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# Federal regulation and aggregate economic growth

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**Abstract** We introduce a new time series 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 offer a straightforward explanation for the productivity slowdown of the 1970s. Qualitatively and quantitatively, our results agree with those obtained from cross-section and panel measures of regulation using cross-country data.

**Keywords** Regulation · Macroeconomic performance · Economic growth · Productivity slowdown

**JEL classification** E20 · L50 · O40

## 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

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received little attention in the macroeconomic literature: regulation. Although microeconomists have analyzed both the causes and effects of regulation for decades, macroeconomists have joined the discussion only much more recently, with a number of empirical studies suggesting that regulation has 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 et al. (2000, 2001), Bassanini and Ernst (2002), Djankov et al. (2002, 2006), Nicoletti and Scarpetta (2003), and Loayza et al. (2004, 2005) for cross-section studies and Bandiera et al. (2000), Alesina et al. (2003) and Kaufman et al. (2003) for panel studies. Almost all these studies conclude that regulation 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 time path of the aggregate economy: (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 (Nicoletti et al. 2000). The time span of the data ranges from none at all (the cross-section data sets) to a maximum of 20 years (the panel data sets of Nicoletti et al. 2001, and Kaufman et al. 2003). Restricting attention to a subset of regulations is problematic because, as we document below, the included regulations often are highly correlated both contemporaneously and intertemporally with the omitted regulations, leading to omitted variables bias in any regression analysis. A short time dimension makes analysis of dynamics difficult or impossible.

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 includes literally all federal regulations over a period of 57 years. 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. The major effect is on the growth rate of output. We find that regulation added since 1949 has reduced the aggregate growth rate on average by about 2 percentage points 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 annual output by 2005 is about 28 % of what 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 explain much of the productivity 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. In particular, our estimated growth rate reduction of about 2 percentage points is in line with results obtained from the cross-section and panel studies.

## 2 Measuring federal regulation

Any attempt to construct a measure of regulation will be limited by difficulties that arise from the nature of regulation itself. We explain those difficulties in the first part of this section. We then present our new measure and compare it to previous measures.

### 2.1 Measurement issues

When we study the effects of taxes on economic activity, we can appeal to economic theory to tell us which taxes to consider and how those taxes should enter our theoretical or empirical model. For example, theory tells us that one of the ways the income tax affects investment decisions is through the change in the rate of return to investment brought about by the marginal tax rate—that is, it is the marginal tax rate that matters and the channel is through the rate of return. Thus, theory tells us what to measure (the marginal tax rate rather than the average tax rate) and where to put it (in the rate of return). Similarly, when we study the effects of government expenditure on the path of gross domestic product, theory tells us to use the amount of purchases and to decompose it into transitory and permanent parts (Barro 1981). Again, we are told what to measure (purchases rather than, say, total expenditure) and how to enter it into the model (decomposed into permanent and transitory parts). Unfortunately, regulation is more difficult to handle. How should one measure the amount of regulation contained in the prohibition "Thou shalt not pollute," and how should it enter a macroeconomic model? Modern growth theory actually does give us some guide to how to address the latter modeling issue, but it does not tell us exactly what to measure. There is no "marginal regulation rate" in either the theory or any available data. There also is no market in which regulations are traded, so there is no market price indicating their value. We thus unavoidably are limited to some kind of counting measure of the volume of regulation. A counting measure obviously is imperfect in that two identical values may comprise regulations of different types and, even within a given type, may represent regulations of different stringency. However, if there are many kinds of regulation (as there are in all countries included in regulation studies), it is reasonable to expect an index to provide a useful overall measure of regulation. In that regard, measures of regulation are no different from many other variables. Government purchases, physical capital, schooling, labor, and so on are composites whose effects may vary with the composition even though two reported values may be the same.

There is an indirect indicator that counting measures of regulation contain useful information, which is the results that arise from including them in regressions. If the measures contained no information, they should not be systematically related to dependent variables of interest. In fact, a raft of studies discussed below find that regulations routinely have coefficients that are both statistically significant and of the sign predicted by theory. For example, regulations that make it costly to start a business should be negatively associated with investment. That is exactly what has been found (Alesina et al. 2003).

The measures of regulation mentioned in the Introduction generally proceed by constructing indices based on binary indicators of whether or not various kinds of regulation exist, assigning a value of 1 to each type of regulation that exists and a 0 to those that do not exist. The index then is constructed as a weighted sum of all the binary indicators. Such measures capture the existence of given types of regulation but cannot capture their extent or complexity. Some measures therefore add indicators of the extent or effectiveness of regulation (see, for example, the detailed Annex in Nicoletti et al. 2000). The measure we propose is an alternative counting measure that is not binary and that probably captures at least some of regulation's complexity. Specifically, 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. It thus is much more comprehensive and covers a much longer time span than previous measures of regulation. Because all federal regulations must be published in the CFR, our page count measure must have at least a rough correlation with the "true" amount of regulation that should enter an economic model. If the CFR page count were zero, there would be no regulation, and it surely is reasonable to suppose that the more pages there are in the CFR, the more regulations there are. It also seems reasonable to suppose that the number of pages is positively related to the complexity of regulation because, at least on average, more complex regulations should require more pages to describe. In that case, our measure captures more than just the existence of a regulation.

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

### 2.2 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 second 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.

### 2.3 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 2005, 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 19,335 pages in 1949 to 134,261 in 2005. Regulation grows most of the time, but its growth rate varies a great deal. The growth rate has a mean of 0.035 and a standard deviation of 0.039. 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

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

[2] See the Appendix for details on the method of construction. Note in particular that we have accounted for changes in typeface and page sizes.

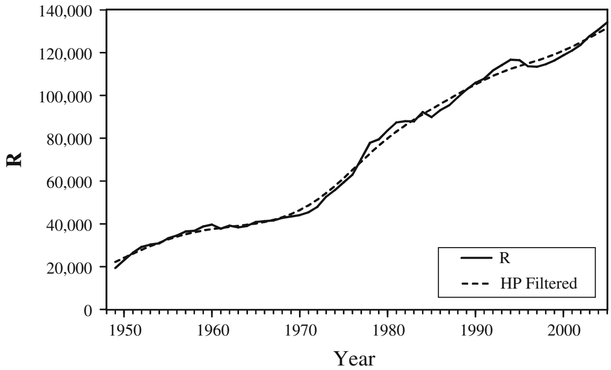


**Fig. 1** Regulation over time

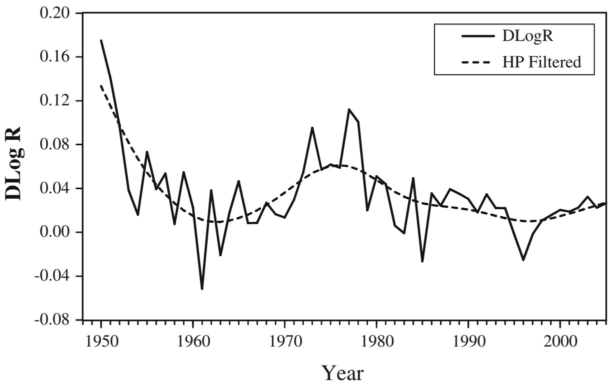


**Fig. 2** Growth rate of regulation over time

regulation in the 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) 1949–1960 (fast growth), (2) 1960–1972 (slow growth), (3) 1972–1981 (fast growth), (4) 1981–1985 (slow growth), (5) 1985–1993 (fast growth), and (6) 1993–2005 (slow growth). As we will see, these segments correspond to behavior of the aggregate variables of interest.

## 2.4 Comparison with other measures of regulation

We present a brief comparison with earlier measures of regulation, restricting attention here to the two most important differences: the time span of the data and the comprehensiveness of the regulations included. A complete discussion of all the differences between our measure and its predecessors is in the Appendix.[3]

[3] The predecessors are Nicoletti et al. (2000, 2001), Bassanini and Ernst (2002), Djankov et al. (2002, 2006), Nicoletti and Scarpetta (2003), Loayza et al. (2004, 2005), Alesina et al. (2003) and Kaufman et al. (2003).

**Table 1** Basic statistics for CFR page count series

| | | |
|---|---|---|
| Number of pages | | |
| Starting Year, 1949 | 19,335 | |
| Ending Year, 2005 | 134,261 | |
| Growth rate of number of pages | | |
| Max | 0.175 | In 1950 |
| Min | −0.052 | In 1961 |
| Mean | 0.035 | |
| Median | 0.027 | |
| SD | 0.039 | |
| 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 | |
| SD | 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 | |
| SD | 0.219 | |

Our measure spans 57 years. The earlier measures have short to non-existent time spans, the longest being 20 years and the shortest 0 years (i.e., observations are available only for a single year). The earlier measures cannot be used to study regulation's effects on 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, but even there one must proceed with caution in light of Ventura's (1997) demonstration that the interpretation of cross-country growth regressions is confounded by the effects of international trade. Long-run growth and dynamic adjustment are intertemporal phenomena, best studied with time-series data. Our measure is naturally suited to studying them. The earlier measures, with their strong cross-section element but weak intertemporal element, are better suited for cross-sectional issues.

Our measure includes literally every regulation issued by the federal government, which makes it far more comprehensive than any of its predecessors. 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 original OECD cross-section data are restricted to product market and employment protection regulation. Other types of regulation, 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. Furthermore, within this restricted set of industries, only a few types of regulations are included, varying by industry: barriers to entry (available for all industries), public ownership (all industries except road freight), vertical integration (only gas, electricity and railways), market structure (only gas, telecommunications and railways), and price controls (only road freight). 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 contemporaneous correlations of the various titles of the CFR are often quite high.[4] Table 2 shows that the intertemporal cross-relations as measured by Granger causality also are quite high.[5] Such high correlations imply that including just one type of regulation in a statistical analysis is likely to be misleading because of multicollinearity and omitted variables bias. As a particular example, consider Nicoletti et al.'s (2001) measure, which the authors interpret as "a proxy for the overall regulatory policies followed by OECD countries over the sample period" (p. 43). Their measure spans 1978–1998 and shows a 66 % decline over that period. If we look at the page counts of CFR titles corresponding to the regulations included in Nicoletti et al.'s measure, we find that they behave similarly to Nicoletti et al's measure. For example, one of Nicoletti et al's regulation groups is air transport, railways, and road freight. In the CFR, those types of regulations are included in titles 23 (Highways), 46 (Shipping), and 49 (Transportation). The page count of titles 23, 46, and 49 behave qualitatively the same as Nicoletti et al.'s measure, dropping from a total of 8,400 in 1978 to 8,261 in 1998. Nevertheless, the page count of the total CFR displays the opposite behavior, rising 47 % over 1978–1998. The inescapable implication is that subsets of regulation are not reliable proxies for total regulation.

