October, 2002

Regulation and the Macroeconomy

John W. Dawson[a,*], John J. Seater[b]

[a]*Department of Economics, Appalachian State University, Boone, NC, 28608-2051*
[b]*Department of Economics, North Carolina State University, Raleigh, NC, 27695*

Abstract

This paper introduces a measure of regulatory activity, namely the number of pages in the *Code of Federal Regulations*, to investigate the relationship between federal regulation and macroeconomic performance in the U.S. Using a simple aggregate production technology in a cointegration framework, we find that the aggregate measure of regulation generally has a negative impact on aggregate economic performance in both the short run and the long run. Some specific areas of regulation are also found to have important long-run effects on economic activity, some positive and some negative.

Keywords: Regulation; macroeconomic performance

JEL classification: L50; O40

---

*Corresponding author: dawsonjw@appstate.edu.

1. Introduction

The macroeconomic impact of regulation has long been a hotly debated topic among politicians and economists alike. While political officials often speak of the benefits of reducing regulation, many public-interest groups demand more regulation. Despite the longstanding debate over the benefits and costs of regulation, estimates of regulation's net cost to society are scant in the economics literature.

One fact which clearly emerges from the few existing studies of regulation is the large and, in many areas, growing amount of regulatory activity in the U.S. economy.[1] Despite slower growth, or possibly decline, in some industry-specific regulation, past decades have witnessed a proliferation of regulations spanning firms in nearly every industry. Prominent among these are environmental regulations promulgated by the Clean Air and Clean Water Acts

---

[1]See, for example, studies by Weidenbaum and DeFina (1978), Litan and Nordhaus (1983), Hahn and Hird (1991), and Hopkins (1991) for estimates of the aggregate cost of the federal regulatory program in the U.S. Hazilla and Kopp (1990) estimate the impact of environmental regulations on the U.S. economy.

of the 1970s and subsequent amendments. Hopkins (1991) estimates that total regulatory costs decreased from $433 billion in 1977 to $371 billion in 1988, and increased steadily thereafter.[2] The decrease from 1977 to 1988 results from decreased costs associated with "economic" (or industry-specific) regulations. Over the same period, however, the EPA (1990) estimates that the costs of environmental regulations increased from $41 billion in 1977 to $86 billion in 1988 and increased even more rapidly into the 1990s following the 1990 Clean Air Amendments.

While these estimates provide evidence on the amount of regulation in the U.S. and a rough indication of trends in regulatory activity, it is difficult to ascertain the net effect of regulation on economic activity at the economy-wide level. This difficulty is in large part due to a lack of time-series data which consistently measure changes in the regulatory environment over time. This paper addresses these shortcomings. Section 2 introduces a measure of federal regulation based on the *Code of Federal Regulations*. Section 3 briefly presents a simple model of the relationship between regulation and economic performance, and provides some preliminary evidence based on traditional regression analysis. Then, the next section uses a more up-to-date methodology based on cointegration to complete the empirical analysis. The last section concludes.

2. The *Code of Federal Regulations* and Measures of Regulatory Activity

2.1. History and Background of the Code of Federal Regulations

Prior to 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."[3] 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

[2]All figures are in 1988 dollars and represent the lower-bound scenario reported by Hopkins.

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

in the case were impugned for their ignorance of the law. This outcome led to the Federal Register Act of 1935 (49 Stat. 500; 44 USC Chapter 15) which established a consistent framework for codification of government regulations throughout the rulemaking process.

The *Federal Register* (*FR*), first published on March 14, 1936, is a daily publication in which proposed regulations appear first in draft form and eventually in final form, if passed into law. The *FR* also contains presidential proclamations, executive orders, announcements of agency hearings and meetings on regulatory issues, grant application instructions and deadlines, official agency decisions and actions, and agency establishments, reorganizations, and dissolutions. Sometimes, there also are long sections containing technical or economic analyses or discussion of issues arising during consideration of a proposed regulation. The final regulations (newly passed into law) contained in the *FR* ultimately are codified in the *Code of Federal Regulations* (*CFR*). Divided into 50 subject categories called titles, the structure of the *CFR* is similar, but not identical, to that of the *United States Code*. Currently, each title of the *CFR* is revised annually and contains all regulations in effect as of the cover date.[4]

The first edition of the *CFR* published regulations in force as of June 1, 1938. In the early years, the *CFR* was not revised annually. Instead, annual supplements carried in full text all changes and additions to the 1938 edition of the *CFR* as published in the *FR*. The supplements covered the periods June 2-December 31, 1938 and subsequent calendar years through 1941, listing regulatory changes promulgated during the period and in effect on December 31 of the year in question.[5] 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

---

[4]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 as a measure of regulation.

