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Regulation and the Macroeconomy

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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, physical capital, and labor. The effects are multifaceted and complex. Regulation changes the way output is produced by changing the mix of inputs. It also affects both the trends and deviations about the trends in output and its factors of production, and the effects differ across dependent variables. The effects display interesting intertemporal dynamics. Changes in regulation and marginal tax rates offer an explanation for the productivity slowdown of the 1970s. Regulation also has substantial opportunity costs in the form of foregone output.

Keywords: Regulation; macroeconomic performance; productivity slowdown

JEL classification: E20; L50; O40

1. Introduction

Macroeconomists often divide government economic activity into four broad classes: purchases, taxation, deficits, and monetary policy. There is, however, a fifth class that has been almost completely ignored in both the theoretical and empirical macroeconomic literature, namely, regulation. Although microeconomists have joined politicians and pressure groups in debating the effects and desirability of regulation, macroeconomists have devoted almost no attention to the impact of regulation on the aggregate economy. Indeed, the only exceptions of which we are aware are Blanchard and Giavazzi's (2003) theoretical study of the macroeconomic effects of product and labor market regulations, Goff's (1996) pioneering and apparently overlooked empirical study, and a small group of empirical papers by economists at the World Bank (Kaufman, Kraay, and Zoido-Lobaton, 1999 and 2002 ; Forteza and Rama, 2003; Kaufman, Kraay, and Mastruzzi, 2003) that study the ability of the extent of government intervention in the economy (including regulation), perceived government effectiveness, and market rigidities to explain cross-country differences in per capita income and growth rates.[1]

In this paper, we present a new time series measure of the extent of United States federal regulation over fifty years. Our measure is the number of pages in the *Code of Federal Regulations* (hereafter, CFR), the US government publication that prints all federal regulations in

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[1]Some empirical work studies regulatory effects related to macroeconomic concerns. Nicoletti and Scarpetta (2003), using OECD data, find that product regulation that creates barriers to entry reduces industry-level multifactor productivity growth; Alesina *et al.* (2003) find that such regulation reduces industrial investment. Purely microeconomic investigations of the cost of regulation have been around for some time. See Goff (1996) for an extensive review of the literature to the mid-1990s. Some kinds of government activity, such as import quotas, are an economic intervention similar in nature to regulation, and their economic impact has been studied extensively. Quotas do not seem to be what people mean by the term "regulation," though, and they certainly are not included in the *Code of Federal Regulations*.

existence during a given year. Any such measure has limitations, of course (e.g., it cannot capture the vigor of enforcement), but it is reasonable to believe that the number of pages of printed regulations is an indicator of the extent of regulation. Federal law requires that all federal regulations be printed in the CFR; if there were no regulations reported in the CFR, there would be no federal regulation, suggesting a positive correlation between the CFR page count and the amount of regulation. Moreover, if our measure lacked content, one would expect it to show no relation to other variables such as output or total factor productivity. In fact, we find very strong relations with turning points in regulation corresponding to turning points in the dependent variables, suggesting that our measure is capturing what we hope it captures. A few researchers have proposed measures similar to ours; see Friedman and Friedman (1979), Becker and Mulligan (1999), and Mulligan and Shleifer (2003).[2] Our measure is more comprehensive and covers a much longer time span than these alternatives.

We use our series to examine regulation's effect on output, total factor productivity (TFP), labor services, capital services, and private investment. Regulation grows almost all 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 begin with Granger-causality, in the spirit of Hamilton's (1983) study of oil and the macroeconomy. We find unidirectional Granger-

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[2]These studies have proposed page counts as a measure of regulation. Friedman 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. As we explain in the Appendix, the CFR is the most appropriate source for measuring federal regulation. Goff (1996) uses factor analysis to construct a clever composite measure of regulation. It spans almost as long a period as ours, and it attempts to capture elements of enforcement vigor, which are absent from our measure. Its main limitation is that, being a factor analysis construct, its meaning is unclear.

causality from regulation to the macro variables. We then estimate reduced-form regressions to explore in more detail the impact of regulation on aggregate economic activity. Regulation added over the last fifty years has reduced aggregate output substantially, both by shifting the level of output down and by reducing output's trend rate of growth. The reduction has varied over time but by the end of our sample has resulted in a net reduction of about ten percent, so that annual output now (about $11 trillion) is about ninety percent of what it would have been (about $12 trillion) if there had been no additions to federal regulation over the past fifty years (the length of our sample). That means the federal regulation added over the past half century now costs the US economy $1 trillion *each year* in foregone output. To put it at a more personal level, the current annual loss of income is about $10,000 per household or $3,600 per person. Alternatively, each page of regulation in the CFR now costs the economy about $9.2 million in foregone output. As of 2000, these numbers were growing (in real terms) at a rate of about two-tenths of a percent per year. In addition, regulation affects the dynamic adjustment paths of all variables in complicated ways; for all variables, the effect of a change in regulation is spread over time. The effects differ for output, TFP, and the factors of production, implying that regulation alters the allocation of resources. The effect of regulation on TFP is especially noteworthy. We find that increases in regulation, together with changes in marginal tax rates, explain the TFP slowdown of the 1970s.

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

We begin with a brief description of the *Code of Federal Regulations* and the measure of regulation that we extract from it. A more complete discussion is in the Appendix.

The CFR was first published in 1938. It was divided into 50 "titles," each of which pertains 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. Between 1938 and 1949, annual supplements to the CFR were published, 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, the annual supplements were replaced by pocket supplements, which were done differently than the annual supplements; also, updated versions of entire titles were published increasingly often. The pocket supplements together with the intermittent revised titles make it possible to construct fairly accurate annual page counts for the CFR between 1949 and 1969. See the Appendix for details on the method of construction. Starting in 1969, the complete CFR has been published annually, so annual page counts can be obtained directly.

Figure 1 shows time series for the total page count of the CFR from 1949 to 1999. Regulation grows almost all the time, but its growth rate varies a great deal. Periods of negative growth are infrequent, and, when negative, the magnitude of the growth rate always is small. By far, the fastest growth occurs in the early 1950s. High growth also occurs in the 1970s, even though that period saw important deregulation in transportation, telecommunications, and energy. Clearly, any deregulation that did occur in those industries is more than offset by increased regulation in other areas, as Hopkins (1991) has noted. The behavior of the regulatory series is equally interesting during the 1980s, when the Reagan administration promoted deregulation as a national priority. Although the growth in the number of pages in the *CFR* slows in the early and late 1980s, a decrease in total pages occurs only in one year, 1985. The

1990s witnessed the largest reduction in pages of regulation in the history of the *CFR*, when three consecutive years of decline are recorded. This coincides with the Clinton administration's "reinventing government" initiative which boasted of reduced regulation in general and a reduction in the number of pages in the *CFR* in particular. 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 1999 (negative to approximately zero growth). As we will see, these segments line up with some interesting behavior of aggregate variables of interest.

