2.08	0.1013
$\Delta r \nrightarrow \Delta k$	4	49	1.31	0.2831
$\Delta k \nrightarrow \Delta r$	4	49	1.82	0.1433

Notes: The variables are: r (regulation), y (output), tfp (total factor productivity), k (physical capital), n (labor), where lower case letters denote the natural logs of the variables in question. Δ is the first difference operator; q is the number of lagged variables in the estimated equation; N is the number of observations.

Table 4 - Part I
Trend-Break Model Estimation
Regulation Parameters

$$x_t = \alpha + [\beta + \sum_{j=1}^{J_1^R} \gamma_j^R R_{t-j} + \sum_{j=0}^{J_2^R} \delta_j^R R_{t-j}^2 + \sum_{j=0}^{J_1^T} \gamma_j^T T_{t-j} + \sum_{j=0}^{J_1^T} \delta_j^T T_{t-j}^2]t + \sum_{j=0}^{J_3^R} \omega_j^R r_{t-j} + \sum_{j=0}^{J_3^T} \omega_j^T \tau_{t-j} + \sum_{j=0}^{J^u} u_{t-j}$$

	x = y	x = tfp	x = n	x = k
γ_0^R	3.94E-07 (1.02E-07)	2.45E-08 (1.42E-08)	5.14E-08 (1.02E-08)	5.63E-07 (6.24E-08)
γ_1^R	-	-2.72E-08 (1.88E-08)	-	-
γ_2^R	-	-4.74E-08 (1.94E-08)	-	-
δ_0^R	-1.27E-12 (4.97E-13)	-	-	-3.01E-12 (3.56E-13)
δ_1^R	1.35E-13 (2.70E-13)	-	-	-
δ_2^R	-1.29E-12 (3.02E-13)	-	-	-
ω_0^R	-0.447 (0.086)	-	-	-0.262 (0.061)
$\beta_R \omega_0^R$	-0.015	-	-	-0.010
$\Sigma \gamma_i^R$	-3.94E-07	-5.01E-08	5.14E-08	5.63E-07
{ χ^2 test: I = 0}	{NA}	{189.044}	{NA}	{NA}
[p-value]	[NA]	[0.000]	[NA]	[NA]
$\Sigma \delta_i^R$	-2.43E-12	-	-	-3.01E-12
{ χ^2 test: I = 0}	{17.187}			{NA}
[p-value]	[0.000]			[NA]

Numbers in parentheses (.) are Newey-West corrected standard errors, brackets [.] are p-values, braces {.} are χ^2 values. Numbers do not always add because of rounding. NA means Not Applicable because there was only one non-zero parameter, making the sum trivial and calculation of χ^2 tests superfluous.

Table 4 - Part II
Trend-Break Model Estimation
Tax Parameters, Other Parameters, Other Statistics

$$x_t = \alpha + [\beta + \sum_{j=1}^{J_1^R} \gamma_j^R R_{t-j} + \sum_{j=0}^{J_2^R} \delta_j^R R_{t-j}^2 + \sum_{j=0}^{J_1^T} \gamma_j^T T_{t-j} + \sum_{j=0}^{J_1^T} \delta_j^T T_{t-j}^2]t + \sum_{j=0}^{J_3^R} \omega_j^R r_{t-j} + \sum_{j=0}^{J_3^T} \omega_j^T \tau_{t-j} + \sum_{j=0}^{J^u} u_{t-j}$$

	x = y	x = tfp	x = n	x = k
γ_0^T	0.085 (0.020)	-	0.0340 (0.014)	-
γ_1^T	-0.102 (0.017)	-	-0.047 (0.023)	-
δ_0^T	-	-	-	-0.165 (0.034)
ω_0^T	-	0.052 (0.035)	-	0.358 (0.053)
ω_1^T	-	-0.130 (0.025)	-	-
$\beta_T \Sigma \omega_i^T$	-	-0.001	-	0.014
$\Sigma \gamma_i^T$ { χ^2 test: $\Sigma \gamma_i = 0$ } [p-value]	-0.0168 {0.457} [0.499]	-	-0.007 {0.206} [0.650]	-
$\Sigma \omega_i^T$ { χ^2 test: $\Sigma \omega_i^T = 0$ } [p-value]	-	-0.0785 {8.930} [0.003]	-	0.358 {NA} [NA]
α	11.61053 (0.875)	0.388 (0.053)	4.292 (0.012)	11.048 (0.692)
β	0.037 (0.003)	0.012 (0.001)	0.012 (0.003)	0.026 (0.002)
Adj. R ²	0.997	0.989	0.988	.999

Numbers in parentheses (.) are Newey-West corrected standard errors, brackets [.] are p-values, braces {.} are χ^2 values. Numbers do not always add because of rounding. NA means Not Applicable because there was only one non-zero parameter, making the sum trivial and calculation of χ^2 tests superfluous.