Another issue concerns the burden of regulation and the vigor of enforcement. Our measure controls for regulatory burden to some extent. The OECD data set measures regulatory burden by the presence or absence of a long list of regulatory requirements. It seems reasonable to suppose that the number of pages required to describe regulatory requirements varies directly with the number of requirements, at least on average. Our page count measure therefore should capture whatever regulatory burden is reflected in the number of regulatory requirements. In fact, 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

[4] Similarly, Loayza et al. (2005) found very high correlations among their seven indices of regulation.

[5] We use Granger causality rather than simple intertemporal cross-correlations because Granger causality controls for autocorrelation and so gives a stronger measure of genuine independent predictive power of one kind of regulation for another kind. Indeed, that is the whole point of Granger causality. Our goal here is to show that measures of regulation that are restricted to just a few types of regulation may lead to spurious inference because they may attribute to one kind of regulation effects arising from other unrelated types. Granger causality seems a better indicator of such a problem than simple correlation.

**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) |

particular provisions (a zero-one variable) but also their complexity (a continuous variable up to the inherent discreteness of numerical page counts), again on the reasonable assumption that more complex regulations require more pages of description at least on average. Another useful dimension of regulatory burden to measure would be the vigor with which regulations are enforced, but we were not able to find anything suitable. We considered using court cases or enforcement budgets, but we could find no useable data. Omitting vigor of enforcement is a problem only if enforcement vigor is correlated with the amount of regulation itself, but we see in the historical record no reason to expect such a correlation. For example, the amount of regulation fell during the Kennedy, Reagan, and Clinton administrations, but none of those administrations was considered to be lax in the enforcement of the regulations that remained. Moreover, regulatory enforcement is conducted by quasi-independent regulatory commissions, at least partly insulated from political pressures. We therefore proceed on the assumption that variation in enforcement vigor is orthogonal to variation in the amount of regulation.

Finally, we restrict attention to federal regulation only, ignoring regulation by the 50 states of the union. Inclusion of state regulation would be highly desirable, but data collection is an enormous task, far beyond our resources. The only way to obtain time series data on the volume of state regulation is to go to each state capital and search the state archives for old editions of state codes of regulation. With 50 capitals spanning distances of literally thousands of miles, we had no choice but to omit state regulations from our measure. Given the very strong economic effects of regulation that we discover and discuss below, collection of time series on state regulations would be a very valuable extension of our work.[6]

In summary, our page count measure has a much longer time span and much more comprehensive coverage than any other measure. It is well-suited to analyzing the effects of regulation on the dynamic behavior of the aggregate economy. It also has some limitations that could be reduced by further data collection.

## 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 also unnecessary for our purposes, so we present only the briefest of summaries.

Even at the micro level, regulation's effects on economic activity 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 quickly 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. Neither Pigou's nor Stigler's theories suggest any clear connection between aggregate variables and the amount of regulation. Even

[6] An issue that afflicts all existing measures of regulation is that some regulation comes into existence in response to innovation: changes in the production process or the invention of new products. Regulations of that type may merely keep the regulatory burden constant as the economy grows rather than increase it, so increases in either the page count of the CFR or the number of regulations would overstate to some extent the increase in regulatory burden over time. Endogenous growth theory suggests that the way to handle that issue is to measure regulation per product or per firm, but time series data on the number of products or number of firms do not exist for our sample period.

Pigou's completely benign view 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).[7] Neither Pigou's nor Stigler's theory suggests any reason to expect aggregate variables to cause changes in the amount of regulation. In contrast, McChesney's theory allows the possibility that politicians respond to the state of the aggregate economy by changing the amount of regulation. One might expect new regulations to appear in response to bad times, and indeed such behavior did occur in the 1930s with the Depression-era financial regulations. The Depression, however, was a unique event in American history, so one must be cautious in using Depression events as the basis of a general conclusion. In particular, it seems unlikely that a run-of-the-mill recession would spawn new regulation. Even the 2008 bailout of Fannie Mae and Freddie Mac seems to have been more an effort to save those two institutions than to respond to the aggregate economy's condition. Indeed, public attitudes have been becoming steadily more favorable toward increased regulation in general at least since 1995, according to opinion polls, with no apparent relation to the current state of the economy.[8] Still, McChesney's theory does suggest a possible reason for aggregate variables to cause changes in the amount of regulation. We test that possibility below.

## 3.2 Regulation and the macroeconomy

We are aware of no theory that addresses the effects that regulation has on the macroeconomy. However, recent work by Peretto (2007a,b,c, 2008) analyzing the effects of taxes on economic growth can be readily adapted to our needs. A detailed discussion of Peretto's approach is well beyond the scope of the present paper, so we give only a brief verbal summary and then explain the estimating equation that emerges. Readers interested in the details should consult Peretto's work.

Peretto's work uses a second-generation endogenous growth model that eliminates the counterfactual scale effects of first-generation models. The important result for our purposes is the solution for final goods (= GDP). The general form is $Y = A\,(.)\,e^{B(.)\mathrm{t}}C\,(.)$, where $A(.)$ is the intercept, $e^{B(.)}$ is the trend, and $C(.)$ is a transient (or "cycle"). The arguments of the functions $A$, $B$, and C are subsets of the model parameters and tax rates, with each function's particular subset depending on the details of the model. For example, for the case where tax rates are exogenous and government expenditure responds to changes in revenue to satisfy the government budget constraint, the solution has the following form (Peretto 2007a):

$$Y_t = A\,(t_L;\,\Omega_A;\,K_0)\,e^{B(t_\pi,t_D,t_V;\Omega_B)t}\,C\,(t_L,\,t_\pi,\,t_D,\,t_V;\,\Omega_C) \tag{1}$$

where $t_L$, $t_D$, $t_\pi$, and $t_V$ are the tax rates on labor income, dividends, profits, and capital gains, and $\Omega_A$, $\Omega_B$, and $\Omega_C$ are subsets of the model's parameters. The various taxes have different effects on the components of $Y$: $A$, $B$, and $C$. Peretto provides closed-form solutions for all

[7] Furthermore, growth theory suggests that anti-monopoly regulation, which would raise output in the short run by eliminating monopoly restrictions on supply, may reduce output in the long run by reducing the monopoly returns necessary to justify R&D and thereby reducing the rate of output growth.

[8] See the Wall Street Journal's front page article "Sour Economy Spurs Government to Grab a Bigger Oversight Role", 25 July 2008.

three of the functions $A$, $B$, and $C$, with the specific forms depending on the details of the model.[9] In all cases, the functions are irreducibly non-linear and mostly quite complicated.[10]

We learn two important things from Eq. (1): the overall form for output as a function of time and the way that taxes and model parameters affect output's time path. First, output is stationary about an exponential trend, not difference stationary. Changes in trend appear as breaks, not random shocks to a difference-stationary process. The consensus among macroeconometricians for some time has been that the best model of the non-stationarity of aggregate data is precisely this kind of log-linear trend with breaks (Perron 1989; Lumsdaine and Papell 1997; Murray and Nelson 2000).[11] Thus Eq. (1) dovetails nicely with current macroeconometric practice, although it differs slightly from the standard trend-break models in that it offers at least a partial explanation of trend breaks as consequences of government policy changes rather than as purely unexplained random phenomena. Second, Eq. (1) tells us which taxes enter each of the three terms on the right side of the equation. We will use these results in formulating our estimating equation.

Taxes alter various net rates of return and so appear in Peretto's models as modifications of some of the underlying parameters. Regulation also alters net rates of return and so has effects similar to taxes. As with taxes, exactly how regulation enters the analysis depends on the specific regulation, but in general regulation may modify every parameter in a macroeconomic model. Various regulations certainly affect TFP in the production of final goods (e.g., Title 7: Agriculture; Title 12: Banks and Banking; Title 15: Commerce and Foreign Trade; Title 30: Mineral Resources; Title 49: Transportation), the degree to which quality improvement raises productivity (e.g., Title 37: Patents, Trademarks, and Copyrights), entry costs (e.g., Title 16: Commercial Practices), and fixed costs (e.g., Title 20: Employees' Benefits; Title 29: Labor; Title 38: Pensions, Bonuses, and Veterans' Benefits). Regulations may affect both the population growth rate and the rate of time preference by affecting the probability of dying (e.g., Title 21: Food and Drugs; Title 42: Public Health).[12] Finally, regulation (e.g., Title 16: Commercial Practices) may even affect the preference for leisure relative to consumption by affecting advertising, which in turn can alter tastes, as discussed by Sa (2007). We thus can expect regulation to enter the equation for output in each place that one of the underlying parameters enters. Ideally, we would enter different kinds of regulations in various parts of the equation, just as Peretto did with taxes, but data limitations make the ideal infeasible. The CFR organizes regulations by broad categories called titles. Different regulations within the same title may do very different things. For example, some regulations in Title 29 (Labor)

[9] One relevant detail is the nature of government purchases. Equation (1) is based on the assumption that the government sets the tax rates and adjusts purchases to satisfy the government budget constraint. If instead the government sets purchases and adjusts tax rates, a somewhat different form arises. As we discuss below, evidence on the behavior of government expenditures and tax rates suggests that equation is the right form to use. Another detail is that Peretto restricts analysis to constant tax rates. Intertemporal changes in tax rates introduce additional elements in the A, B, and C functions. We address this issue briefly in our discussion of the specification of the estimating equation.