3. Estimation

We now examine the relation between regulation and macroeconomic performance. We begin with a discussion of the variables to be examined and the data measuring them; we then turn to Granger causality tests; we end with an examination of two reduced-form regression models.

3.1. 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. 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

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

where A_t is total factor productivity, K_t is capital services, and N_t is labor services. In what follows, we examine how regulation affects *Y* and also *A*, *K*, and *N*. The capital stock, a component of the capital services measure (as explained immediately below), moves very slowly and with a great deal of persistence, which may make it difficult to uncover a relationship between capital and regulation. In contrast, the gross change in the capital stock - gross investment - is much less persistent than the stock itself. We therefore also examine the relation between regulation and gross private domestic investment.

3.2. Data: Sources and Issues

The measure of 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 taken from the *Monthly Labor Review*.[3] 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. Total factor productivity (*TFP*) is the Solow residual from a Cobb-Douglas production function assuming a capital share of thirty percent. Gross private domestic investment (*I*) is taken from the National Income and Product Accounts (NIPA). In addition, our regression analysis includes two explanatory variables other than regulation: total government spending (*G*) and the federal marginal tax rate on personal income (*T*). Government spending is taken from

[3]Historical data on these variables from 1949-1999 are available at www.bls.gov/mfp.

NIPA. The marginal tax rate is from Stephenson (1998) and includes both the personal income tax and the Social Security tax. Figures 2 and 3 show the time series for *G* and *T*. All data are annual observations for the U.S. over the period 1949 to 1999 except for marginal tax rates, which end in 1995.

Phillips-Perron tests indicate that the logarithms of all variables except capital are integrated order one, implying stationarity of growth rates but not levels. Test results for capital are somewhat anomalous, indicating that the logarithm of capital is integrated order two. This result implies that not only the level of capital but also its growth rate are non-stationary, which is inconsistent with the result obtained for investment. A simple calculus exercise shows that, in steady state, the growth rate of any variable *X* equals the growth rate of its change dX/dt. Investment is the change in capital, so the growth rates of capital and investment should be the same. The growth rate of investment appears to be stationary, whereas the growth rate of capital appears to be first-order integrated. Economic theory suggests that capital, investment, and output all should have the same order of integration; on a balanced growth path, their growth rates all should be constant. We therefore conduct our analysis of the difference-stationary models under the assumption that the log of capital is integrated order one. We have examined the effect of treating capital as integrated order two and found that the results were qualitatively unchanged from those we report here.

3.3. Granger Causality Tests

We are not aware of any well-developed theory of how regulation and the macroeconomy interact, so it seems prudent to begin our exploration with the completely non-structural method of Granger causality tests (Feige and Pearce, 1979; Granger, 1969, 1988; Jacobi, Leamer, and

Ward, 1979; Sims, 1972; Zellner, 1988). These tests allow us to examine two questions at once. First, in the spirit of Hamilton's (1983) pioneering study of oil and the macroeconomy, the tests give direct evidence on the question of whether regulation plays any role in determining the behavior of the macro variables of interest. Second, the tests provide evidence on whether regulation is endogenous or exogenous, an issue relevant to the structural estimation that we pursue later.

The results of the bivariate Granger-causality tests between regulation and each of the macroeconomic variables are reported in Table 1 for the period 1949-1999. The tests are conducted on the first-differences of the logs in order to have stationary variables. In Table 1, *y*, *tfp*, *n*, *k*, *i*, and *r* denote the logs of output, total factor productivity, labor, capital, investment, and regulation, respectively. The notation $\Delta r \nRightarrow \Delta x$ refers to the test that Δr does not Granger-cause Δx. Table 1 reports the results for a lag length of 4; results were similar for lag lengths of 3 or 5, with the exception of *K*, as we explain momentarily. The tests indicate that regulation Granger-causes *Y*, *TFP*, and *I* and that *Y*, *TFP*, and *I* do not Granger-cause regulation. For *K*, the results are the reverse: regulation does not Granger-cause *K*, but *K* does Granger-cause regulation. For *N*, there is no Granger-causality in either direction. The results for *K* and *I* are inconsistent. Investment is the change in capital, so it seems reasonable to expect the same causality results for *K* and *I* with respect to *R*. We therefore explored using longer lags for both *I* and *K*. The causality results for *I* are unchanged by adding more lags; in contrast, the results for *K* change. Starting with six lags, the test results for *K* stabilize, showing no evidence of causation running in either direction between *R* and *K*. This result is more consistent with that for *I* (showing that *R* causes *I*) but still not completely consistent. Our interpretation is that the longer lags required for stable test results for *K* probably reflect the great persistence of *K*. The absence of Granger-causality from *R* to *K* even though there is causality from *R* to *I* probably reflects the very small size of annual (net) investment relative to the capital stock. In summary, then, we have unidirectional causation running from regulation (*R*) to output (*Y*), total factor productivity (*TFP*), and investment (*I*); and no causation in either direction between regulation (*R*) on the one hand and labor (*N*) or capital (*K*) on the other.[4] These results have two important implications.

First, the tests strongly suggest a relation between regulation and several macroeconomic variables. In studying the effect of oil and the macroeconomy, 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.[5] If we were to stop here, we would draw a similar conclusion about regulation.

Rather than stop here, however, we continue below with an examination of the structural relation between regulation and the macroeconomy. In doing that, the second implication of the Granger causality tests is relevant, namely, that regulation is econometrically exogenous. This

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[4]We also performed Granger-causality tests on the individual titles of the *CFR* rather than the aggregate measure. Few of the individual areas of regulation 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 50 observations, the tests have few degrees of freedom, leaving the results uninformative. This problem with degrees of freedom arises again later when we explore structural relations between regulation and macroeconomic variables. We discuss it in more detail there.

[5]Goff's (1996, Chapter 7) empirical analysis of the macroeconomic effects of regulation amount to a limited form of Granger causality tests. He estimates an ARMA model for each of several macro variables and then adds a few lags of his factor analysis measure of regulation to the model, effectively performing a test of whether regulation Granger-causes the dependent variables. He finds that regulation does cause the dependent variables.

strong result is very convenient because it means we do not have to worry about how to model any simultaneity between regulation and our dependent variables, something for which there would be essentially no theoretical basis.

We now turn to reduced-form regressions to explore in more detail the relations suggested by our Granger causality tests.

3.4. Regression Models

Phillips-Peron tests indicate that all the series are non-stationary in levels, which is consistent with the visual evidence that all the series have strong upward trends. There is considerable disagreement in the profession over how to interpret non-stationarity, with trend-stationarity, trend-stationarity with trend breaks, difference stationarity, and fractional integration all offered as plausible models (e.g., Nelson and Plosser, 1982; Murray and Nelson, 2000; Diebold and Inoue, 2001). We therefore examine the two most widely-accepted models: (1) linearly detrended with trend breaks and (2) first-differenced. The current consensus seems to favor trend-break models, so we give those more emphasis than the first-difference alternatives.