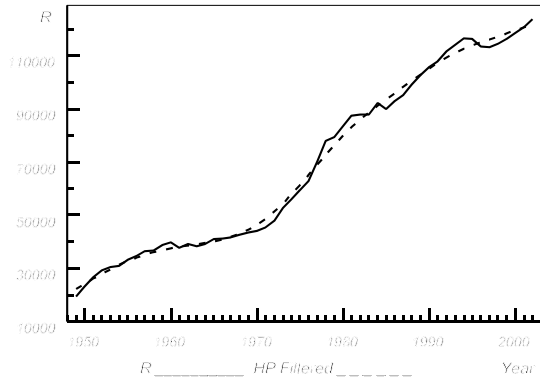


Fig. 1: Regulation over time

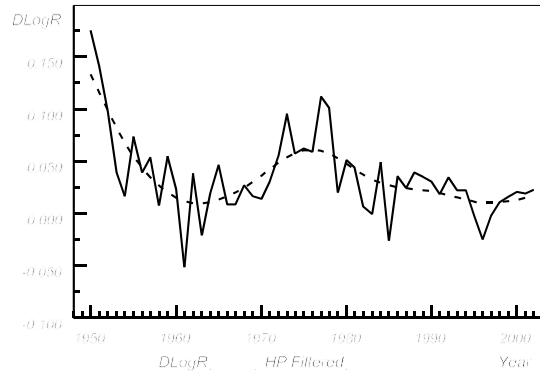


Fig. 2: Growth rate of regulation over time

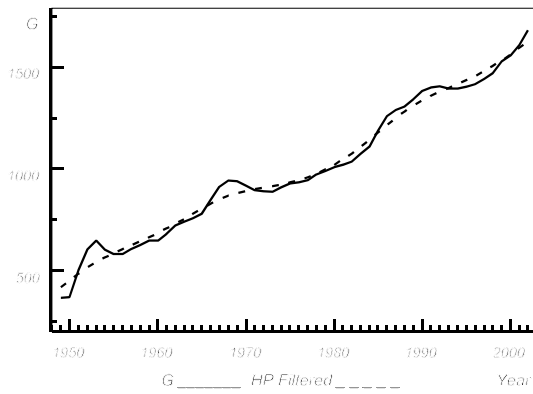


Fig. 3: Government purchases over time

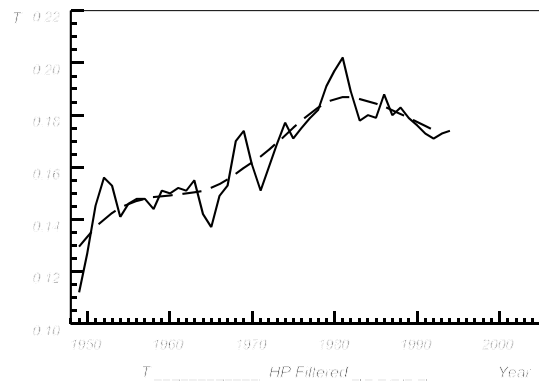


Fig. 4: Marginal tax rate over time

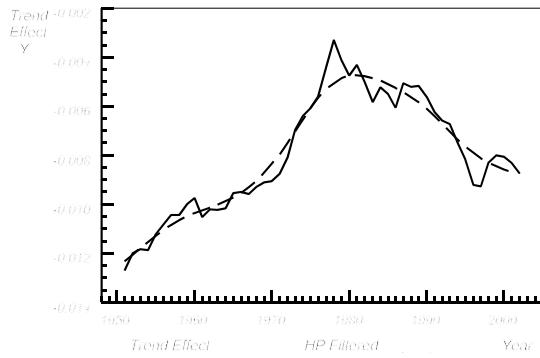


Fig. 5: Effect of R on Y trend



Fig. 6: Ratio of actual Y to counterfactual $Y|_{R=R(1949)}$

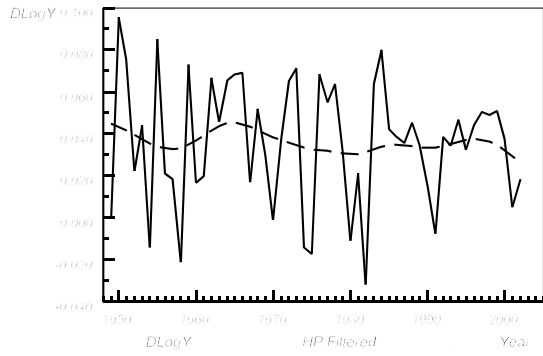


Fig. 7: DLogY

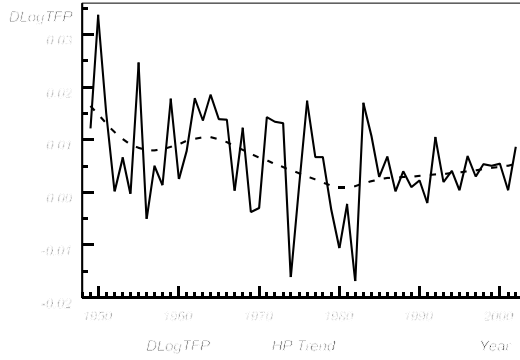


Fig. 8: DlogTFP

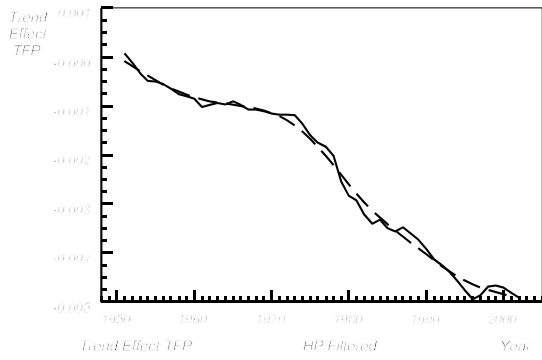


Fig. 9: Effect of R on TFP trend

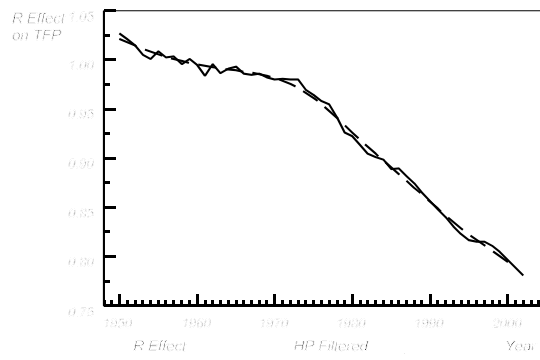


Fig. 10: Ratio of actual TFP to counterfactual TFP_{R=R(1949)}

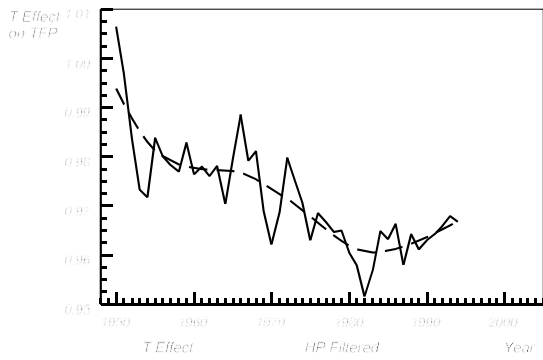


Fig. 11: Ratio of actual TFP to counterfactual TFP_{T=T(1949)}

Appendix: Code of Federal Regulations

1. History and Background of the Code of Federal Regulations

Before 1935, no systematic process existed for the promulgation of federal regulations; regulations were simply typed and filed by individual agencies. The lack of public notification regarding regulatory activity later came to be known as "hip pocket" law, which led the government to embarrassment in *Panama Refining Company v. Ryan* (293 U.S. 388, 1935), also known as the "Hot Oil Case."²² The government's case, which was based on a provision that was later nullified by a subsequent regulation, was dismissed by the Supreme Court, and both parties in the case were impugned for their ignorance of the law. This outcome led to the Federal Register Act of 1935 (49 Stat. 500; 44 USC Chapter 15), which established a consistent framework for codification of government regulations throughout the rulemaking process.