[10] For example, the intercept term $A(t_L)$ in Peretto (2007a) is the simplest of the three functions and has the form:

$$\frac{(1-t_L)(1-\theta)}{(1-t_L)(1-\theta)(1+\gamma)+\gamma(\rho-\eta)\beta\theta^2}$$

which is neither linear nor log-linear in $t_L$ or even $1-t_L$. The functions $B(.)$ and $C(.)$ are far more complicated.

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

[12] The probability of dying affects the rate of time preference in the household choice model extended to include random time of death. See Blanchard (1985).

deal with job safety, and others deal with collective bargaining. We have page counts for each CFR title but not for lower levels. We thus cannot distinguish among regulations that may be very different and so are limited to including only our overall measure of regulation as follows:

$$Y_t = A\left(R_t, t_L; \Omega_A; K_0\right) e^{B(R_t, t_\pi, t_D, t_V; \Omega_B)t} C\left(R_t, t_\pi, t_D, t_V; \Omega_C\right) \tag{2}$$

where $R_t$ is our measure of regulation. Regulation has three distinct types of effects: (i) *level effects* through the intercept term $A(.)$, (ii) *growth rate (or trend) effects* through the exponent $B(.)$, and (iii) *transition dynamic effects* through the term $C(.)$. We will estimate a version of Eq. (2) so that we can measure these three effects.

## 4 Estimation

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

### 4.1 Variables to be examined

We want to study the effect that federal regulation has on macroeconomic activity. As noted earlier, 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. 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, so we also examine how regulation affects the main determinants of output. If we suppose a Cobb–Douglas production function, then output $Y_t$ is given by

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

where $D_t$ 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 $D$, $K$, and $N$ as well as $Y$.

### 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 30 %. We include two explanatory variables other than regulation: total government purchases $(G)$ and the federal marginal tax rate on personal income $(T)$. Government purchases are the sum of government consumption and government investment and are from NIPA. The marginal tax rate is from Stephenson (1998) with updates to 2005 computed by us. It includes both the federal personal income tax and the Social Security tax.[13] Although theory suggests different

[13] Barro and Ridlick (2011) present an alternative series on marginal income tax rates. It is an extension of the series originally published by Barro and Sahaskul (1983, 1986). However, Akhand and Liu (2002) have

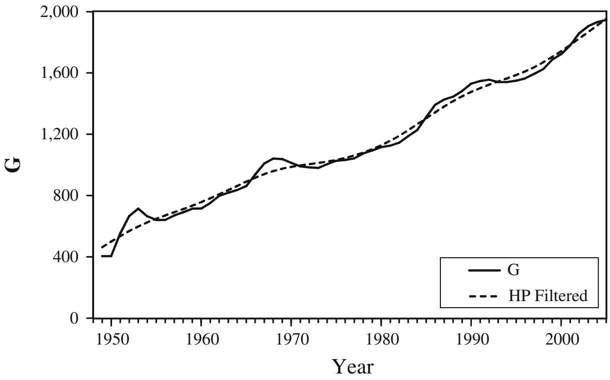


**Fig. 3** Government purchases over time

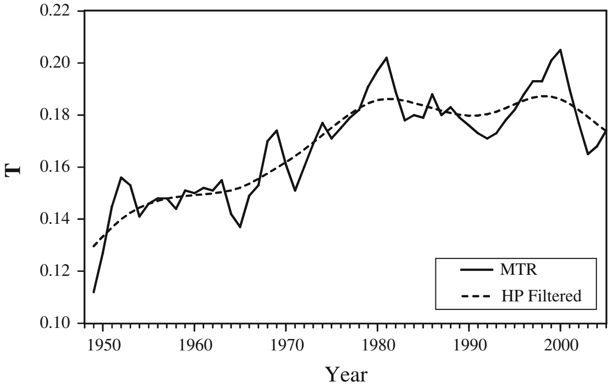


**Fig. 4** Marginal tax rate over time

roles for different taxes, as shown in Eq. (2), data are not available for separate marginal tax rates on labor income, corporate profits, dividends, or capital gains. The main taxes on labor income are the personal income tax and the Social Security tax. Corporate profits are taxed twice, once by the corporate income tax and once by the personal income tax. The marginal corporate income tax is notoriously difficult to measure because of almost-whimsical tax provisions that come and go over time, such as safe harbor leasing in the 1980s, have huge effects on the effective tax rate. Dividends are taxed at the personal income tax rate, but capital gains sometimes are taxed that way and sometimes are taxed under special provisions. All in all, Stephenson's measure of the effective marginal income tax rate seems a reasonable proxy for "the" tax rate. We thus replace the four separate tax rates $t_L$, $t_\pi$, $t_D$, and $t_V$ in Eq. (2) by Stephenson's single rate and modify the estimation equation accordingly, as discussed below. Figures 3 and 4 show the time series for $G$ and $T$. All data are annual observations for the U.S. over the period 1949–2005.

Finally, Figs. 1, 2, 3, 4, and indeed all the rest of our figures, show both the raw data and the H-P filtered data, but all the analysis that follows uses the raw data only. The H-P filtered data are shown solely to illustrate the general movements of the data, which often are obscured

shown that the Barro–Sahaskul construct is considerably inferior to Stephenson's as a measure of average marginal tax rates, so we restrict attention to Stephenson's series.

by the high-frequency variation of the raw series. Also, some figures show levels and others show growth rates, whereas our empirical work uses a mixture of levels and log-levels, as discussed below.

## 4.3 Regression model and estimation issues

We explain how we use Eq. (2) to obtain a tractable estimating equation. We then discuss the stationarity properties of the data and the exogeneity of our explanatory variables.

### *4.3.1 The regression model*

Equation (2) gives us a general form for our estimation equation. Peretto (2007a) derives closed-form solutions for the three functions $A(.)$, $B(.)$, and $C(.)$ in his Eq. (1) because he has a completely specified function for each tax law; in particular, each tax is linear and constant over time. We cannot obtain closed-form solutions for $A(.)$, $B(.)$, and $C(.)$ because we do not have specific functional forms for the way regulation affects the economic quantities. We therefore use quadratic approximations for $A(.)$, $B(.)$, and $C(.)$. Also, regulation is not constant but changes over time, so we generalize Eq. (2) by including lagged as well as current regulation to pick up the extra transition dynamics induced by time-varying regulation. The quadratic is simple and suffices for this first empirical exploration of regulation's macroeconomic effects. 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.

We thus have

$$X_t = \left(e^{\left[\beta+\sum_{j=0}^{J_1}\gamma_j Z_{t-j}+\sum_{j=0}^{J_2}\delta_j Z_{t-j}^2\right]t}\right)\left(e^{\alpha}\left[\prod_{j=0}^{J_3} Z_{t-j}^{\omega_j}\right]U_t\right) \tag{3}$$

where $X$ is any of our dependent macro variables $Y$, $TFP$, $K$, or $N$; $Z$ is an exogenous explanatory variable for $X$ (such as $R$, $T$, or $G$); $\alpha$, $\beta$, $\gamma_j$, $\delta_j$, and $\omega_j$ are constants; $J_i$ are lag lengths; and $U$ is a log-normally distributed residual. Generalization to the case where $Z$ is a vector is straightforward. The entire first term in parentheses in Eq. (3) is a trend term. The trend coefficient is a quadratic function of $Z$. It nests the simpler linearly detrended model with constant trend ($\gamma_j = 0, \delta_j = 0$ for all $j$). The second large term in parentheses in (3) is a combination of the intercept and cycle terms $A(.)$ and $C(.)$ in Eq. (2) that we explain in more detail presently.

We base our estimation on Eq. (3). It is not quite in the same form as Eq. (2), the growth equation that motivates it, because $Z$ may contain a trend of its own. Equation (2) collects all trend elements in the single term $B(.)$. It is easy to see, however, that Eqs. (2) and (3) are equivalent by decomposing $Z$ into its trend and cycle components and then combining the trend component with the first term in parentheses in (3) to obtain a total trend term. Suppose $Z$ obeys

$$Z_t = e^{\alpha_Z} e^{\beta_Z t} V_t$$

where $\alpha_Z$ and $\beta_Z$ are constants, with $\beta_Z$ being the trend in $Z$, and $V$ is a log-normally distributed residual that is the variation about the trend. Substituting into (3) and doing some straightforward algebra gives

$$\begin{aligned} X_t &= \left(e^{\left[\beta+\beta_Z \sum_{j=0}^{J_3}\omega_j+\sum_{j=0}^{J_1}\gamma_j Z_{t-j}+\sum_{j=0}^{J_2}\delta_j Z_{t-j}^2\right]t}\right)\left(e^{\left[\alpha+\alpha_Z \sum_{j=0}^{J_3}\omega_j-\beta_Z\sum_{j=0}^{J_3}\omega_j\right]}\left[\prod_{j=0}^{J_3}V_{t-j}^{\omega_j}\right]U_t\right) \\ &= Ae^{B(Z_t)t}\left(\left[\prod_{j=0}^{J_3}V_{t-j}^{\omega_j}\right]U_t\right) \\ &= Ae^{Bt}C \end{aligned} \tag{4}$$

where $A = \exp(\alpha + \alpha_Z\Sigma\omega - \beta_Z\Sigma \mathrm{j}\omega_j)$ and $C = (\Pi V_{t-j}.^{\omega})U_t$. Equation (4) has the same form as Eq. (2).