3.4.A. Trend-Break Model. The trend-break model allows government policy variables including regulation to explain the breaks in the trend of the dependent variable as well as movements about trend. We start with a simple generic model to facilitate discussion of the issues. 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:

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

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

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

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.

In (1), the first term in parentheses is a trend term; the trend coefficient is a function of regulation and relation squared, allowing regulation to cause breaks in trend. The usual practice in estimating models with trend breaks is to use an atheoretical time series approach of searching for the breaks with a statistical procedure. In contrast, the foregoing model takes a structural approach, allowing regulation to be the cause of trend breaks. This model nests within it the simpler linearly detrended model with constant trend ($\gamma_i = 0$ and $\delta_j = 0$ for all I and j).[6]

The second large term in parentheses in (1) is 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:

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[6]That is, models of the form

$$x_t \;=\; \alpha + \beta t + \sum_{i=0}^{I_x} \omega_i z_{t-i} + u_t .$$

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

where $\mathbf{T}$ and $A \,\mathbf{/}\, e^{\alpha + \alpha_Z}$ are constants, *U* is a normally distributed residual, and W is the log-normally distributed compound residual $UV^{\mathbf{T}}$. We see from the last line that the total trend in *X* is $\mathbf{\$} + \mathbf{T\$}_Z$. 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 $\mathbf{\$}$, not $\mathbf{\$}+\mathbf{T\$}_Z$. In some of what follows, we will discuss counterfactual paths that would have emerged if exogenous variables had been held constant. Doing that requires distinguishing among the quantities $\mathbf{\$}$, $\mathbf{T\$}_Z$, and $\mathbf{\$}+\mathbf{T\$}_Z$.

Applying these results to (1) leads to

(3)
$$
\begin{aligned}
X_t &= \left(e^{[\beta + \beta_Z \sum_{j=0}^{J_3} \omega_j + \sum_{j=0}^{J_1} \gamma_j Z_{t-j} + \sum_{j=0}^{J_2} \delta_j Z_{t-j}^2]\, t} \right) \left(e^{\alpha + \alpha_Z} \left[\prod_{j=0}^{J_3} V_{t-j}^{\omega_j} \right] U_t \right) \\
&= e^{B(Z_t)t} \left(e^{\alpha + \alpha_Z} \left[\prod_{j=0}^{J_3} V_{t-j}^{\omega_j} \right] U_t \right)
\end{aligned}
$$

The trend is $B(Z_t) = \mathbf{\$} + \mathbf{\$}_Z\, \mathbf{ET}_j + \mathbf{E(}_j\, Z_{t-J} + \mathbf{E*}_j\, Z_{t-j}^2$, a quadratic function of *Z*. The first term $\mathbf{\$}$ 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 $\mathbf{\$}$ as the *trend-apart effect* (because it captures the trend that would be present if all the exogenous variables were trendless), the second term $\mathbf{\$}_{Z}\mathbf{E}\mathbf{T}_{j}$ 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 (3), the term in large parentheses is the detrended intercept of *X*, consisting of two parts. The first part is the constant, $e^{\alpha + \alpha_Z}$, and the second part is the compound residual, $U(\mathbf{A}V^{\mathbf{T}})$. Because *X* is trend-stationary, the compound residual is purely transient, causing fluctuations about trend. We refer to this transient as the *cycle effect*.[7]

In this model, an exogenous variable *Z* can have two types of permanent effects on an endogenous variable *X*. First, a permanent change in the level of *Z* permanently alters the level of *X* if the sum of the $\mathbf{T}_i$ coefficients differs from zero. Second, a permanent change in the level of *Z* alters the trend in *X* if any of the $\mathbf{(}_i$ or $\mathbf{*}_i$ coefficients differ from zero; this alteration is permanent if the sums of the $\mathbf{(}_i$ or $\mathbf{*}_i$ coefficients differ from zero.

<u>*3.4.B. Difference-Stationary Model.*</u> As an alternative to the foregoing trend-break model, we examine a difference-stationary ARMAX of the form:

$$(4) \qquad \Delta x_t = \left(\beta + \sum_{j=0}^{J_1} \gamma_j Z_{t-j} + \sum_{j=0}^{J_2} \delta_j Z_{t-j}^2\right) + \sum_{i=1}^{I} \eta_i \Delta x_{t-i} + \sum_{j=0}^{J_3} \omega_j \Delta z_{t-j} + \sum_{m=0}^{M} \theta_{t-m} e_{t-m}$$

where the first term in parentheses is the drift, a quadratic function of the exogenous variable *Z*. In light of the discussion of the trend-break model, the meaning of the terms in this difference-stationary model is straightforward and requires no further discussion.

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[7]The *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 $\mathbf{\$}$, so that *U* captures the transient components.

3.5. Estimation Results.

For both models, we examine the sensitivity of our endogenous macro variables to three policy variables: regulation *R*, the average marginal tax rate *T*, and government purchases *G*. We have not pursued the possibility of decomposing *G* into major parts (such as federal versus 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 harmless in any case if Ricardian equivalence holds.

3.5.A. Trend-Break Model. The equation to be estimated is

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

This form corresponds to equation (1), 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). It turns out that *T* and *G* affect only intercepts; that is, the optimal lag lengths J_1^G, J_1^G, J_1^G, and J_1^G all are zero. (Equivalently, $(_j^G$, $*_j^G$, $(_j^T$, and $*_j^T$ all are zero.) To simplify notation, we therefore write the estimating equation as

$$X_t = \left(e^{[\beta + \sum_{j=0}^{J_1^R} \gamma_j^R R_{t-J} + \sum_{j=0}^{J_2^R} \delta_j^R R_{t-j}^2]\, t} \right) \left(e^{\alpha} \left[\prod_{j=0}^{J_3^R} R_{t-j}^{\omega_j^R} \right] \left[\prod_{j=0}^{J_3^T} T_{t-j}^{\omega_j^T} \right] \left[\prod_{j=0}^{J_3^G} G_{t-j}^{\omega_j^G} \right] U_t \right). \qquad (6)$$

Taking logs gives

$$x_t = \alpha + [\beta + \sum_{j=0}^{J_1^R} \gamma_j R_{t-J} + \sum_{j=0}^{J_2^R} \delta_j R_{t-j}^2] t + \sum_{j=0}^{J_3^R} \omega_j^R r_{t-j} + \sum_{j=0}^{J_3^T} \omega_j^T \tau_{t-j} + \sum_{j=0}^{J_3^G} \omega_j^G g_{t-j} + u_t \tag{7}$$

which is the equation actually estimated.

Table 2 reports the estimation results for the five macro variables of interest. Part I of Table 2 reports estimates for all parameters pertaining to regulation; Part II reports all other parameter estimates. The lag lengths J_x were chosen by imposing a large initial value and searching over all possible smaller values to find that which minimized the Schwarz information criterion. The residuals were serially correlated, so we used a Newey-West correction.[8] We focus our attention on Part I of Table 2 because regulation is the object of study.