The *Federal Register* (FR), first published on March 14, 1936, is a daily publication in which proposed regulations appear first in draft form and eventually in final form, if passed into law. The FR also contains presidential proclamations, executive orders, announcements of agency hearings and meetings on regulatory issues, grant application instructions and deadlines, official agency decisions and actions, and agency establishments, reorganizations, and dissolutions. Sometimes, there also are long sections containing technical or economic analyses or discussion of issues arising during consideration of a proposed regulation. The final regulations (newly passed into law) contained in the FR ultimately are codified in the *Code of Federal Regulations* (CFR). Divided into 50 subject categories called titles, the structure of the CFR is similar, but not identical, to that of the *United States Code*. Currently, each title of the CFR is revised annually and contains all regulations in effect as of the cover date.²³

The first edition of the CFR published regulations in force as of June 1, 1938. In the early years, the CFR was not revised annually. Instead, annual supplements carried in full text all changes and additions to the 1938 edition of the CFR as published in the FR. The supplements covered the periods June 2-December 31, 1938 and subsequent calendar years through 1941, listing regulatory changes promulgated during the period and in effect on December 31 of the year in question.²⁴ The first revision of the CFR, scheduled for June 1, 1943 under the Federal Register Act, was postponed because of the volume of rapidly changing regulations related to World War II and the preoccupation of all government agencies with the war effort. In its place, a *cumulative* supplement to the 1938 edition of the CFR compiled regulations in force as of June 1, 1943. However, regulations in effect at that date whose text was identical to that in the 1938 edition of the CFR are included only by reference to the original CFR. Also, emergency controls associated with the war period are recorded by tabulation rather than codification in the cumulative supplement. Thus, the cumulative supplement served as an adjunct to the original edition rather than a replacement of it. Following the cumulative supplement, annual supplements continued to update the 1938 edition of the CFR for regulatory changes published in the FR during the remainder of 1943 and each calendar year through 1947. The wartime suspension of the first revision of the CFR was terminated in 1948 and the second edition of the CFR, recording regulations in effect on January 1, 1949, was issued.²⁵

Following the 1949 edition of the CFR, "pocket supplements" were used to record regulatory changes

²²Throughout the appendix, the following citation format is used: volume or title number followed by name of publication followed by page or section number. For example, "49 Stat. 500" designates Volume 49 of the *United States Statutes at Large*, page 500. The following abbreviations are also used in the citations: USC for *United States Code*, FR for *Federal Register*, and CFR for *Code of Federal Regulations*.

²³The *U.S. Statutes at Large* and *U.S. Code* are comparable to the *Federal Register* and *Code of Federal Regulations*, respectively, except that the former are primarily concerned with the publication and codification of laws, whereas the later are concerned with transmitting to the public written requirements to be carried out and enforced by government agencies (i.e., regulations). Thus, the CFR is more appropriate than the *U.S. Code* as a measure of regulation.

²⁴No supplement was published for 1942.

²⁵Due to the imminence of the second edition of the CFR, no supplement was issued for 1948. Regulatory changes published in the FR during 1948 were codified for the first time in the 1949 edition of the Code.

published in the FR.²⁶ Pocket supplements differed from the annual supplements to the first edition of the CFR in that they were cumulative; that is, the pocket supplement for a given year recorded the full text of all changes to the 1949 CFR in effect at the end of the given year, irrespective of the year that the change occurred. The first pocket supplement covered changes during the June 2 to December 31, 1949 period and subsequent pocket supplements included any additional changes in effect at the end of each succeeding calendar year. So, for example, the 1950 pocket supplement documents changes to the 1949 edition of the CFR that occurred between June 2, 1949 and December 31, 1950. Some of those changes occurred between June 2 and December 31 of 1949 and so already were reported in the 1949 pocket supplement. The 1950 pocket supplement repeats them and adds all changes that occurred between January 1 and December 31 of 1950.²⁷

From time to time, as warranted by growth of the pocket supplements, individual titles (or individual parts of a title) of the 1949 CFR were revised. These revisions represented a complete codification of regulations in effect as of December 31 of the year in which they were published. The timing of revisions varied considerably across titles. In all titles, however, revisions became more frequent over time. In 1950, for instance, only Parts 71-90 of Title 49 (Transportation and Railroads) were revised. In 1960, all or parts of Titles 1-5, 14, 18-20, 26, 27, 32, 40, 41, 49, and 50 were revised, and by 1968, all *except* Titles 34, 35, and 37 were revised. Beginning in 1969, all titles of the CFR have been revised annually.²⁸