The first term $A$ in Eq. (4) is a constant, the second term is the trend with trend coefficient $B(Z_t) = \beta + \beta_Z\Sigma\omega_j + \Sigma\gamma_j Z_{t-j} + \Sigma\delta_j Z_{t-j}^2$, and the third term $C$ is the compound residual $U(\Pi V^{\omega})$. The first term inside $B(Z_t)$ is the constant $\beta$, which captures trend elements apart from any effects of $Z$. In particular, it would be the trend in $X$ if $Z$ were held constant, a fact we use in the analysis below. The last three terms in $B(Z_t)$ collect the various effects of $Z$ on the trend in $X$. In what follows, we refer to the first term $\beta$ as the *trend-apart effect* (because it captures the trend that would be present if all the exogenous variables were trendless), the second term $\beta_Z\Sigma\omega_j$ 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. Below, we discuss counterfactual paths that would have emerged if exogenous variables had been held constant. Doing that requires distinguishing among the quantities $\beta$, $\omega\beta_Z$, and $\beta+\omega\beta_Z$. The compound residual $U(\Pi V^{\omega})$ is the cycle term and corresponds to $C(.)$ in Eq. (2). If $X$ is either individually stationary or jointly trending (cointegrated) with $Z$, the compound residual is purely transient, causing fluctuations about trend. The term $\Pi V^{\omega}$ captures the part of the residual due to $Z$'s deviation from its trend. We call this transient component the *cycle effect*.[14] When $Z$ equals its "normal" (or balanced growth) value, there is no transient effect, $V_{t-j} = 1$ for all $j$, and the only remaining level effect arises from the intercept term $e^{\alpha_Z}$.

Transition dynamics appear in two places in Eqs. (3) or (4): the lagged values of the explanatory variables included in the growth rate and the lagged values included in the cycle effect. The balanced growth rate is the value of the growth rate in (3) or (4) when $Z$ is constant. When $Z$ changes, the balanced growth rate also changes, causing transition dynamics in going from the old balanced growth path to the new one. Changes in $Z$ induce additional transition dynamics through the cycle effect. The cycle effect captures movements about a given growth path. The same type of distinction appears in discrete-time ARIMA models in which a variable's level has a growth term and a random term, and the growth term itself is stochastic with a random term, such as

$$\begin{aligned} X_t &= W_t + \varphi(L)\,u_t, \\ W_t &= W_{t-1} + \theta(L)\,v_t. \end{aligned}$$

Both $u_{\mathrm{t}}$ and $v_{\mathrm{t}}$ cause transition dynamics. The growth term that appears in the transition dynamics here was absent from Eq. (2). It arises from time variation in the regulation and tax parameters which were treated as constant in Eq. (2). Peretto (2007a) restricted his analysis to

[14] 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 $\beta$, so that $U$ captures the transient components.

an economy that starts off the balanced growth path (and therefore has transition dynamics) but that has constant values for all policy parameters. In contrast, we must allow changes in the policy parameters to capture the intertemporal variation present in the data.

To carry out the estimation, we take the natural log of both sides in (3) to obtain

$$x_t = \alpha + \left[ \beta + \sum_{j=0}^{J_1^Z} \gamma_j^Z Z_{t-j} + \sum_{j=0}^{J_2^Z} \delta_j^Z Z_{t-j}^2 \right] t + \sum_{j=0}^{J_3^Z} \omega_j^Z z_{t-j} + u_t \tag{5}$$

where lower-case variables are the natural logs of corresponding upper-case variables.

#### *4.3.2 Stationarity properties of the variables*

Equations (3) and (5), derived from economic theory, suggest that output may be trend-stationary. Is it? If so, we can estimate Eq. (5) as is. If not, we must reconsider the appropriate econometric method for estimating the equation. We test the natural log of output for a unit root in both the absence and presence of structural breaks. We use natural logs because those are the forms in the estimating Eq. (5). Table 3 reports the test results. The DF-GLS (Elliott et al. 1996) test cannot reject a unit root in the natural log of output at the 5 % level. However, it is well known that conventional unit-root tests often fail to reject the unit-root null when there is a break in the trend function under the stationary alternative hypothesis.[15] Thus, we consider the unit-root test proposed by Zivot and Andrews (1992) which allows for structural breaks. The Zivot–Andrews test also fails to reject the unit-root null. These results suggest that output's time series behavior is nonstationary. The DF-GLS and Zivot–Andrews tests also fail to reject a unit root for TFP, physical capital, and labor—the other dependent variables considered in our analysis.

We also conduct stationarity tests for the independent variables in our model—namely, regulation, the marginal tax rate, and government purchases. These results are also reported in Table 3. The DF-GLS and Zivot–Andrews tests fail to reject the unit-root null for regulation, suggesting that it is nonstationary. For the marginal tax rate, the Zivot–Andrews test provides a rejection at the 5 % level of significance, but not the 1 % level. In light of this marginal rejection, we will proceed in the analysis that follows with the assumption that the marginal tax rate is also nonstationary. The Zivot–Andrews test provides a strong rejection of the unit-root null for government purchases, clearly suggesting that it is a stationary variable.

Taken together, the results in Table 3 suggest that the variables in our model are nonstationary. (The only exception is government purchases, whose inclusion in the model will be addressed further in the following section.) A finding that all of the variables in the model are nonstationary suggests that a cointegrating relationship is the only possibility that is consistent with our theory. To test for cointegration, we conduct Engle–Granger cointegration tests for each of our dependent macro variables (individually) and the independent variables (jointly, excluding government purchases) in our model. The null hypothesis of no cointegration is rejected at conventional significance levels in the test for cointegration among output and the independent variables. However, the tests for TFP, physical capital, and labor fail to reject the null hypothesis. It is well known that the power of these tests is low (see Hakkio and Rush 1991) and we do not want to throw out a well-formulated theory on the basis of low-power econometric tests. An alternative test for cointegration is the Johansen test. The

[15] See, for example, Perron (1989, 1997), Banerjee et al. (1992), Zivot and Andrews (1992), and Vogelsang and Perron (1998) for further details.

**Table 3** Stationarity tests

| Variable | DF-GLS test: Test t-statistic | DF-GLS test: Critical values | Zivot–Andrews test: Test t-statistic | Zivot–Andrews test: Critical values |
|---|---|---|---|---|
| $y$ | −2.893 | 1% −3.751 | −5.114 | 1 % −6.28 |
| | | 5 % −3.174 | | 5 % −5.61 |
| | | 10 % −2.875 | | 10 % −5.28 |
| $tfp$ | −1.151 | 1 % −3.747 | −5.208 | 1 % −6.28 |
| | | 5 % −3.171 | | 5 % −5.61 |
| | | 10 % −2.872 | | 10 % −5.28 |
| $k$ | −2.847 | 1 % −3.751 | −4.554 | 1 % −5.76 |
| | | 5 % −3.174 | | 5 % −5.07 |
| | | 10 % −2.875 | | 10 % −4.75 |
| $n$ | −1.981 | 1 % −3.751 | −3.147 | 1 % −6.28 |
| | | 5 % −3.174 | | 5 % −5.61 |
| | | 10 % −2.875 | | 10 % −5.28 |
| $r$ | −1.940 | 1 % −3.759 | −3.850 | 1 % −6.28 |
| | | 5 % −3.180 | | 5 % −5.61 |
| | | 10 % −2.881 | | 10 % −5.28 |
| $\tau$ | −2.686 | 1 % −3.751 | −5.845 | 1 % −6.28 |
| | | 5 % −3.174 | | 5 % −5.61 |
| | | 10 % −2.875 | | 10 % −5.28 |
| $g$ | −3.039 | 1 % −3.751 | −10.097 | 1 % −6.28 |
| | | 5 % −3.174 | | 5 % −5.61 |
| | | 10 % −2.875 | | 10 % −5.28 |

*Notes*. The variables are: $y$ = ln (output), $tfp$ = ln (total factor productivity), $k$ = ln (physical capital), $n$ = ln (labor), $r$ = ln (regulation), $\tau$ = ln (marginal tax rate), and $g$ = ln (government purchases). The DF-GLS test includes an intercept and trend. The Zivot–Andrews test assumes one break in both intercept and trend at an unknown, endogenously determined time for all variables except $k$, which includes only a break in trend. Critical values for the Zivot–Andrews test are finite-sample critical values calculated by Monte Carlo methods for a sample size of 58 based on 10,000 replications and no trimming (per the recommendation of Perron (1997))

Johansen methodology, however, also seems inappropriate since it generally produces a vector error correction model which is inconsistent with our theory which suggests estimating the model in levels. As such, we will proceed with the estimation of the model suggested in Eq. (5) using single-equation methods that correct the standard errors for the presence of cointegrated variables. We defer further discussion of the appropriate estimation technique to the following sections.

### *4.3.3 Exogeneity*

We also need to examine the sensitivity of our endogenous macro variables to the three policy variables: regulation $R$, government purchases $G$, and the marginal tax rate $T$.[16] Granger-causality tests for $R$ and the dependent variables we want to consider (output $Y$, $TFP$, physical

[16] 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. We also ignore

**Table 4** Bivariate Granger-causality tests, 1949–2005

| Null Hypothesis | Lags | $N$ | $F$ | $p$ value |
|---|---|---|---|---|
| $r \nrightarrow y$ | 3 | 54 | 1.638 | 0.193 |
| $y \nrightarrow r$ | 3 | 54 | 2.198 | 0.101 |
| $r \nrightarrow tfp$ | 3 | 54 | 3.447 | 0.024 |
| $tfp \nrightarrow r$ | 3 | 54 | 1.719 | 0.176 |
| $r \nrightarrow k$ | 3 | 54 | 2.422 | 0.078 |
| $k \nrightarrow r$ | 3 | 54 | 1.694 | 0.181 |
| $r \nrightarrow n$ | 3 | 54 | 2.333 | 0.086 |
| $n \nrightarrow r$ | 3 | 54 | 1.986 | 0.129 |

*Note*. The notation $z \nrightarrow x$ means that $z = \ln Z$ does not Granger-cause $x = \ln X$

capital $K$, and labor $N$) are reported in Table 4. We conduct the tests using natural logs of all variables $x = \ln X$ to be consistent with our estimating Eq. (5).[17] At the 10 % significance level, there is causality running from $r$ to *tfp*, $k$, and $n$, and no causality running from any of the dependent variables to $r$, a finding that is consistent with econometric exogeneity of $r$. We also perform Granger-causality tests (not reported in Table 4) of the exogeneity of $g$ and $\tau = \ln T$. The tests indicate econometric exogeneity of $\tau$, but $g$ appears to be econometrically endogenous, with causality never running from $g$ to the dependent variables but running from both $y$ and $\tau$ to $g$. These results are consistent with government setting tax rates and then choosing $G$ to satisfy the government budget constraint as tax revenue fluctuates with the movement of the economy. In light of these results, we treat $R$ and $T$ as exogenous and exclude $G$ from the analysis that follows. Exploration of regressions that include $G$ show no important differences from regressions that omit it, so henceforth we ignore $G$. Eliminating $G$ from the analysis also excludes the only variable that is found to strongly reject the unit-root null in the Zivot–Andrews test reported in Table 3, thus ensuring a cointegrated system.[18]

### 4.4 Estimation

When we replace $Z$ by the vector $(R, T)$ in Eq. (5), we obtain the following estimating equation:

$$x_t = \alpha + \left[\beta + \sum_{j=0}^{J_1^R} \gamma_j^R R_{t-j} + \sum_{j=0}^{J_2^R} \delta_j^R R_{t-j}^2 + \sum_{j=0}^{J_1^T} \gamma_j^T T_{t-j} + \sum_{j=0}^{J_2^T} \delta_j^T T_{t-j}^2\right] t + \sum_{j=0}^{J_3^R} \omega_j^R r_{t-j} + \sum_{j=0}^{J_3^T} \omega_j^T \tau_{t-j} + u_t. \quad (6)$$

government debt on the assumption that Ricardian equivalence holds, a proposition with much support in the empirical literature.