Regulation has significant but different effects on all five dependent macro variables. Regulation affects the trend of all five variables in some way, and it has cycle effects on all variables except TFP. These effects enter with distributed lags, implying that changes in regulation cause non-trivial dynamic responses in the dependent variables. Also, the number of terms in the lags and the coefficient magnitudes and signs differ across dependent variables, indicating that regulation has compositional effects, altering not only the level of aggregate output but also the way in which it is produced. Table 4 collects the effects of regulation in summary form for easy reference. We now examine these effects in more detail.

3.5.A.1: Trend. As we have seen above, regulation can have two kinds of effects on a dependent

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[8]We 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.

variable's trend: a permanent shift in the trend (the trend-intercept effect) and breaks in trend (the trend-linear and trend-quadratic effects). Our results indicate that regulation has both kinds of effects on all variables except TFP, which does not show any trend-intercept effect. For simplicity, we will concentrate discussion on the results for output and note differences with other dependent variables when they are significant.

The trend-intercept effect is the product of regulation's trend $\$_R$ and the sum of the T_j^R coefficients. The latter are reported toward the bottom of Part I of Table 2; $\$_R$ is obtained by estimating the equation

$$r_t = \alpha_R + \beta_R t + v_t$$

The estimated value of $\$_R$ is reported in Table 3, along with analogous estimates for *G* and *T*. For output, the sum of the T_j coefficients is -0.151, and $\$_R$ is 0.0339. Their product is -0.0051 (see the last line of Table 2, Part I), indicating that regulation shifts the trend in output down by almost exactly half a percentage point. 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 linear coefficient is positive, and the quadratic coefficient is negative, causing output's trend initially to increase but then eventually to fall as regulation grows.

We can determine the effect of post-1949 regulation on the dependent macro variables' trends easily enough by using the parameter estimates and the regulation data to evaluate the trend-intercept, trend-linear, and trend-quadratic terms and then adding them together. That is, for *Y* we compute the series $\$_R E T^R + (_0^R (R_t - R_{1949}) + *_0^R (R_t - R_{1949})^2$, which is the change in output's trend caused by regulations added since 1949. The result is shown in Figure 4. The trend-intercept effect of -0.005 means that the entire path shown in Figure 4 is shifted down half a percentage point compared to what it would have been in the absence of new regulation. The trend grows from 1949 to 1978, reflecting the dominance of the trend-linear term for low values of *R*. The trend then falls from 1978 to 1999, reflecting the dominance of the trend-quadratic term for high values of *R*. Almost the entire path is in the negative region, indicating that regulation reduced output's trend over most of the sample.

The same method can be applied to the other dependent variables to determine regulation's effect on their trends. Figures 5-8 show the results. The differences among them and also between them and Figure 4 are striking. Regulation over the past fifty years not only has reduced the trend in output but also has reduced the growth rate of TFP and raised the growth rates of capital and labor. Apparently, regulation has reduced the growth in economic efficiency (TFP) and induced a trend toward substitution of physical inputs for mental inputs as a partial offset.

Regulation also has reduced the trend in investment. The level of investment is closely related to the trend in physical capital (capital's trend, or growth rate, is investment divided by capital), so the trend in investment is closely related to the *change* in the trend in capital. As we shall see momentarily, regulation has raised the *level* of investment through another channel, which is consistent with our finding that regulation has lifted the trend in capital. The decline in investment's trend, shown in Figure 8, corresponds to the decline in the *slope* of capital's trend, which explains the concavity of the time path of capital's trend in Figure 7.

3.5.A.2. Cycle (or Level) Effects. Regulation affects not only trend but also causes fluctuation about trend and one-time shifts in levels. These effects are captured by the $\mathbf{T}_j^R$ coefficients. For output, the first three $\mathbf{T}_j^R$ coefficients are statistically significant, indicating that current, once-lagged, and twice-lagged regulation affect fluctuations about trend. Note that statistical significance here is determined by minimization of the Schwarz-Bayes information criterion (SBC) and therefore is joint significance. Indeed, the second $\mathbf{T}_j^R$ coefficient is individually insignificant, but eliminating it raises the SBC. The individual $\mathbf{T}_j^R$ coefficients are of mixed sign, but their sum is negative, indicating that a permanent change in the level of regulation has a net negative effect on the level of output. In graphical terms, the time path of output ultimately is shifted downward by an increase in regulation, independently of any effect on trend. The standard error on the sum of the $\mathbf{T}_j^R$ is a bit large, leading to some imprecision of the estimated sum, but the point estimate indicates a non-trivial elasticity of output's level with respect to regulation of about -15 percent.

That regulation affects output with a distributed lag means that regulation has dynamic effects on output. An interesting aspect of the dynamic path is that output's response to regulation changes sign over time. Initially, output responds positively to regulation ($\mathbf{T}_0^R > 0$) but later reverses direction ($\mathbf{T}_1^R$, $\mathbf{T}_2^R < 0$) and ultimately responds negatively ($\mathbf{ET}_j^R < 0$).

The response of the other variables' levels is mostly similar to that of output. The notable exception is the net *positive* response of investment to regulation; the sum of the $\mathbf{T}_j^R$ is positive for *I*. Figure 9 shows the cycle effect on investment of regulations added since 1949. The large upward jump in the early 1970s corresponds to the introduction of extensive environmental and occupational health and safety regulation, both of which required large investments by firms.

We see, then, that regulation has permanent effects on the various dependent macro variables through alterations in trend and shifts in levels, and it also has transitory cyclical effects. In addition, the differing coefficient magnitudes, coefficient signs, and lag lengths of the responses of *TFP*, *N*, and *K* to regulation indicate that regulation has allocative effects. The economy responds to regulation not only by changing the amount of output produced but also by changing the way it produces that output.

3.5.A.3. The Productivity Slowdown of the 1970s. Our results suggest a straightforward explanation for the productivity slowdown of the 1970s and 1980s: regulation and taxes. Figures 10 and 11 show the level and growth rate of TFP over the sample period. The solid lines depict the raw data; the dashed lines are the Hodrick-Prescott (HP) filtered series. It is clear from the two figures that TFP growth starts falling sometime in the mid to late 1960s, stops falling around 1981, and perhaps regains some of its former vigor after that. Figure 1 shows that regulation's growth increased dramatically around 1971 (reflecting the new environmental and occupational safety regulations), and Figure 3 shows that the marginal tax rate's growth rate abruptly increased a few years earlier, around 1965. Furthermore, tax rates stopped rising around 1980 and fell somewhat afterward, and regulation stabilized and perhaps even fell over the 1990s. The graphs show that major changes in the paths of regulation and taxes correspond the major changes in the path of TFP.