[5]No supplement was published for 1942.

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.[6]

Following the 1949 edition of the *CFR*, "pocket supplements" are used to record regulatory changes published in the *FR*.[7] Pocket supplements differ from the annual supplements to the first edition of the *CFR* in that they are cumulative; that is, pocket supplements record the full text of changes to the 1949 *CFR* in effect at the end of the year covered. The first pocket supplement covers changes during the June 2 to December 31, 1949 period and subsequent pocket supplements include 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* which occurred between June 2, 1949 and December 31, 1950 that were in effect on December 31, 1950. Thus, even if no regulatory changes occurred during 1950, the 1950 pocket supplement would record any changes which occurred from June 1 through December 31, 1949 (i.e., the 1950 pocket supplement would be identical to the 1949 pocket supplement).[8]

From time to time, as warranted by growth of the pocket supplements, individual titles (or individual parts of a title) of the 1949 *CFR* are revised. These revisions represent a complete codification of regulations in effect as of December 31 of the year in which they are published. The timing of revisions varies considerably across titles. In all titles, however, revisions

---

[6]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*.

[7]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.

[8]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.

become more frequent over time. In 1950, for instance, only Parts 71-90 of Title 49 (Transportation and Railroads) are revised. In 1960, all or parts of Titles 1-5, 14, 18-20, 26, 27, 32, 40, 41, 49, and 50 are revised, and by 1968, all *except* Titles 34, 35, and 37 are revised. Beginning in 1969, all titles of the *CFR* are revised annually.[9]

2.2. Measuring Regulatory Activity Using the CFR

The consistent codification of federal regulations in the *CFR* since its inception in 1938 provides a unique source of information on regulatory activity over the years. Dawson (2000) constructs series measuring regulatory activity based on the number of pages published in the *CFR's* various editions and supplements. Although the number of pages of regulation cannot capture the differential effects of alternative regulations on economic activity, it affords new information on the temporal behavior of regulatory activity. The remainder of this section provides a summary of these *CFR*-based measures of regulation. For a complete description of the methodology used to construct the series and a statistical comparison of the various series, see Dawson (2000).

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.[10] Estimating total pages of regulation during the periods between the 1938, 1949, and subsequent revisions is more problematic. One approach, which explicitly utilizes 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. Although it is conceivable that this behavior marks the increase in regulation associated with World War II and the subsequent

[9]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.

[10]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.

decrease following the war, it is more likely a result of the additive nature of the supplements used to codify regulatory changes between the 1938 and 1949 revisions of the *CFR*. The additive nature of the supplements leads to a sort of "double-counting" problem in years between complete revisions of the *CFR*.[11] Growth in the estimated pages of regulation resumes in the early 1950s and moderates into the 1960s. This behavior is also consistent with a double-counting problem in the proposed series, as supplements are widely used in the years following the 1949 revision but become less frequent as revisions begin to make up a larger portion of each year's codification. Double-counting is not an issue once the *CFR* adopts a policy of annual revisions beginning in 1969.

Figure 1 displays the series measuring regulatory activity for the period 1949-2000. Casual examination of Figure 1 indicates that the 1970s witnessed more rapid growth in the number of pages of federal regulation than any decade from 1938 through 2000. This finding is somewhat surprising given the deregulation that occurred in transportation, telecommunications, and energy beginning in the 1970s. Clearly, any deregulation that did occur in these industries is offset by increased regulation in other areas. Recall that Hopkins (1991) suggests that decreases in economic or industry-specific regulations are coincident with increases in social regulations over the period 1977-1988. The behavior of the regulatory series is equally interesting during the 1980s, when the Reagan administration promoted deregulation as a national priority. While the growth in the number of pages in the *CFR* clearly slows in the early 1980s, a decrease in total pages occurs only in one year, 1985, and by the late 1980s the growth rate is comparable to that of the 1970s. The 1990s witnessed the largest reduction in pages of regulation in the history of the *CFR*, when three consecutive years of decline are recorded. This coincides with the Clinton administration's "reinventing government" initiative which boasted of reduced regulation in general and a reduction in the number of pages in the *CFR* in particular. Figure 2 provides further evidence on regulatory activity since the 1970s. This figure graphs the number of pages in titles 40 (Protection of the Environment), 46 (Shipping), 47 (Telecommunications), and 49 (Transportation) of the *CFR*.