2. *Measuring Regulatory Activity Using the CFR*

The consistent codification of federal regulations in the CFR since its inception in 1938 provides a unique source of information on regulatory activity over the years. Dawson (2002) constructs series measuring regulatory activity based on the number of pages published in the CFR's various editions and supplements. Although the number of pages of regulation cannot capture the differential effects of alternative regulations on economic activity, it affords new information on the temporal behavior of the total amount of regulation in place. The remainder of this section provides a summary of these CFR-based measures of regulation. For a complete description of the methodology used to construct the series and a statistical comparison of the various series, see Dawson (2002).

Before counting pages, we must standardize the pages in the CFR for different words per page across the years. That turns out to be almost effortless. The CFR uses the same font and page size in all years except the very first, 1938. We converted 1938 pages to "standard" pages simply by multiplying by an adjustment factor based on average words per page computed by sampling words per page in each title of the Code. Even this adjustment turns out to be irrelevant to our empirical work below because, for reasons to be explained momentarily, we started our sample period in 1949, thus omitting the non-standard 1938 edition of the Code entirely.

Measuring regulatory activity using data on the number of pages in the CFR is straightforward in years when the CFR is revised. These include the years 1938, 1949, all years after 1969, and some years between 1949 and 1969.²⁹ Estimating total pages of regulation during the periods between the 1938, 1949, and subsequent revisions is more problematic. One approach, which explicitly uses all annual and pocket supplement data to estimate total pages of regulation during years in which no revision is published, adds the number of pages in a nonrevision-year's supplement to the number of pages in its corresponding complete CFR. The series that results

²⁶The term "pocket supplement" derives from pockets which were made in the books of the 1949 edition of the CFR for placement of the forthcoming supplements.

²⁷On several occasions, an "added pocket part" (APP) was published instead of a pocket supplement. The APP served as an addition or supplement to the previous year's pocket supplement. APPs were not cumulative unless they appeared in consecutive years, in which case the old APP was replaced by the current APP as a supplement to the most recent pocket supplement.

²⁸Beginning with the 1973 revision of the CFR, the effective revision date of each title varies within the year according to the following quarterly schedule: Titles 1-16 as of January 1; Titles 17-27 as of April 1; Titles 28-41 as of July 1; and Titles 42-50 as of October 1.

²⁹Recall from the discussion above that the timing of revisions to the 1949 edition of the CFR varies across titles between the years 1949 and 1969.

from this methodology exhibits rapid growth in pages of regulation during most of the 1940s followed by a drastic decline in 1949. This behavior in part may reflect the increase in regulation associated with World War II and the subsequent decrease following the war, but it also is likely to reflect in part an element of double counting that is, for practical purposes, unavoidable with the supplements used to codify regulatory changes between the 1938 and 1949 revisions of the CFR. The supplements print the entire text of any section of regulation that changed, even if only one word was different. Consequently, a page of text in a supplement may represent completely new text that was not present in 1938 or may be almost entirely repetition of previously existing text. The only way to avoid double counting repeated text would be to read each reported change to determine how much of it was repetition, an obviously impractical task. Growth in the estimated pages of regulation resumes in the early 1950s and moderates into the 1960s. The same double counting problem exists after 1949 as before but is less severe because revised volumes of the CFR were published intermittently between 1949 and 1969. The frequency of these intermittent updates increased as time passed, with almost the entire CFR being revised in 1968. Consequently, the growth in the CFR page count between 1949 and 1969 is much more likely to be a genuine phenomenon than is the pre-1949 growth. Double-counting ceases to be an issue after 1968 because the entire CFR is published every year after that. Because the counting problems are much more severe before 1949 than after, we restrict attention in our study to the period 1949-1999.³⁰ Also, because we are interested in the effects of regulation on the private economy, we exclude from our page count all regulations in the first six titles, which pertain to the internal organization and operation of the federal government itself.

³⁰Dawson (2002) discusses the “double-counting” problem in more detail and offers some alternative methods for constructing the regulatory series based on interpolation in the non-revision years. The results of the analysis in this paper are not sensitive to the construction method, thus we restrict attention to the series discussed here.