We can do better than an impressionistic visual analysis by using our parameter estimates to calculate the effect on TFP of regulations added since 1949. For TFP, the estimated version of equation (6) is

$$(8) \qquad X_t = \left(e^{[0.026 + (4.72E-08)\cdot R_t - 1.52E-07)\cdot R_{t-1}]\,t} \right) \left(e^{-0.275}\, T_t^{-0.15}\, U_t \right).$$

What we want to calculate are the effects of changes in regulations and tax rates after 1949. We therefore rewrite (8) as

(9)

$$X_t = \left(e^{[0.026 + (4.72E-08)\cdot(R_t - R_{1949} + R_{1949}) - 1.52E-07)\cdot(R_{t-1} - R_{1949} - R_{1949})]\,t} \right) \left(e^{-0.275} \left[\frac{T_t}{T_{1949}} T_{1949} \right]^{-0.15} U_t \right)$$

$$= \left(e^{[(4.72E-08)\cdot(R_t - R_{1949}) - 1.52E-07)\cdot(R_{t-1} - R_{1949})]\,t} \right) \left[\frac{T_t}{T_{1949}} \right]^{-0.15} W_t$$

where W_t collects all elements of X_t not due to post-1949 regulations or marginal tax rates. Figures 12 plots the time path of the first term in (9), which is the effect of post-1949 regulation on TFP. That term gradually falls from 1949 to 1973 and then turns sharply downward, plummeting from 0.93 in 1973 to 0.62 in 1995, after which it nearly stabilizes. Figure 13 plots the time path of the second term in (9), which is the effect of post-1949 changes in the marginal tax rate. Judging from the HP filtered series, the tax effect takes an abrupt turn downward in about 1965 and bottoms out in 1982, after which it partially reverses. The two effects together have an enormous impact on TFP. In 1982, the regulatory effect has a value of 0.79, meaning that post-1949 regulation had reduced TFP to 79 percent of what it would have been if those additional regulations had not been enacted. In that same year, the marginal tax rate effect has a value of 0.92, meaning that TFP was 92 percent of what it would have been had marginal tax rates remained at their 1949 level. By 1995, the marginal tax rate effect had risen slightly to 0.94, but the regulation effect had fallen still farther to 0.62. The small upward kinks in the HP filtered series for Dlog*T* in 1982 and 1991 suggest that the reduction in marginal tax rates in the early 1980s and the stabilization of regulation in the early 1990s allowed TFP growth to resume somewhat.

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; in contrast, our story is quite straightforward: heavy regulation and high marginal tax rates 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; both could be operative. The data, however, almost certainly imply that Greenwood and Yorukoglu's theory cannot be the complete explanation. They 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; recall Figures 10 and 11. 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.

3.5.A.4. Opportunity Cost of Regulation. We can calculate a measure of the output cost of regulation added since 1949.[9] We compute what output's path would have been if regulation had stayed at its 1949 level by evaluating (6) with estimated parameter values and holding regulation fixed at its 1949 value of 19335 (that is, with $R_t = 19335$ for all t). We then take the ratio of that hypothetical path to the actual path of output. The result is shown in Figure 14. Values of the ratio larger than 1 indicate years in which output would have been higher with the 1949 level of regulation than with the level that actually prevailed in those years. The ratio usually is above 1 and ends at a value of 1.099, meaning that in the last year of the sample (1999), real output

[9]We cannot calculate the cost of total federal regulation because our data do not include an observation when federal regulation did not exist. Given the highly non-linear relation between regulation and output, we are unwilling to extrapolate back to estimate the cost of all federal regulation. Initial levels of federal regulation may have increased rather than reduced output by making the economy more efficient, protecting property rights, and so on.

would have been almost 10 percent higher if regulation had remained at the 1949 level. This difference is large in dollar terms. The current GDP of the US is about $11 trillion, so if the same 10 percent figure applies today, the annual output foregone because of post-1949 regulation is about $1.1 trillion. That foregone output represents an opportunity cost of regulation. We can express it somewhat differently by noting that there are about 100 million households in the US, so the additional regulation costs each household about $11,000 each year. Another way to put it, perhaps a little too dramatically, is that each page of federal regulation today costs the economy about $9.2 million in foregone output. Our estimates indicate that the opportunity cost will grow at a rate of about 0.2 percent a year (the amount by which output's trend was reduced in 1999), *if* regulation is merely kept at its 1999 level and not increased further. Two-tenths of a percent is an appreciable magnitude for a growth rate.

At its current size, the output opportunity cost of regulation is about the same size as the direct compliance costs of regulation. 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 $900 billion in 2003.[10] The output opportunity cost has not been previously considered in measures of regulation's costs. Including it more than doubles regulation's total cost.

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

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[10]This 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.

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, we make no attempt to measure the non-production benefits of regulation and so offer no judgement on whether regulation is a net social benefit.

<u>*3.5.B. Difference-Stationary Model.*</u> The difference-stationary model is similar to the trend-break model in that the exogenous policy variables affect the drift as well as the level of the dependent variable:

$$X_t/X_{t-1} = \exp[\beta + \sum_{i=0}^{J_1} \gamma_i Z_{t-i} + \sum_{j-0}^{J_2} \delta_j Z_{t-j}^2] \cdot \prod_{j=1}^{J_3} \eta_j (X_{t-j}/X_{t-j-1}) \prod_{j=0}^{J_4} \omega_j (Z_{t-j}/Z_{t-j-1}) \prod_{j=0}^{J_5} \theta_j e_{t-j} \tag{10}$$

The argument of the exponential function is the drift. Taking logs gives the form of the equation to be estimated:

$$\Delta x_t = \beta + \sum_{j=1}^{J_1} \gamma_i Z_{t-i} + \sum_{j=0}^{J_2} \delta_j Z_{t-j}^2 + \sum_{j=1}^{J_3} \eta_j \Delta \ln x_{t-j} + \sum_{j=1}^{J_4} \omega_j \Delta \ln z_{t-j} + \sum_{j=1}^{J_5} \theta_j e_{t-j}. \tag{11}$$

The policy variables *Z* enter in delta-log form in the standard way and also in level form through the drift term. As in the trend-break model, *Z* is the vector (*R*, *G*, *T*) and the coefficients **(**, *****, and **T** also are vectors. Lag lengths were chosen by the same method as for the trend-break regressions, by minimizing the Schwarz information criterion.