[11]Dawson (2000) discusses the "double-counting" problem in more detail and offers some alternative methods for constructing the regulatory series based on interpolation in the non-revision years. The results of the analysis in this paper are not sensitive to the construction method, thus we restrict attention to the series discussed here.

3. Regulation and the Economy: Model and Preliminary Evidence

In this section the emphasis shifts from measuring regulatory activity to an analysis of the relationship between regulation and macroeconomic performance. We begin with a brief outline of the simple theoretical model used to study this relationship, and then present some empirical evidence that serves as an initial reference point for the analysis that follows in the next section of the paper.

3.1. The Model

The empirical analysis in the remainder of the paper is based on a simple model of aggregate production. Let aggregate private-sector output, Y_t, be determined by the production technology

$$Y_t = A_t f(N_t, K_t), \tag{1}$$

where N_t represents aggregate employment of labor services and K_t represents the stock of private capital. A_t is an index of total factor productivity or Hicks-neutral technical change which is assumed to be a function of aggregate shocks, Z_t, and government regulation, R_t; i.e.,

$$A_t = A(Z_t, R_t). \tag{2}$$

By assuming a generalized Cobb-Douglas form for the aggregate production function and taking logarithms, (1) can be rewritten as

$$y_t = a_t + e_N n_t + e_K k_t, \tag{3}$$

where lower-case letters denote logarithms of their upper-case counterparts and e_i represents the elasticity of output with respect to factor $i = N, K$. If the technology (1) exhibits constant returns to scale over the inputs N_t and K_t, then $e_N + e_K = 1$.[12] Under this assumption, (3) can be written

[12]The constant returns to scale assumption is consistent with the data used in the analysis below. OLS estimation of (3) with a correction for first-order serial correlation provides an F-statistic of 1.21 (p-value=0.2772) for the null hypothesis that $e_N + e_K = 1$.

as

$$y_t - k_t = a_t + e_N(n_t - k_t). \tag{4}$$

If we make explicit reference to the factors affecting productivity as described in (2), we can derive a specification that is useful for testing the effects of government regulation on aggregate economic activity. This specification is taken from (4) under the assumption that the implicit function (2) can be expressed as a log-linear relation of the determinants of total factor productivity; viz.

$$y_t - k_t = \alpha + \beta z_t + \theta(n_t - k_t) + \sum_{j=0}^{J} \phi_j (r_{t-j} - k_{t-j}) + \epsilon_t, \tag{5}$$

where ϵ_t is a disturbance term. The regulation measure enters the equation as the regulation-to-capital ratio to maintain consistency with the other level variables in the model. We include up to J lags of the regulation measure, as regulatory change may affect economic activity over an extended period of time.

The empirical analysis in the subsection below utilizes annual data from the U.S. over the period 1949 to 1999.[13] Data on private business output (y), hours of labor services (n), and private capital services (k) are prepared by the U.S. Department of Labor and reported in the *Monthly Labor Review*.[14] The capacity utilization rate in the manufacturing sector of the economy (cu), published in the *Federal Reserve Bulletin*, is used as a proxy for z. Finally, the measure of regulatory activity discussed in Section 2 is employed for the variable r.

3.2. Preliminary Empirical Results

This subsection provides a summary analysis of the relationship between regulation and macroeconomic performance. We begin with standard regression estimates of (5) above, followed by bivariate Granger-causality tests of regulation versus several basic macroeconomic

[13]Starting the sample in 1949 adheres to the standard practice of excluding the World War II period from the analysis, and also discards the period 1938-1949 during which there were no revisions in the *CFR*.

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

variables.