17 Both log-levels and levels of $R$, $T$, and $G$ appear in, so we examined Granger causality using levels of $R$, $T$, and $G$ as well. The results are qualitatively the same as those reported in Table 4.

18 We 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 57 observations, the tests have few degrees of freedom, leaving the results uninformative. This problem with degrees of freedom arises again later, where we discuss it in more detail.

Note that Eq. (6) derives from a coherent theory of endogenous growth. It contains no lagged dependent variables because the underlying theory does not predict the presence of such variables, as shown in Eq. (2). It also is not a VAR. VARs usually can be considered as linear approximations to a poorly understood theoretical model, useful when the theory provides little guidance on the correct specification. In contrast, we have a well-developed theory providing a great deal of information on the equation to be estimated, which does not include a lagged dependent variable.

In light of the stationarity properties of our variables discussed above, we need an appropriate methodology for estimating equation (6). We use the dynamic OLS (DOLS) procedure suggested by Saikkonen (1992) and Stock and Watson (1993). The DOLS method involves augmenting Eq. (6) with $p$ lags and $q$ leads of $\Delta Z_t$, which eliminates the feedback in the cointegrating system and produces an asymptotically efficient estimator. More specifically, for $Z$ given by the vector $(R, T)$ the DOLS procedure adds

$$\sum_{j=-p}^{q} \mu_j^R \Delta r_{t+j} + \sum_{j=-p}^{q} \mu_j^T \Delta \tau_{t+j}$$

to the model to be estimated in Eq. (6). Standard OLS estimates of the coefficients from the augmented regression are consistent, but the usual $t$- and $F$-statistics must be re-scaled using an estimate of the long-run variance of the DOLS residuals. See Hamilton (1994, pp. 608–612) for a description of this non-parametric correction for serial correlation. The coefficient estimates on the lags and leads of $\Delta Z_t$ are of no interest and are not reported.

In the DOLS estimation of (6), the lag lengths $J_x$ and the appropriate number of lags $p$ and leads $q$ of $\Delta Z_t$ are chosen by imposing an initial value of 3 on $J_x$ and 5 on $p$ and $q$ and searching, subject to two restrictions, over all possible smaller values to find that which minimizes the Schwarz–Bayes Criterion (SBC). 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 $Z_t$ but retained $Z_{t-1}$, that model was not considered. Exclusion of $Z_t$ would be allowed only if $Z_{t-1}$ also was excluded. The reason for that 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. Tables 5 and 6 reports the estimation results for the four macro variables of interest. Table 5 shows the estimated values for all coefficients pertaining to regulation and Table 6 shows all other parameter estimates. Very few lagged variables are significant, so to save space Tables 5 and 6 restricted to those lags that had significant coefficients 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 $\beta_R$ and the sum of the $\omega_j^R$ coefficients. The latter are reported in Tables 5, and 6 and the former is obtained by estimating the equation $r_t = \alpha_R + \beta_R t + \nu_t$. The estimated value of $\beta_R$ is 0.0322. Estimating the analogous equation for the tax rate gives a trend in $T$ of $\beta_T = 0.0058$.

**Table 5** Model estimates—regulation parameters

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

| | $x = y$ | $x = tfp$ | $x = n$ | $x = k$ |
|---|---|---|---|---|
| $\gamma_0^R$ | 1.06E−06 (8.092) | 3.93E−07 (11.643) | 7.07E−07 (10.598) | 3.84E−07 (4.104) |
| $\gamma_1^R$ | 2.47E−07 (2.927) | −3.42E−09 (−0.142) | – | – |
| $\gamma_2^R$ | −4.17E−07 (−5.283) | −1.27E−07 (−6.120) | – | – |
| $\delta_0^R$ | −4.81E−12 (−9.846) | −1.67E−12 (−11.792) | −3.33E−12 (−9.059) | −1.84E−12 (−3.659) |
| $\omega_0^R$ | −0.795 (−3.945) | −0.434 (−11.257) | −0.304 (−3.164) | −0.241 (−1.508) |
| $\omega_1^R$ | −0.415 (−2.625) | – | −0.366 (−4.416) | −0.203 (−1.572) |
| $\beta_R \Sigma\omega_j^R$ | −0.039 | −0.014 | −0.022 | −0.014 |
| $\Sigma\gamma_j^R$ | 8.90E−07 | 2.62E−07 | 7.07E−07 | 3.84E−07 |
| {$F$ test: $\Sigma\gamma_j^R = 0$} | {92.156} | {92.053} | {NA} | {NA} |
| [$p$-value] | [0.000] | [0.000] | [NA] | [NA] |
| $\Sigma\delta_j^R$ | −4.81E−12 | −1.67E−12 | −3.33E−12 | −1.84E−13 |
| {$F$ test: $\Sigma\delta_j^R = 0$} | {NA} | {NA} | {NA} | {NA} |
| [$p$-value] | [NA] | [NA] | [NA] | [NA] |
| $\Sigma\omega_j^R$ | −1.210 | −0.434 | −0.670 | −0.443 |
| {$F$ test: $\Sigma\omega_j^R = 0$} | {114.561} | {NA} | {73.345} | {16.806} |
| [$p$-value] | [0.000] | [NA] | [0.000] | [0.000] |

Estimation by DOLS includes $p$ lags and $q$ leads of $\Delta r_t$ and $\Delta\tau_t$ whose coefficient estimates are not reported. Numbers in parentheses (.) are $t$-statistics corrected for serial correlation using the non-parametric procedure described in Hamilton (1994, pp. 608–612) and may be compared to standard $t$ tables. Numbers in braces {.} are $F$-statistics corrected in a similar manner and may be compared to standard $F$ tables. Numbers in brackets [.] are p-values. Numbers do not always add because of rounding. NA ≡ Not Applicable (when there is only one non-zero parameter, making the sum trivial and calculation of $F$ tests superfluous). The value of $\beta_R$ is 0.0322, as reported in the text

**Table 6** Model estimates—tax parameters, other parameters, other statistics

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

| | $x = y$ | $x = tfp$ | $x = n$ | $x = k$ |
|---|---|---|---|---|
| $\gamma_0^T$ | – | −0.296 (−3.783) | – | – |
| $\gamma_1^T$ | – | 0.251 (3.894) | – | – |
| $\delta_0^T$ | – | 0.722 (3.733) | – | – |
| $\delta_1^T$ | – | −0.591 (−3.733) | – | – |
| $\delta_2^T$ | – | −0.051 (−2.773) | – | – |
| $\omega_0^T$ | −0.613 (5.033) | 0.478 (7.372) | 0.194 (2.989) | 0.043 (0.450) |
| $\omega_1^T$ | −0.159 (−2.271) | −0.193 (−3.507) | −0.137 (−1.907) | 0.401 (3.504) |
| $\beta_T \Sigma\omega_j^T$ | 0.003 | 0.002 | 0.000 | 0.003 |
| $\Sigma\gamma_j^T$ | – | −0.046 | – | – |
| {$F$ test: $\Sigma\gamma_j = 0$} | | {0.299} | | |
| [$p$-value] | | [0.613] | | |
| $\Sigma\delta_j^T$ | – | 0.081 | – | – |
| {$F$ test: $\Sigma\delta_j^R = 0$} | | {0.147} | | |
| [$p$-value] | | [0.714] | | |
| $\Sigma\omega_j^T$ | 0.454 | 0.285 | 0.057 | 0.444 |
| {$F$ test: $\Sigma\omega_j^T = 0$} | {12.368} | {17.544} | {0.449} | {12.506} |
| [$p$-value] | [0.000] | [0.000] | [0.551] | [0.000] |
| $\alpha$ | 20.419 (15.535) | 5.492 (11.993) | 11.527 (13.875) | 13.214 (11.272) |

**Table 6** continued

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

| | $x = y$ | $x = tfp$ | $x = n$ | $x = k$ |
|---|---|---|---|---|
| $\beta$ | 0.028 (10.692) | 0.015 (2.094) | −0.005 (2.980) | 0.029 (11.628) |
| $p, q$ | 3, 3 | 5, 3 | 2, 3 | 1, 3 |
| Adj. $R^2$ | 0.999 | 0.999 | 0.998 | 0.999 |

Estimation by DOLS includes $p$ lags and $q$ leads of $\Delta r_t$ and $\Delta \tau_t$ whose coefficient estimates are not reported. Numbers in parentheses (.) are $t$-statistics corrected for serial correlation using the non-parametric procedure described in Hamilton (1994, pp. 608–612) and may be compared to standard $t$ tables. Numbers in braces {.} are $F$-statistics corrected in a similar manner and may be compared to standard $F$ tables. Numbers in brackets [.] are p-values. Numbers do not always add because of rounding. NA ≡ Not Applicable (when there is only one non-zero parameter, making the sum trivial and calculation of $F$ tests superfluous). The value of $\beta_T$ is 0.0058, as reported in the text