Table 5 summarizes the results for output; the results for the other dependent variables are similar. The coefficient patterns lead to the same conclusion as those in the trend-break model: regulation affects **)***y* through both the drift and the cycle components. The main substantive difference between the difference-stationary and trend-break models is that the marginal tax rate affects the drift rate in the difference-stationary model, whereas it did not affect the trend in the trend-break model. Unfortunately, two aspects of the difference-stationary results seriously limit the usefulness of the model. First, the number of jointly significant terms is very large, making the estimated model unwieldy and also greatly reducing the degrees of freedom. Second, the individual parameter estimates are virtually all very imprecise (at least in part because of the reduced the degrees of freedom). Only one parameter out of thirty-eight has a p-value less than 0.2 (and even that is barely less at 0.19), and only twelve of the thirty-eight have p-values less than 0.5. Imprecision of such magnitude makes it impossible to perform useful tests of whether subsets of the coefficients sum to zero; the standard error on the sum is always so large that zero is included in any reasonable confidence interval. Consequently, we cannot say much about whether regulation has long-run effects on either the drift in y or in the level of)*y*. The same is true of government purchases and the marginal tax rate. For the same reason, we cannot construct useful counterfactual paths to estimate the magnitude of regulation's effect on the path of output. We therefore do not pursue the difference-stationary model further.

3.5. Disaggregagted Regulation

There is an interesting pattern in Figures 4-7. Around 1972, the time paths of the trends in *Y*, *TFP*, *N*, and *K* all show a sharp change in slope. The 1970s were a time of major innovation in environmental, health, and occupational safety regulations, which were different in character from the regulations in existence up to that time. Perhaps the new kinds of regulations also had new kinds of impacts that explain the patterns in the time trends. To explore that possibility, we turn to an investigation of the effects of the individual titles of the CFR.

Our goal was to isolate the CFR titles having significant impacts on our aggregate dependent variables.[11] To do so, we regressed the five dependent variables on the set of page counts for the individual titles instead of on the total 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. We thus ran simplified regressions of the forms

$$x_t = \alpha + [\beta + \sum_{w=7}^{50} \gamma_w r_w] t + u_t$$

and

$$x_t = \alpha + \beta t + \sum_{w=7}^{50} \omega_w r_w + u_t$$

for the trend-break case and

$$\Delta x_t = \beta + \sum_{w=7}^{50} \gamma_w R_w + e_t$$

and

$$\Delta x_t = \beta + \sum_{w=7}^{50} \omega_w \Delta r_w + e_t$$

for the difference-stationary case, where w indexes the CFR titles.

In the trend-break case, for each 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

[11]Note 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. Constructing such a measure requires some means of sorting regulations, a daunting task.

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.

In the difference-stationary regressions, for each dependent variable almost none (for capital, literally none) of the titles was individually significant, and the null hypothesis that the full set of titles were jointly insignificant never could be rejected. These results contradict those obtained for the aggregate measure of regulation, where total regulation had statistically significant effects on all five macro variables.

The results are uninformative. The trend-stationary regressions imply that individual titles are significant but cannot identify which titles those are. The difference-stationary regressions imply that no titles are significant, contrary to the results obtained earlier for regulation taken as a whole. This general absence of information almost certainly is due to the small number of degrees of freedom when all titles are included individually. Low degrees of freedom directly causes large standard errors of estimated coefficients, which in itself is sufficient to prevent extraction of useful information about which titles are important; in addition, low degrees of freedom also prevents inclusion of lagged regulation, which our previous analysis found to be highly significant and whose exclusion therefore introduces biases into the estimation. We thus abandoned our inquiry into the effects of individual CFR titles.

4. Conclusion

We have presented a new time series measuring the extent of federal regulation in the

United States. We find that 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 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. Most striking, perhaps, is the uniformly negative impact on TFP, which captures technical progress. Our results suggest that federal regulation, together with changes in marginal tax rates, may explain the famous and famously puzzling TFP slowdown of the 1970s.

Regulation’s overall effect on output is negative and substantial. In the trend-break model, federal regulations added over the past fifty years have reduced real output by about 10 percent compared to what it would have been if regulation had remained at its 1949 level. As a result, annual output now ($11 trillion) is about nine-tenths of what it would have been ($ 12.1 trillion) if regulation had stayed at its 1949 level, implying an annual cost in 2003 of $1.1 trillion in foregone output. This opportunity cost is a dimension of regulatory cost not previously measured. Our estimate is roughly the same magnitude as standard estimates of the direct costs of federal regulation and so doubles the estimated cost of federal regulation.   Furthermore, the cost continues to grow at a rate of about two-tenths of a percent per year.

Regulation is a type of government activity heretofore omitted from macroeconomic analysis. Our findings suggest that omission should cease, both for the sake of better understanding macro dynamics and for the sake of better estimating regulation's net benefit.

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Table 1
Bivariate Granger-Causality Tests
1949-1999

Null Hypothesis	q	N	F	p-value
$\Delta r \nrightarrow \Delta y$	4	46	2.43	0.0652
$\Delta y \nrightarrow \Delta r$	4	46	0.30	0.8763
$\Delta r \nrightarrow \Delta tfp$	4	46	2.66	0.0480
$\Delta tfp \nrightarrow \Delta r$	4	46	0.72	0.5840
$\Delta r \nrightarrow \Delta n$	4	46	0.97	0.4372
$\Delta n \nrightarrow \Delta r$	4	46	1.96	0.1206
$\Delta r \nrightarrow \Delta k$	4	46	1.45	0.2364
$\Delta k \nrightarrow \Delta r$	4	46	3.23	0.0226
$\Delta r \nrightarrow \Delta k$	6	44	0.35	0.9031
$\Delta k \nrightarrow \Delta r$	6	44	1.57	0.1888
$\Delta r \nrightarrow \Delta i$	4	46	3.19	0.0240
$\Delta i \nrightarrow \Delta r$	4	46	0.78	0.5481

Notes: The variables are: r (regulation), y (output), tfp (total factor productivity), k (physical capital), n (labor), i (private investment), 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 2: Part I
Regulation Parameter Estimates
Trend-Break Models, 1949-1995

$$x_t \;=\; \alpha + [\beta + \sum_{j=0}^{J_1^R} \gamma_j R_{t-J} + \sum_{j=0}^{J_2^R} \delta_j R_{t-j}^2] t + \sum_{j=0}^{J_3^R} \omega_j^R r_{t-j} + \sum_{j=0}^{J_3^T} \omega_j^T \tau_{t-j} + \sum_{j=0}^{J_3^G} \omega_j^G g_{t-j} + u_t$$

	x = y	x = tfp	x = n	x = k	x = i
($_0$	2.80E-07 (9.18E-08) [0.000]	4.72E-08 (4.46E-08) [0.296]	2.35E-07 (1.30E-07) [0.079]	-3.72E-07 (2.26E-07) [0.128]	-1.22E-07 3.63E-08) [0.002]
($_1$	-	-1.52E-07 (4.34E-08) [0.001]	1.56E-07 (7.69E-08) [0.051]	3.87E-07 (4.42E-07) [0.400]	-
($_2$	-	-	-	2.66E-07 2.97E-07) [0.391]	-
($_3$	-	-	-	6.59E-08 (3.70E-08) [0.103]	-
($_4$	-	-	-	1.61E-07 (4.30E-08) [0.003]	-
*$_0$	-1.81E-12 (5.08E-13) [0.001]	-	-1.77E-12 (3.95E-13) [0.000]	7.87E-13 (1.10E-12) [0.490]	-
*$_1$	-	-	-	-2.63 (2.21E-12) [0.258]	-
*$_2$	-	-	-	-1.01E-12 (1.49E-12) [0.514]	-
T$_0^R$	0.281 (0.107) [0.013]	-	0.235 (0.136) [0.093]	0.324 (0.074) [0.001]	1.227 (0.259) [0.000]
T$_1^R$	-0.098 (0.178) [0.586]	-	-0.329 (0.095) [0.002]	0.076 (0.184) [0.687]	-1.124 (0.328) [0.001]
T$_2^R$	-0.334 (0.096) [0.001]	-	-	-0.139 (0.155) [0.389]	-
T$_3^R$	-	-	-	-0.047 (0.105) [0.667]	-