Table 1 presents estimates of the specification in (5). Five lags of the regulation variable are included to capture the adjustment of economic activity to changes in regulation (i.e., J=5 in equation (5) above). Estimation of the model includes a correction for first-order autocorrelation in the error process. The reported estimates include the sum of current and lagged coefficients on the regulation variable along with F-statistics for significance of the sum. The first row of the table uses an aggregate measure of regulation (i.e., total pages in the *CFR*), while the remaining rows consider specific areas of regulation (pages in the individual titles of the *CFR*). The results in row one suggest a strong negative relationship between output per unit of capital and the aggregate regulation-capital ratio.[15] A one percentage point increase in the regulation-capital ratio is associated with a combined 0.24 percentage point reduction in the productivity of capital over the five-year adjustment period considered here.

Turning now to the individual areas of regulation, the results in Table 1 suggest that some areas of regulation are important and some are not. The current and lagged impact of regulation in Titles 15 (commerce), 20 (employee benefits), 22 (foreign relations), 24 (housing credit), 29 (labor), 30 (mineral resources), 37 (patents and copyrights), 42 (public health), 49 (transportation), and 50 (wildlife and fisheries) are found to be significantly related to output per unit of capital. For each of these areas, the estimated impact is negative. The estimated size of the impact differs across these areas, ranging from a 0.05 percentage point decrease to a 0.12 percentage point decrease in capital productivity for each one percentage point increase in the regulation-capital ratio. As might be expected, each of these individual effects is estimated to be smaller than the 0.24 percentage point effect estimated for the aggregate measure of regulation in row one of the table. Looking across the list of areas that are found to be significantly related to economic activity, there is no rationale for explaining why these particular areas of regulation are significant and others are not. It is easy, for example, to imagine that labor regulations or transportation regulations negatively impact output. But, it is also easy to imagine that many of the areas not found to be significant could be related to

[15]Although not reported in Table 1, the estimated coefficients on the other independent variables in the model are generally statistically significant and of the expected sign across all equations reported in the table.

growth, either positively or negatively. Ultimately, determining which areas of regulation are significantly related to growth is simply an empirical issue.

Our next set of summary results on the relationship between regulation and economic activity is based on Granger-causality tests. These tests were popularized by the work of Granger (1969) and Sims (1972). The basic idea of 'Granger' causality is to test whether lagged values of a particular variable significantly affects the contemporaneous value of another variable. The test is carried out by estimating the equation

$$Y_t \ = \ \alpha_0 + \sum_{i=1}^{q} \alpha_i Y_{t-i} + \sum_{i=1}^{q} \beta_t X_{t-i} + \epsilon_t, \tag{6}$$

and testing whether the group of coefficients β_1, β_2,...,β_q are significantly different from zero. If they are, then we can reject the hypothesis that "*X* does not cause *Y*." Then, we can test the hypothesis "*Y* does not cause *X*" by running the same regression as above, but switching *X* and *Y* and testing whether lagged values of *Y* are significantly different from zero. To conclude that *X* causes *Y*, we must reject the hypothesis "*X* does not cause *Y*" and not reject the hypothesis "*Y* does not cause *X*." If both hypotheses are rejected, we conclude that *X* and *Y* are both endogenously determined.

We should note at the outset that the concept of 'Granger causality' is not equivalent to the notion of causation in the traditional sense of the word. Indeed, no econometric test can prove causation. Granger causality may best be thought of as a test of 'firstness' rather than causation, so that if *X* Granger-causes *Y* then we have evidence that *X* precedes *Y*. However, evidence in favor of Granger-causality is certainly supportive of the notion that *X* causes *Y* in the traditional sense. In the discussion that follows, the use of the word 'cause' is understood to imply 'Granger' causality rather than some stronger sense of the word.[16]

Turning to the analysis, we examine the role of regulation using a set of six variables taken to represent macroeconomic activity. The variables include real output, unemployment, physical capital input, labor input, real hourly wages, and an implicit price deflator. Output (*y*),

---

[16]For an example and further discussion of the use of Granger-causality tests in practice, see Hamilton (1983). For a critical examination of Granger-Sims causality tests, see Jacobi, Leamer, and Ward (1979) and Feige and Pearce (1979). Also, for a more general and recent discussion of causality, see Granger (1988) and Zellner (1988).