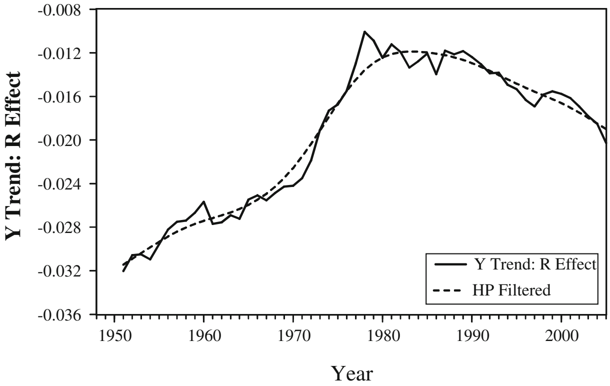


**Fig. 5** Effect of R on Y trend

### *4.4.1 Output*

For output, there are two $\omega_j^R$ coefficients, which sum to $-1.210$. Its product with $\beta_R$ is $-0.039$, indicating that regulation shifts the trend in output down by 3.9 percentage points. That 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 sum of the trend-linear coefficients is positive, causing output's trend to rise on net as regulation grows, and the trend-quadratic coefficient is negative, indicating that the trend-quadratic effect is negative and causes 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 raises output's trend (that is, makes it less negative) until about 1980 and then reduces it. The effect is actually positive from the mid-1970s through the mid-1990s. The average value of the negative effect on output's trend is $-0.0195$, or nearly two percentage points.

The reduction in trend is not the total effect of regulation on output. The large negative values of $\omega_0^R$ and $\omega_1^R$ indicate that regulation also has a substantial cycle effect on output. We can determine regulation's total effect 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.[19] Figure 6 plots the ratio of actual $Y$ to counterfactual $Y$. The time pattern is irregular. The ratio first falls until about 1960, then remains nearly constant until about 1970, then falls again before leveling off in the early 1980s, falls sharply from the mid-1980s through the mid-1990s, rises in the late 1990s, and then falls sharply through 2005. Overall, the ratio falls over the sample period indicating that regulation's total effect on output has been negative.

The overall decline in output relative to its counterfactual is large. By the end of the sample period, output is down to 28 % of its counterfactual, reflecting principally the compounding

[19] More specifically, in the trend term we replace $R_t$ with $[(R_t - R_{1949}) + R_{1949}]^2 = \Delta R_t + R_t$ and replace $R_t^2$ with $[(R_t - R_{1949}) + R_{1949}]^2 = (\Delta R_t)^2 + 2R_t\Delta R_t + (R_t)^2$, multiply by the appropriate estimated trend coefficients ($\gamma$ and $\delta$), and collect all terms containing $\Delta R_t$. Similarly, in the cycle term we replace $R_t$ with $R_{1949}(R_t/R_{1949})$, raise to appropriate estimated power ($\omega$), 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 $\Delta R_t$ and by the cycle components $(R_t/R_{1949})^{\omega}$ is equivalent to setting $\Delta R$ 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.

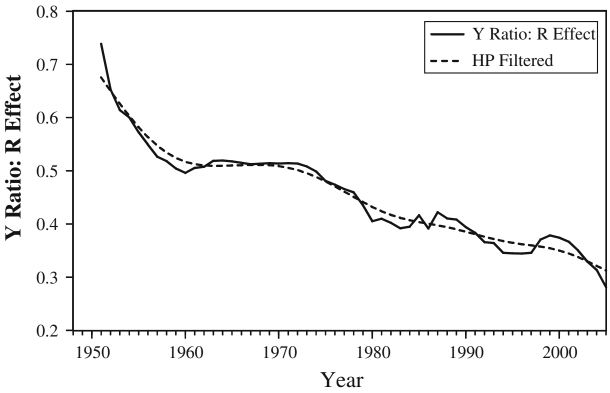


**Fig. 6** Ratio of actual Y to counterfactual $Y|_{\mathrm{R=R(1949)}}$

of the reduction in output's trend growth rate and secondarily the accumulation of cycle effects over a period of 57 years. Using the formula $0.28 = e^{\beta^* 57}$, where $\beta^*$ is the realized average reduction in output's growth rate and 57 is the number of years in the sample, we can calculate that $\beta^* \approx -0.022$, or just over two percentage points. As noted above, the average reduction in output's trend is about 0.0195. The discrepancy reflects the fact that the regulation-induced reduction in output's trend is not constant but rather varies over time, as shown in Fig. 5, and it interacts with the time-varying cyclical effects.

We can convert the reduction in output caused by regulation to more tangible terms by computing the dollar value of the loss involved. The sample period ends in 2005, but let us assume that the same final ratio of 0.28 applies today. In 2011, nominal GDP was \$15.1 trillion. Had regulation remained at its 1949 level, current GDP would have been about \$53.9 trillion, an increase of \$38.8 trillion. With about 140 million households and 300 million people, an annual loss of \$38.8 trillion converts to about \$277,100 per household and \$129, 300 per person. Furthermore, our estimates indicate that the opportunity cost will grow at a rate of about 2 % a year (the average reduction in trend over the sample period) if regulation is merely kept at its 2005 level and not increased further.[20]

Four 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*, our measure does not include any non-production benefits of regulation. The non-production benefits could be both large and growing. Pollution, for example, presumably grows as unregulated industrialization expands and the costs of pollution presumably are convex in the amount of pollution. Any such costs have been reduced by environmental regulation. If regulation has reduced the growth rate of pollution, not just its level, then it correspondingly has introduced a growing benefit that is not included in measured output. We do not attempt to

[20] Of 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.

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.[21] They do, however, make clear that the cost of regulation is substantial and must be taken seriously in any evaluation of regulation's net social benefit. *Third*, 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 would be lower in the absence of traffic regulations, for example. However, most of those most basic regulations were in place well before 1949, so for our work their benefits are simply a given that is impounded in the intercept. *Fourth*, regulation's opportunity cost is far larger than its compliance costs, a topic studied in the existing literature. For example, Crain and Hopkins (2001) estimate the compliance cost of all federal regulation (not just post-1949 regulation) to be about 8 % of current GDP, or about \$1.2 trillion in mid-2011.[22] Our estimates of regulation's opportunity cost arising from reduced GDP are approximately 32 times larger than the compliance cost.

### 4.4.2 *Comparison with previous estimates of regulation's growth effects*

Our estimates of the output losses induced by regulation may elicit "sticker shock" on the part of the reader. Partly that is the nature of exponential growth. Even experts used to working with growth rates often substantially underestimate the effects of even small changes in growth rates on the corresponding levels (Christandl and Fetchenhauer 2009). However, there also is a legitimate concern that such large effects should be checked to make sure they are not an artifact of something. We therefore turn to a comparison of our estimates of regulation's growth effects with estimates already in the literature. Our estimates are consistent with previous estimates obtained from both aggregate and disaggregate data. In fact, our estimates are on the low side compared to many previous results.

Consider first the aggregate data. Maddison (2000) reports (Table B-22) that country growth rates around the world varied tremendously over the period 1950–1998. The highest rates were in Japan (5.20 %), the rest of Asia (3.23 %), and Western Europe (2.93 %), and the lowest rates were in the former USSR (0.81 %) and Africa (1.04 %). Parente and Prescott (1999) argue that most of the differences between the high and low growth rates were the result of "barriers to riches" erected principally by various forms of regulation.[23] The difference between Maddison's maximal and minimal growth rates is 4.39 % or 0.0439. Our estimate of 0.02 for regulation's impact on the US growth rate is quite modest by comparison.

Several studies mentioned earlier use the cross-section data on regulation to examine the impact of specific types of regulation on aggregate growth. Loayza et al. (2004) examine seven main areas of a firm's activity subject to regulation: entry, exit, labor markets, fiscal burden, international trade, financial markets, and contract enforcement. For each area, they construct an index of the severity of regulation and also an overall index for the seven areas taken together (the index comprises a small subset of all regulations, as explained in Sect. 2.4 above). They find that increasing a country's index of regulation by one standard deviation

21 Indeed, Peretto (2008) finds that changes in tax rates can reduce the growth rate of output but raise social welfare. The same divergence could happen with regulation.

22 This 8 % 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 1 % of GDP.

23 See especially Parente and Prescott's chapters 6 and 7.

(i.e., by 34 %) would decrease its annual rate of per capita GDP growth by 0.4 percentage points. By comparison, our time-series study of the US indicates that an increase in *total* regulation of *600 %* reduces growth by just 2 percentage points. Relatively speaking, our effect is smaller. Also, Loayza et al. find that decreasing *product market regulation* in a typical developing country to the median level of industrial countries (that is, from 0.51 to 0.17, which is a reduction of 67 %) would raise that country's annual growth rate by about 1.3 percentage points, a magnitude that is completely consistent with Parente and Prescott's (1999) argument that regulation is a major cause of income differences. It is also in line with our result for changing *total* regulation in the US.

Djankov et al. (2006) study regulations that help or hinder business performance in 135 countries and in several regulatory areas: starting a business, hiring and firing workers, registering property, getting bank credit, protecting equity investors, enforcing contracts in the courts, and closing a business. They find very large effects on growth:

> "The impact of improving regulations is large. In Table 3, we analyze the magnitude by including dummies for each quartile of the business regulation index in the OLS regressions. Improving from the worst (first) to the best (fourth) quartile of business regulations implies a 2.3 percentage point increase in average annual growth [rate of GDP per capita]." (p. 400)[24]

All of these results, many of which only apply to subsets of regulations, are similar to or larger than what we obtain with time series data for total regulation in the US. By comparison, then, our result is not incredibly large.[25]

One may wonder if perhaps the large growth effects are somehow an artifact of aggregate data. It therefore is useful to look at estimates of regulation's effects obtained from disaggregate data. Such studies cannot, of course, estimate aggregate growth effects, but they can tell us whether regulation's effects at the micro level are large or small. If they are small, then one has reason to be skeptical of the large effects found at the aggregate level. In fact, the effects at the micro level are quite large.