$\mathbf{T}_4^R$	-	-	-	-0.280 (0.114) [0.033]	-
$\mathbf{T}_5^R$	-	-	-	-0.225 (0.057) [0.002]	-
$\mathbf{T}_6^R$	-	-	-	0.107 (0.049) [0.054]	-
$\mathbf{E(}_i$ {$\mathbf{P}^2$ test: $\mathbf{E(}_i = 0$} [p-value]	2.80E-07 {-} [-]	-1.04E-07 {211.425} [0.000]	3.910E-07 {28.625} [0.000]	5.08E-07 {15.658} [0.000]	-1.22E-07 {-} [-]
$\mathbf{E*}_i$ {$\mathbf{P}^2$ test: $\mathbf{E*}_i = 0$} [p-value]	-1.81E-12 {-} [-]	-	-1.77E-12 {-} [-]	-2.85E-12 {13.998} [0.000]	-
$\mathbf{ET}_i^R$ {$\mathbf{P}^2$ test: $\mathbf{ET}_i = 0$} [p-value]	-0.151 {2.343} [0.126]	-	-0.094 {1.643} [0.200]	-0.184 {8.976} [0.003]	0.104 {0.170} [0.680]
Trend-intercept effect of regulation: $\mathbf{\$}_R\ \mathbf{ET}_i^R$	-0.0051	-	-0.0014	-0.0071	-0.0014

Note: Numbers in parentheses (.) are Newey-West corrected standard errors; numbers in brackets [.] are p-values; numbers in braces {.} are $\mathbf{P}^2$ values.

Table 2: Part II
Other Parameter Estimates and Fit Statistics
Trend-Break Models, 1949-1995

$$x_t \;=\; \alpha + [\beta + \sum_{j=0}^{J_1^R} \gamma_j R_{t-J} + \sum_{j=0}^{J_2^R} \delta_j R_{t-j}^2] t + \sum_{j=0}^{J_3^R} \omega_j^R r_{t-j} + \sum_{j=0}^{J_3^T} \omega_j^T \tau_{t-j} + \sum_{j=0}^{J_3^G} \omega_j^G g_{t-j} + u_t$$

	x = y	x = tfp	x = n	x = k	x = i
"	6.376 (1.170) [0.000]	-0.275 (0.098) [0.008]	6.271 (0.879) [0.000]	6.626 (0.715) [0.000]	3.094 (2.984) [0.306]
$	0.027 (0.004) [0.000]	0.026 (0.001) [0.000]	-0.010 (0.003) [0.003]	0.023 (0.006) [0.002]	0.054 (0.006) [0.000]
T_0^G	0.264 (0.057) [0.000]	-	0.0137 (0.053) [0.015]	-0.062 (0.135) [0.655]	-
T_1^G	-	-	0.134 (0.059) [0.030]	-0.101 (0.096) [0.315]	-
T_2^G	-	-	-	0.272 (0.109) [0.029]	-
T_3^G	-	-	-	0.017 (0.080) [0.831]	-
T_4^G	-	-	-	-0.077 (0.075) [0.325]	-
T_5^G	-	-	-	0.124 (0.058) [0.055]	-
T_6^G	-	-	-	-0.0001 (6.25E-05) [0.055]	-
T_0^T	-0.261 (0.085) [0.004]	-0.150 (0.046) [0.002]	-0.015 (0.071) [0.831]	0.201 (0.056) [0.004]	-0.459 (0.270) [0.098]
T_1^T	-	-	-0.254 (0.061) [0.000]	-0.113 (0.063) [0.102]	-
T_2^T	-	-	-	-0.022 (0.042) [0.612]	-

$\mathbf{T}_3^T$	-	-	-	0.013 (0.073) [0.864]	-
$\mathbf{T}_4^T$	-	-	-	0.030 (0.037) [0.427]	-
$\mathbf{T}_5^T$	-	-	-	-0.061 (0.067) [0.379]	-
$\mathbf{ET}_i^G$ {$\mathbf{P}^2$ test: $\mathbf{ET}_i^G = 0$} [p-value]	0.264 {-} [-]	-	0.271 {48.974} [0.000]	0.173 {3.666} [0.056]	-
Trend-intercept effect of government purchases: $\mathbf{\$}_G\mathbf{ET}_i^G$	0.0064	-	0.0065	0.0042	-
$\mathbf{ET}_i^T$ {$\mathbf{P}^2$ test: $\mathbf{ET}_i^T = 0$} [p-value]	-0.261 {-} [-]	-0.150 {-} [-]	-0.269 {13.951} [0.000]	0.049 {0.045} [0.831]	-0.459 {-} [-]
Trend-intercept effect of marginal tax rate: $\mathbf{\$}_T\mathbf{ET}_i^T$	-0.0019	-0.0011	-0.0020	0.0003	-0.0033
Adj. R^2	0.998	0.992	0.992	1.000	0.977

Note: Numbers in parentheses (.) are Newey-West corrected standard errors; numbers in brackets [.] are p-values; numbers in braces {.} are $\mathbf{P}^2$ values.

Table 3
Trends in Exogenous Variables
$$Z_t = Z_0 e^{\beta_Z t} V_t$$

Variable	Z	β_Z (Standard error)
Regulation	R	0.0339 (0.0016)
Government purchases	G	0.0241 (0.0021)
Marginal tax rate	T	0.0073 (0.0013)

Note: Numbers in parentheses (.) are Newey-west corrected standard errors.