labor (n), and capital (k) data are the same as those used in the analysis above. The implicit price deflator (p) is for private business output (y), and also comes from the Department of Labor's *Monthly Labor Review*. The unemployment rate (U) is taken from the Bureau of Labor Statistics, and the real wage (w) comes from the St. Louis Fed. The analysis uses the natural logarithm of all variables except for U. Standard Dickey-Fuller tests indicate the presence of unit roots in the levels of all variables except U. This nonstationarity is treated by first-differencing until the series is rendered stationary. The results of bivariate Granger-causality tests between regulation and each of the six variables are reported in Table 2 for the period 1950-1999. Statistics for $X \nrightarrow Y$ represent an F-test of $\beta_1 = ... = \beta_q = 0$ in an OLS estimation of (6). The variable r represents the natural logarithm of the aggregate regulation series discussed above. For all tests, the lag length q is 4, but the results are generally not sensitive to the choice of q.

The results in Table 2 suggest that the aggregate level of regulation Granger-causes several of the macroeconomic variables under consideration. These include output, the unemployment rate, and real wages. Interestingly, the results also suggest that causation runs in the opposite direction with regard to physical capital; that is, changes in capital input cause changes in regulation. Results for labor input and the implicit price deflator suggest no causal relationship. Although the results are not reported, when Granger-causality tests are extended to the individual titles of the *CFR*, the results suggest that few of the individual areas of regulation Granger-cause the macroeconomic variables considered here. Still, the results in Table 2 are at least suggestive that regulation is related to various aspects of aggregate economic activity in a causal sense.

We close this section by noting that the perspective of modern dynamic economics questions the validity of estimations such as those presented in Tables 1 and 2. First, the levels of aggregate time series are often found to be nonstationary, possibly making them unsuitable for use in standard regression analysis. Second, there are questions about the appropriate estimation techniques given the possible presence of aggregation and simultaneity bias. Despite the valid criticisms, these results establish an initial reference point in estimating the relationship between regulation and economic performance. In the next section, we turn to more modern statistical techniques to estimate this relationship.

4. Regulation and the Economy: Long-Run Trends and Short-Run Dynamics

This section employs updated statistical techniques to investigate the relationship between regulation and aggregate economic activity. We begin by estimating the long-run relationship between the variables in equation (5). With these estimates in hand, we move on to analyze the response, over time, of economic activity to a shock in regulation.

4.1. Estimating Long-Run Trends

The empirical procedure used in the previous section provides a descriptive summary of the relationship between macroeconomic performance and regulation. Those results are useful because they provide a basis for comparison with other empirical studies of the macroeconomy, and are based on empirical practices that are still in wide use today. Nevertheless, econometric theory exposes a number of pitfalls with this traditional approach. One problem concerns the failure to account for the time-series properties of the variables used in the analysis. Each of these variables is likely to contain a stochastic trend, and conventional techniques do not take into account the implications of this type of nonstationarity. A second problem pertains to endogeneity bias; that is, the possibility of reverse causation in the relationship between regulation and economic activity. Indeed, the results in Table 2 suggest that changes in the capital stock Granger-cause changes in regulation. Failure to address either of these problems could skew statistical inference, resulting in inconsistent estimates of how a change in regulation affects the economy. We now present an alternative approach, based on the theory of cointegration, which can address both difficulties.

We illustrate using the model described above, which provides the empirical specification given in equation (5). However, the empirical approach used below is not conditional on any particular theory and is robust to a variety of departures from the framework presented above. We repeat equation (5) here for convenience:

$$y_t - k_t = \alpha + \beta z_t + \theta(n_t - k_t) + \sum_{j=0}^{J} \phi_j (r_{t-j} - k_{t-j}) + \epsilon_t. \tag{7}$$

Our goal is to estimate the parameters α_i. The appropriate estimation technique must account for the time-series properties of the variables in (7). Dickey-Fuller tests indicate the null hypothesis of a unit root cannot be rejected for the measures ($y-k$), ($n-k$), and ($r-k$); the test statistics are -2.92, -1.23, and -3.14, respectively, compared to 5% critical values of -3.51, -4.15, and -3.51.[17] By contrast, unit-root tests indicate that the first differences of these variables are stationary; the test statistics are -5.51, -5.60, and -3.59, respectively, compared to 5% critical values of -1.95, -3.57, and -1.95. In other words, the variables in (7) are said to be first-order integrated, or $I(1)$. If the error term ϵ in (7), on the other hand, is stationary, or $I(0)$, then the variables ($y-k$), ($n-k$), and ($r-k$) are said to be cointegrated. In other words, the variables in (7) are individually trending, but they share a common trend while deviating from each other in the short run. Intuitively, we expect this result; otherwise, the variables ($y-k$), ($n-k$), and ($r-k$) would be found to drift unrealistically away from one another. Indeed, the results of Johansen tests reported in Table 3 support the hypothesis that these variables are cointegrated, which suggests that the error term ϵ is in fact stationary.