Several studies use the OECD series on regulation discussed above to examine the effect of regulation at the micro level. Nicoletti et al. (2001), using industry level data in OECD countries, find that employment protection and product market regulations have large and statistically significant effects on market structure and R&D expenditure. The employment share of large enterprises (a measure of industrial structure) has an average elasticity across countries and industries with respect to employment protection regulation of about $-1.5$ in manufacturing and $-1.0$ in non-manufacturing (see their Table 2).[26] Manufacturing R&D expenditure has an elasticity with respect to employment protection regulation of about $-0.6$

[24] Note that there is a misprint in Djankov et al.'s published Table 3. The entry in the first row of the last column of Table 3 should be $-2.3241$, not $-0.3241$. See their earlier working paper (2005) for the correct entry.

[25] Other estimated aggregate effects of regulation also are large. For example, Loayza et al. (2005) find that increasing a country's index of labor regulation by one standard deviation in the cross-country sample would increase the size of the informal sector relative to GDP by nearly 3 percentage points. Nicoletti et al. (2001) find that cross-country differences in product market regulations in the OECD account for up to 3 percentage points of deviations of the employment rate from the OECD average.

[26] Nicoletti et al. report (in their Table 2) a semi-elasticity of $-0.75$. We converted that to an elasticity using the average product market regulation value of 1.94, found by averaging the individual country values reported in Nicoletti et al. (2000, Table A3.6). We made similar conversions for semi-elasticity estimates pertaining to employment protection regulation, using the average value of employment protection regulation from Table A3.11 in Nicoletti et al. (2000).

(see their Table 6). Nicoletti and Scarpetta (2003) study the effects of product market regulations for detailed manufacturing and service industries in the OECD countries. They find that countries with an above-average share of state-owned firms (such as Finland, France, and Italy, among others) would increase transitional multifactor productivity growth by about 0.7 percentage point and steady-state growth in the manufacturing sector by 0.2–0.4 percentage point simply by reducing the state-owned share to the OECD average (p. 40). Bassanini and Ernst (2002) find that the elasticity of R&D intensity (R&D expenditures relative to the value of output) in 2-digit manufacturing industries of the OECD countries has an elasticity with respect to employment protection regulation of about −1.0 (see their Table 3). Alesina et al. (2003) study the relation between regulation and investment in the transport (airlines, road freight and railways), communication (telecommunications and postal) and utilities (electricity and gas) sectors. Their cross-country estimates imply that decreasing regulation by 40 % raises investment in those sectors by about 2.5 percentage points. An estimate of the effect of regulatory reform in the UK over several years starting in 1984 yields an elasticity of communication and transport investment with respect to their index of regulation of −0.77. The OECD (2003), using firm-level data, finds an elasticity of new firm entry rates with respect to product market regulations of about −3.0 (see their Table 4.6, column D).[27] All of these estimated effects of regulation are quite large and show that the large effects at the aggregate level are consistent with what is seen at the micro level.

The foregoing studies all use some type of cross-section data on subsets of regulation, whereas our estimates are based on time series data on the total body of US regulation. Our estimates of regulation's effects typically are lower than those from the cross-section data. There may be a systematic difference between time series data on the one hand and cross-section and panel data on the other, or it may be that many of the regulations included in our comprehensive measure that are omitted from the OECD measure have smaller effects than the subset of regulations included in the OECD measure, thus leading to a smaller average effect, or our page-count series may have more measurement error that the OECD survey measure, diluting the measured impact of regulation that we obtain from our measure. The source of the difference deserves further study. In any case, though, the comparison of our estimates with those obtained from alternative series using both macro and micro evidence does suggest that the large effects we report above are by no means implausibly large.

One difference between our results and those of most of the existing literature is that we include the marginal tax rate $T$ as an explanatory variable. We explored the effect of omitting $T$. Qualitatively, the results were unchanged, but quantitatively the omission of $T$ made a difference. The effects of $R$ on $Y$ and $TFP$ is larger when $T$ was omitted than when it is included, though they still are smaller than what most of the micro-based literature finds. Thus part but not all of the difference between our estimated effects and those of the micro-based literature seems to reflect an omitted variable problem in the latter.[28]

#### *4.4.3 TFP and the productivity slowdown*

Figures 7 and 8 plot the growth rates of output and TFP. It is easy to see from the Hodrick–Prescott filtered series that the two variables move closely together with closely matched

[27] The OECD's (2003) regressions for entry rates are in levels, so the estimated coefficients are simple derivatives: d(entry rate)/d(regulation). We converted to elasticities by multiplying by (regulation)/(average entry rate $\approx 0.10$), where the country entry rates are reported in Nicoletti et al. (2001, Table A2.1).

[28] In 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.

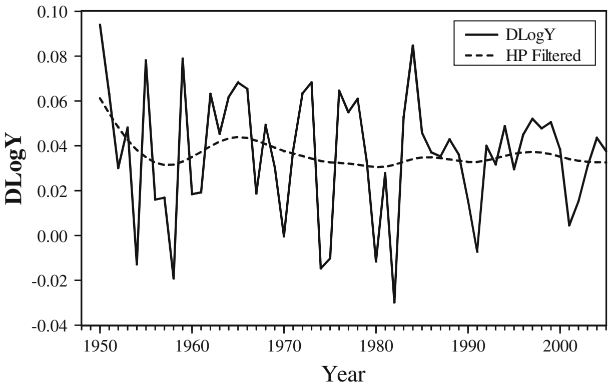


**Fig. 7** Growth rate of output over time

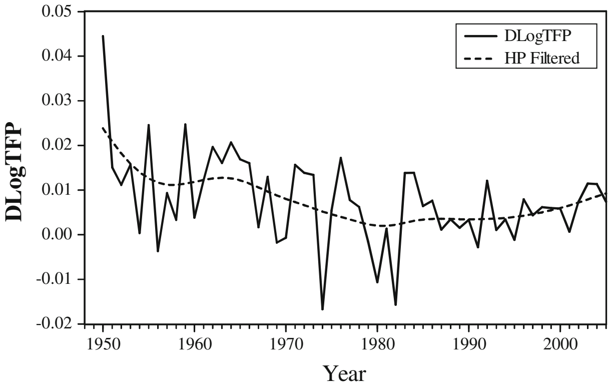


**Fig. 8** Growth rate of TFP over time

turning points. Comparing Fig. 8 with Figs. 1, 2, and 4 suggests that regulation and taxes had something to do with TFP's behavior. TFP growth drops abruptly at the start of the sample, but that seems to be an artifact of the leverage that the first point in the sample has on the initial part of the Hodrick–Prescott filtered series. If we ignore that episode, then TFP really starts falling sometime in the mid-1960s, stops falling in the early 1980s, and grows slowly after that. The falling growth rate between about 1965 and 1980 is the well-known "productivity slowdown." 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 Fig. 2). Tax rates peaked at almost exactly the same time that TFP bottomed out, stopping their rise around 1980 and falling somewhat afterward. Thus, it appears that 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 Tables 5 and 6 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 increases in regulation actually decrease this negative effect on TFP's trend through about 1980. This suggests that, to the extent that regulation contributed to the productivity

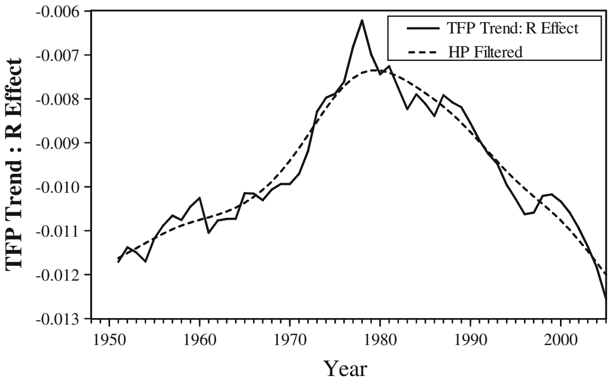


**Fig. 9** Effect of R on TFP trend

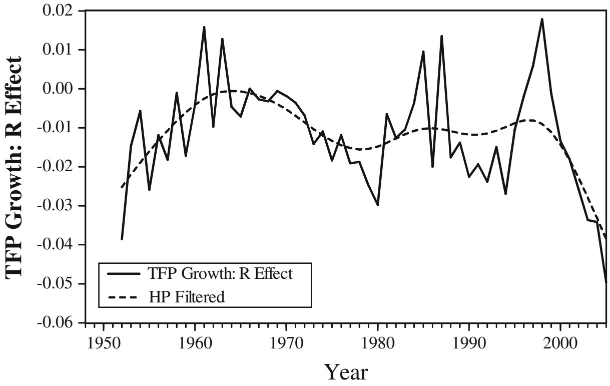


**Fig. 10** Change in ratio of actual TFP to counterfactual TFP$|_{R=R(1949)}$

slowdown, it must have occurred through cyclical rather than trend effects. The combined trend and cyclical effects of regulation can be obtained by constructing the counterfactual series which shows the level of TFP had regulation remained at its 1949 level. Figure 10 shows the change in the ratio of actual TFP to counterfactual TFP. As with output, there is an initial point artifact. Ignoring that, we see that the change in the ratio is about zero in 1965 and then becomes increasingly negative until about 1980. After that, the change rises sharply and is positive in some years through the late 1990s, after which it falls again. Throughout the 1965–1980 period the change is negative, indicating a persistent negative effect of regulation on TFP during the infamous productivity slowdown.

Taxes may also play a role in explaining TFP growth. Figure 11 plots the change in the ratio of actual TFP to counterfactual TFP obtained by holding the marginal tax rate at its 1949 level. Although the effect of taxes is far from constant over time, it is difficult to detect a discernable trend during the years of the productivity slowdown. Indeed, the HP filtered series is generally flat and close to zero over the entire sample period. Thus, it appears that the effect of regulation may dominate that of taxes in explaining the productivity slowdown. This conclusion is confirmed in Table 6, where the sums of the trend-linear and trend-quadratic tax terms in TFP's trend are found to be insignificantly different from zero.