Table 4
Trend-Break Models, Summary of Regulation Effects

Variable	Regulation effects			
	Trend-intercept	Trend-linear	Trend-quadratic	Cycle
Y	Yes, < 0	Current, > 0	Current, > 0	Current and 2 lags, sum < 0 but not significantly so
TFP	No	Current and 1 lag, sum < 0	No	No
N	Yes, < 0	Current and 1 lag, > 0	Current, < 0	Current and 1 lag, sum < 0 but not significantly so
K	Yes, < 0	Current and 4 lags, sum > 0	Current and 2 lags, sum < 0	Current and 6 lags, sum < 0
I	Yes, < 0	Current, <0	No	Current and 1 lag, sum > 0 but not significantly so

Table 5

Summary of Difference-Stationary Model for Output, 1949-1995

$$\Delta y_t = \beta + \sum_{j=1}^{J_1} \gamma_i Z_{t-i} + \sum_{j=0}^{J_2} \delta_j Z_{t-j}^2 + \sum_{j=1}^{J_3} \eta_j \Delta \ln y_{t-j} + \sum_{j=1}^{J_4} \omega_j \Delta \ln z_{t-j} + \sum_{j=1}^{J_5} \theta_j e_{t-j}$$

Variable	Coefficient	Jointly significant terms	Terms with p-value < 0.2	Terms with p-value < 0.5
Drift function elements:				
Constant	$	One	0	0
Regulation (R)	$(^{R}$	Current & 7 lags	0	5
Regulation^2	$*^{R}$	Current & 4 lags	1	2
Govt Purchases (G)	$(^{G}$	None	0	0
Govt Purchases^2	$*^{G}$	None	0	0
Marginal tax rate (T)	$(^{T}$	Current & 3 lags	0	0
Marginal tax rate^2	$*^{T}$	Current & 3 lags	0	0
Non-drift function elements:				
)y	0	3 lags	0	2
)r	T^{R}	Current & 3 lags	0	1
)g	T^{G}	Current & 2 lags	0	0
)J	T^{T}	Current & 4 lags	0	2
e	2	1 lag	0	0
Adj. R^2	0.996			

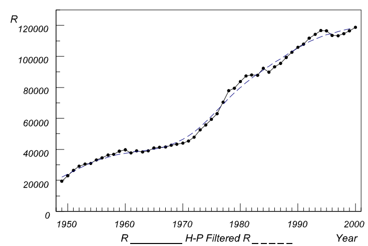

Fig. 1: Regulation over time

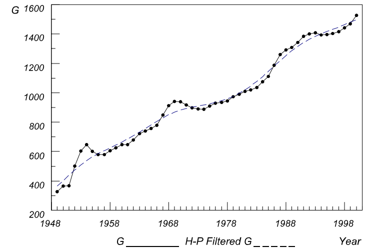

Fig. 2: Government purchases over time

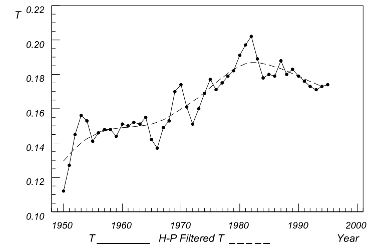

Fig. 3: Marginal tax rate over time

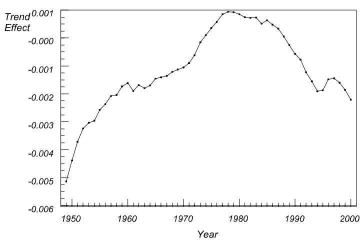

Fig. 4: Effect of R on Y trend

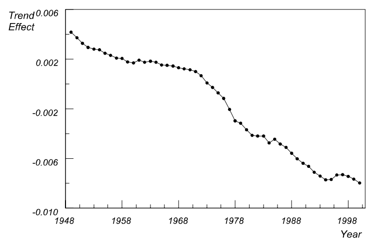

Fig. 5: Effect of R on TFP trend

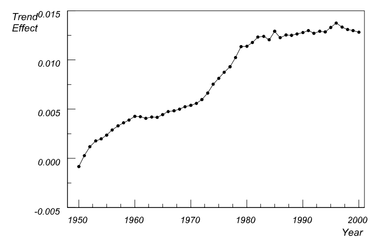

Fig. 6: Effect of R on N trend

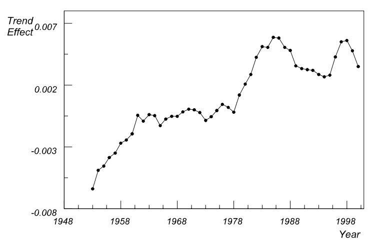

Fig. 7: Effect of R on K trend

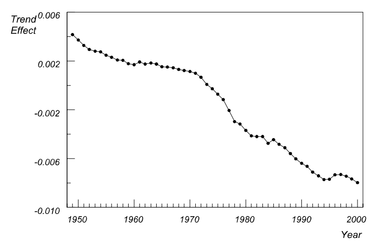

Fig. 8: Effect of R on I trend

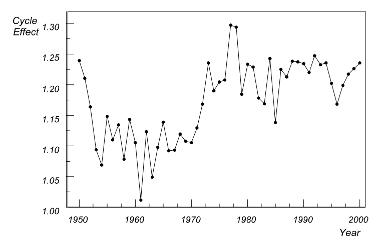

Fig. 9: Cycle effect of R on I

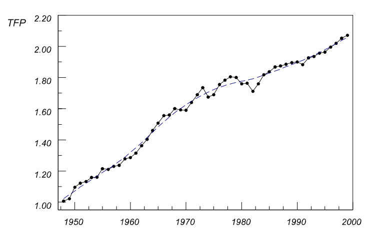

Fig. 10: TFP

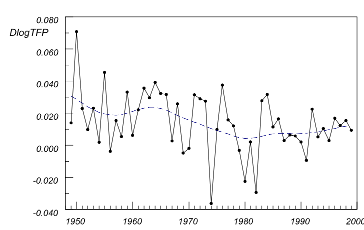

Fig. 11: DlogTFP

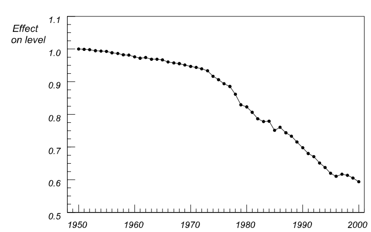

Fig. 12: Effect of R on TFP

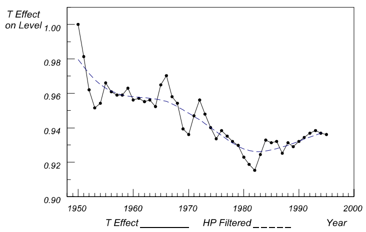

Fig. 13: Effect of T on TFP

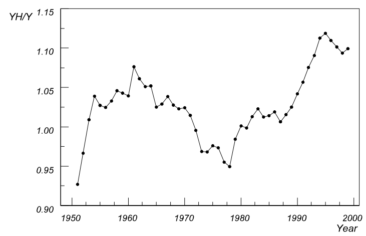

Fig. 14: Ratio of hypothetical Y to actual Y

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."[12] 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.[13]

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.[14] 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.[15]

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

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[12]Throughout 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*.

[13]The *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.

[14]No supplement was published for 1942.

[15]Due 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.[16] 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.[17]

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.[18]

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 (2000) 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 (2000).

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.[19] 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

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[16]The 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.

[17]On 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.

[18]Beginning 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.

[19]Recall 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.[20] 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.

[20]Dawson (2000) 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.