The finding of cointegration among the variables in (7) is important for several reasons. First, notice that the error term ϵ in (7) will typically be both serially correlated and correlated with the regressors ($n-k$) and ($r-k$). While serial correlation is straightforward to address in conventional econometric techniques, correlation between the error term and the regressors leads to inconsistent parameter estimates. By contrast, OLS estimation of the cointegrating parameters–or cointegrating vector–are robust to the presence of this type of correlation. This property results because ϵ is stationary while the regressors are individually nonstationary. There may be some transitory correlation between the error term and the regressors, but the long-run correlation must be zero since trending variables must eventually diverge from stationary ones. Thus, we can obtain accurate estimates of long-run relationship present in equation (7) using single-equation techniques.

A second desirable property that results from similar reasoning is that the estimation of cointegrated systems is robust to a wide range of underlying theoretical models. Consistent estimates of the parameters in (7) can be obtained even if there are omitted explanatory variables that are correlated with the regressors in (7). In other words, as long as the variables

---

[17]The aggregate measure of regulation is used for the variable r in this test and throughout the following discussion.

in (7) are cointegrated, we can consistently estimate the parameters of that long-run relationship. We now turn to the empirical procedure for estimating such a relationship.

The logic of the empirical procedure discussed above requires the presence of a single cointegrating vector linking ($y-k$), ($n-k$), and ($r-k$). The results of the Johansen "Trace" test reported in Table 3 suggest that the hypothesis of a single cointegrating vector is in fact consistent with the data. Therefore, we can proceed with the estimation of the cointegrating vector. As noted above, standard OLS estimation will produce consistent estimates of the cointegrating vector. However, statistical inference cannot be carried out using conventional standard errors; some correction is necessary. We use the dynamic OLS (DOLS) procedure of Stock and Watson (1993), as described in Hamilton (1994, p. 608). This procedure specifies a single equation of the form

$$y_t - k_t = \alpha + \pi(n_t - k_t) + \gamma(r_t - k_t) + \sum_{i=-k}^{k} \pi_i \Delta(n_{t+i} - k_{t+i}) + \sum_{i=-k}^{k} \gamma_i \Delta(r_{t+i} - k_{t+i}) + \epsilon_t^*, \tag{8}$$

where Δ is the first-difference operator and ϵ_t^* is related to ϵ_t such that $\epsilon_t^* = \epsilon_t - \sum_{i=-k}^{k} \pi_i \Delta (n_{t+i} - k_{t+i}) - \sum_{i=-k}^{k} \gamma_i \Delta (r_{t+i} - k_{t+i})$. Equation (8) is estimated by OLS, but leads and lags of the first difference of the right-hand-side variables are included to eliminate the effects of regressor endogeneity on the distribution of the OLS estimator. A non-parametric correction for serial correlation is also required for the *t*-statistics; see Hamilton for details. This procedure provides consistent estimates of the cointegrating vector $\{1, -\pi, -\gamma\}$ and the corrected *t*-statistics can be compared to standard *t*-tables.

Equation (8) appears, at first glance, to be very similar to the equation (5) estimated in the previous section. There are, however, some noteworthy differences. Unlike equation (5), equation (8) contains leads and lags of the first differences of all right-hand-side variables. Equation (5) includes lags of the level of the regulation variable only. Thus, the estimates of the regulation parameter from equation (5) are the sum of the coefficients on the current and lagged levels of regulation, in order to capture the long-run impact of regulation when there are adjustment lags. Likewise, equation (5) includes the capacity utilization rate as an explanatory variable to proxy for aggregate shocks z, in order to account for short-run economic fluctuations around the trend relationship. By contrast, the estimate of γ in equation (8) is only the coefficient on the current level of regulation, and leads and lags are included simply to eliminate the effects of regressor endogeneity on the distribution of the least squares estimator. Intuitively, equation (8) is specified to estimate only the cointegrating relationship linking $(y-k)$, $(n-k)$, and $(r-k)$ in the long run. By contrast, equation (5), as estimated in Table 1, implicitly models both the long-run parameters and the adjustment of the economy to changes in regulation over the short run. It is reasonable to suppose that a procedure–such as the estimation of equation (8)–that separates these two steps will provide more accurate estimates of the long-run trend relationship.