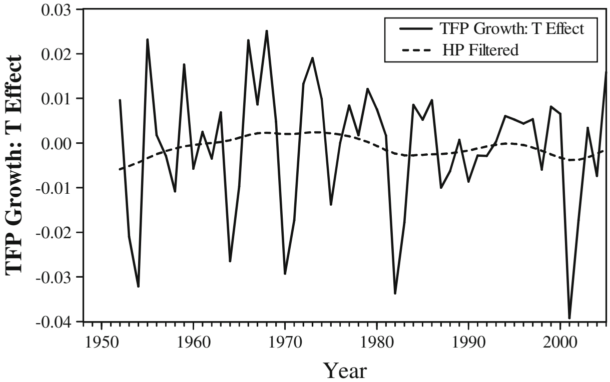


**Fig. 11** Change in ratio of actual TFP to counterfactual TFP$|_{T=T(1949)}$

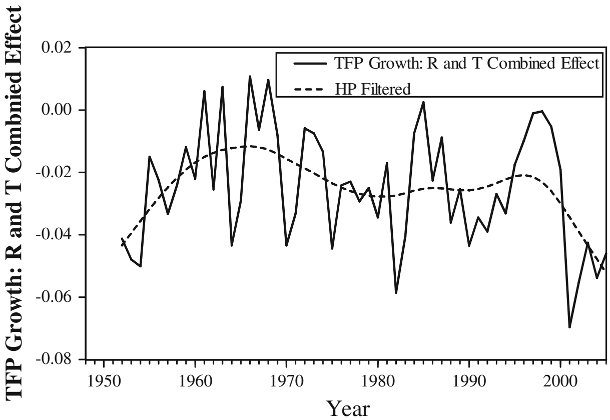


**Fig. 12** Change in ratio of actual TFP to counterfactual TFP$|_{R=R(1949),T=T(1949)}$

Figure 12 shows the combined effects of regulation and taxes on TFP, plotting the change in the ratio of actual TFP to counterfactual TFP obtained by holding both the level of regulation and the marginal tax rate at their 1949 values. The pattern over time is similar to that shown in Fig. 10, again confirming that regulation's effect is dominant. Comparing Figs. 10 and 12 with Fig. 8, which plots the growth rate of TFP over the sample period, shows a close match in the slopes and turning points, suggesting that regulation explains a great deal of the changes in TFP's path, including the productivity slowdown.

The foregoing explanation of the productivity slowdown has an important advantage over alternatives in the literature. Greenwood and Yorukoglu (1997) offer an explanation of the productivity slowdown based on the idea that the economy underwent severe adjustment difficulties in adapting to the large amount of investment expenditure on information technology that began in the late 1970s. Nordhaus (2004) suggests that the productivity slowdown resulted from the OPEC oil shock of the early 1970s. Though both mechanisms may well have had a role to play, they cannot be the entire story because the productivity slowdown started five to ten years earlier than either the technology investment or oil shock in the

1970s. In contrast, as we have seen, significant changes in regulation occurred at the start of the productivity slowdown in the mid-1960s and then again at the slowdown's end around 1980.

It also is interesting that the TFP slowdown apparently was not due to an R&D slowdown and that regulation seems to have no statistically significant net effect on R&D. We use the real value of private R&D expenditure reported by the National Science Foundation as our measure of R&D. The growth rate of TFP has a slightly negative correlation with the growth rate of R&D. Regressing TFP growth on current and two lags of R&D growth yields an adjusted $R^2$ of $-0.04$ and p-values for all R&D coefficients over 0.15. The plot of R&D growth shows that it is negative in much of the first half of the productivity slowdown but positive in all the second half. Also, there are other periods in the 1980s and 1990s when R&D growth is similarly negative with no TFP slowdown. There also is no economically significant relation between regulation and R&D. A simple linear regression of R&D on current and lagged regulation yields insignificant coefficients. If we use the functional form in Eq. (6) with ln(R&D) as the dependent variable $x$, we obtain statistically significant coefficients on both current and lagged values of the linear and quadratic regulation terms in the trend. However, the sums of all linear coefficients and of all quadratic coefficients are both negative but also insignificantly different from zero, implying that regulation's effects on R&D growth disappear within three years. Analysis of the counterfactual path of R&D (i.e., holding regulation at its 1949 level) yields the result that the regulation terms cancel each other out for the most part, with no economically significant short-term effects even on the level of regulation.

The absence of an effect of total regulation on R&D is consistent with other econometric evidence and also with the endogenous growth model that underlies our estimation. The OECD (2003), using data on 18 manufacturing industries in 18 OECD countries, finds that most types of regulation (their PMR measure, consisting of non-tariff barriers to international trade, tariff barriers to international trade, direct state control of economic activities, and their EPL, consisting of labor market regulations) reduce R&D expenditure, but regulation that erects barriers to entry ("barriers to entrepreneurship", which comprises legal limitations on access to markets and administrative burdens and opacities hampering creation of businesses) have a strong positive effect on R&D intensity, with an elasticity of about 1.3.[29] This pattern is completely consistent with Peretto's theoretical results that taxes that directly reduce the return to R&D reduce R&D, as one would expect, but taxes that reduce the return to entry raise the profit of incumbent firms and induce them to increase their R&D. Our total measure of regulation mixes regulations that directly make R&D less profitable and regulations that reduce the return to entry. We tried to examine the effects of individual types of regulation on R&D, but data limitations rendered that impossible, as discussed below.

#### *4.4.4 Capital and labor*

We do not dwell on the results for capital and labor because they are of secondary interest and are generally of the same character as those for output and TFP. See the last two columns in Tables 5 and 6. In particular, regulation has significant effects on both variables, has both growth and cyclical effects, has both linear and non-linear effects on the growth terms, and enters with lags. The pattern of coefficients for labor is very similar to the pattern for output, but the pattern for capital is quite different from those for all the other dependent variables.

[29] See the OECD's (2003, Table 3.3) for estimated coefficients and see Nicoletti et al. (2000, Table A3.2), for values of barriers to entrepreneurship.

The different patterns across the three inputs mean that changes in regulation causes shifts in the relative amounts of inputs used to produce a given amount of output. Regulation affects not only output's path but also the way output is produced.

#### *4.4.5 Individual titles of the CFR*

We tried to isolate the CFR titles with significant impacts on our aggregate dependent variables.[30] However, the large number of titles leaves us too few degrees of freedom to obtain useful results. We could not include any lags. For each possible dependent variable, a small subset of the titles had coefficients individually significant at the 10 % level or less. However, a joint test of the remaining titles always strongly rejected the null that the remaining titles were jointly insignificant, 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.

## 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, TFP, labor, and physical capital. 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 50 years have reduced real output growth by about 2 percentage points on average over the period 1949–2005. That reduction in the growth rate has led to an accumulated reduction in GDP of about \$38.8 trillion as of the end of 2011. That is, GDP at the end of 2011 would have been \$53.9 trillion instead of \$15.1 trillion if regulation had remained at its 1949 level. One channel through which regulation has reduced output is TFP. We find that federal regulation can explain much of the famous and famously puzzling productivity slowdown of the 1970s.

Our results are qualitatively consistent with those obtained from studies using the various cross-country and panel data sets on regulation. Quantitatively, our estimated impact of regulation on aggregate output, large as it is, is similar to or lower than the micro-level impacts estimated in the cross-country and panel data studies. The cross-country and panel data are constructed very differently from our data, covering a subset of total regulations but over an array of countries. It thus seems that regulation has strong and robust negative effects on aggregate output.

Recent years have seen substantial increases in financial, health, and environmental regulation by the Bush and Obama administrations. We do not yet have quantitative measures of those increases. Indeed, many of the new regulations had been mandated by new law but

[30] 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. This is the approach taken in some of the literature cited in the Introduction.

not yet written at the time this article was published. It is up to future research to assess their impact on the economy, but the foregoing results suggest that the impact will be quite large.

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## Appendix: Code of federal regulations

### A.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."[31] 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 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.[32]

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

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[31] 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*, p. 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*.

[32] 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.

year in question.[33] 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.[34]

Following the 1949 edition of the CFR, "pocket supplements" were used to record regulatory changes published in the FR.[35] 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.[36]

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.[37]

### A.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 (2007)

33 No supplement was published for 1942.

34 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.

35 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.

36 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.

37 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.

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 (2007).

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 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.[38]

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.[39] 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 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

[38] There is the possibility that the nature of the language used has changed over time, becoming more or less verbose as time passes, so that the word-to-regulatory-content ratio changes over time. We have no way to control for or even check the existence of such a problem. We know of no evidence that such changes in language occurred over our sample period. Kimble (2002, 2007) has suggested changes in usage that would replace the legalese of federal laws and regulations with more straightforward English, but Kimble himself notes that such changes sometimes result in longer rather than shorter documents. Also, we know of no evidence that such suggested changes ever have been adopted. Indeed, there is anecdotal evidence that simplification has not occurred. President Carter signed an executive order in 1978 requiring that federal regulations be written as simply and clearly as possible. Twenty years later, in 1998, the Clinton administration demanded that regulations be written in plainer prose, suggesting that President Carter's order had not had achieved its goal. We thus proceed on the assumption that there have been no significant changes in the verbosity of regulation over time.

[39] 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.

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 1949–2005.[40] 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.

## A.3 Comparison with other measures of regulation

### *A3.1 Description*

Our measure covers one country over 57 years; earlier measures cover many countries over much shorter periods of time.[41] 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 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.

40 Dawson (2007) 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.

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

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 et al. 2002). Our measure is based solely on the page count of the CFR.

### A3.2 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.[42] 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.

[42] Similarly, Loayza et al. (2005) found very high correlations among their seven indices of regulation.

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 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.[43] 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 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

[43] Nicoletti 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).

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.

### *A3.3 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.[44] 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.[45] The *Federal Register* contains proposed regulations and other irrelevant material; the *U. S. Code* contains all federal laws, not just regulations.

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

45 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.

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