To facilitate comparison with previous results, the DOLS estimates of γ from equation (8) are reported alongside the estimated regulation parameters from equation (5) in Table 1. The DOLS estimation uses k=3.[18] The corrected t-statistics are also reported. In the first row of the table, the aggregate measure of regulation is used. The results suggest a negative relationship between regulation and output per unit of capital over the long run. The size of the impact is estimated to be slightly larger than in the analysis of the previous section–a one percentage point increase in the regulation-capital ratio causes nearly a 0.27 percentage point decrease in output per unit of capital, compared to a 0.24 percentage point decrease estimated in the previous section.

In the remaining rows of Table 1, the individual titles of regulation are used in the analysis. Of the 32 areas of regulation considered, 22 are found to have a statistically significant long-run impact on aggregate economic activity.[19] Only 10 such areas were found in the previous section. Additional areas of regulation found to be important using the present analysis include titles 13 (business credit), 17 (commodity and securities exchange), 18 (conservation of power), 19 (customs duties), 21 (food and drugs), 23 (highways), 26 (internal revenue), 28 (judicial administration), 36 (parks and forests), 38 (pensions, bonuses, and veterans relief), 41 (public contracts), and 43 (public lands). In each case where a title is found to be statistically significant in both analyses, the size of the estimated impact is larger using

[18]The results are generally not sensitive to choosing different values for k.

[19]Cointegration tests analogous to those in Table 3 indicate the presence of a single cointegrating relationship among $(y\text{-}k)$, $(n\text{-}k)$, and the individual titles of regulation, with the possible exception of titles 15, 22, 31,33, 38, and 46. For these titles, interpretation of the DOLS results is somewhat tenuous.

the cointegration analysis. Only one title is found to be significant in the previous section, but not in the present analysis–title 29 (labor). Interestingly, several areas of regulation are estimated to have a positive effect on the economy. These include titles 13, 18, 21, 26, 28, and 41. It is certainly reasonable to suppose that regulations relating to the judicial system (title 28) might have such an effect, insofar as these regulations promote the enforcement of property rights and contracts. Admittedly, it is more difficult to explain how regulations relating to internal revenue (title 26) might have a positive effect. The largest positive impact is associated with title 26, with a 0.75 percentage point impact. The largest negative impact is associated with title 19, with a −0.34 percentage point impact. Neither of these titles was found to be significantly related to economic activity in the previous section.

In summary, the cointegration analysis confirms most of the results from the previous analysis regarding the impact of regulation on the economy. This analysis suggests an even larger impact for those areas of regulation found previously to be significant, and also suggests that some additional areas of regulation are significantly related to economic performance–some positive and some negative. Taken together, the results suggest an important role for regulation in the economy, both at an overall level and within specific areas of regulation.

4.2. Estimating Short-Run Dynamics

We specify a model of short-run dynamics that imposes the long-run relationship estimated above, while also allowing for temporary divergences from this trend. The model takes the form

$$\Delta \mathbf{x}_t = \mu + \delta\left[(y_{t-1} - k_{t-1}) - \hat{\pi}(n_{t-1} - k_{t-1}) - \hat{\gamma}(r_{t-1} - k_{t-1})\right] + \sum_{j=1}^{k} \mathbf{\Phi}_j \Delta \mathbf{x}_{t-j} + e_t, \tag{9}$$

where $\Delta\mathbf{x}$ is the vector of first differences $\{\Delta(y-k), \Delta(n-k), \Delta(r-k)\}'$. The parameters $\hat{\pi}$ and $\hat{\gamma}$ are the previously estimated cointegrating coefficients for $(y-k)$, $(n-k)$, and $(r-k)$. The parameters μ, δ, and Φ govern the short-run dynamics. This restricted vector autoregression (VAR) specification is referred to as the error-correction representation of the system. For any set of cointegrated variables, the error-correction representation is the appropriate VAR for describing the short-run dynamics among the variables in that set.

To examine the dynamic response of output to a shock in the regulation variable, a k=2 version of the error-correction model (9) is estimated over the period 1949-1999.[20] Note that the cointegrating vector obtained from the DOLS analysis above is imposed in the estimation of (9). Rather than report the individual parameter estimates, it is customary to study short-run dynamics using the impulse response functions and variance decompositions of the model variables. Figure 3 shows the matrix of impulse response functions when the aggregate measure of regulation is used in the analysis. The two-standard-deviation error bands are also shown for the responses. These graphs can be used to determine the length of time over which a change in regulation typically affects aggregate economic activity. The lower left graph in the figure shows the response of output per unit of capital to a one-standard-deviation shock in the regulation variable. Over the first two years following the shock, the change in regulation has virtually no effect on the economy; the standard-error bands are initially wide enough that the response cannot be considered more than noise. By contrast, over a horizon of 2-11 years, there is a statistically significant negative impact on output.

Another perspective on the economy's dynamic response to a regulation shock can be gained using variance decompositions. The variance decompositions for $(y-k)$ are reported in Table 4. These variance decompositions provide the percentage of the j-year ahead mean-squared forecast error in $(y-k)$ due to innovations in the other model variables. The more interesting information is found at longer horizons, where the interaction among the model variables have sufficient time to become felt. Table 4 reports that the importance of regulation in explaining the variation in output increases over time. At five years out, nearly 20% of the variation in $(y-k)$ is attributable to regulation; at a horizon of 15 years, more than half of the variation in $(y-k)$ is explained by the regulation variable.

The error-correction model can also be estimated using the individual areas of regulation. Although the results are not reported, the findings generally suggest that specific areas of regulation do not have important short-run effects on the economy. Examination of variance decompositions for individual areas of regulation suggests that some areas may be important in explaining aggregate economic behavior in the short run. These include titles 15 (19% at a

[20]The results are not sensitive to different values of k.

15 year horizon), 17 (19%), 20 (22%), 29 (17%), 31 (18%), 33 (28%), 42 (39%), and 47 (30%). We interpret these result as suggesting that these areas of regulation are most closely related to the economy's short-run behavior. It may also be that these specific areas of regulation do not have a large enough impact to be felt at the aggregate level, thus providing the statistically negligible effects from the impulse response analysis. However, it may be reasonable to suppose that these areas of regulation have an important effect on the particular sectors of the economy where their impact would be most evident. Ultimately, of course, this is an empirical issue which we do not address here.

5. Conclusion

This paper introduces a time series which consistently measures federal regulatory activity in the U.S. since 1938. The measure is the number of pages in the *Code of Federal Regulations*. Although counting pages of regulation has its obvious shortcomings, the consistent codification of regulations over the years provides a unique source of information on regulatory activity. The *CFR*-based measures exhibit growth in regulatory activity during the 1940s and 1950s. This growth is shown to moderate somewhat during the 1960s, but the 1970s display the most rapid growth in pages of regulation since the inception of the *CFR*. Growth in the regulatory series slows during the early 1980s but returns to near its 1970s level by the end of the 1980s. The most significant decline in pages of regulation occurs during the early 1990s. This behavior is generally consistent with many preconceived notions regarding the history of regulatory activity in the U.S.

We also investigate the empirical relationship between regulatory activity and macroeconomic performance. A simple model of aggregate production which describes how regulation affects output in the economy is used. The model is estimated using the *CFR*-based regulatory measures. Preliminary empirical evidence based on simple regression techniques and Granger-causality tests indicate that regulation–both aggregate measures of regulation as well as some specific areas of regulation–may be significantly related to several key macroeconomic variables. When more advanced statistical techniques based on cointegration analysis is used, evidence of a long-run trend (cointegrating) relationship between output, capital, labor, and regulation is found. The empirical results indicate that regulatory activity has significantly negative impact on aggregate economic performance in the U.S.

The finding that regulation is important in determining long-run aggregate economic outcomes is further supported by an analysis of area-specific regulations. The evidence suggests that 22 out of 32 areas of regulation have significant long-run effects, some negative and some positive.

Estimation of an error-correction model, which takes the cointegrating relationship between regulation and the other model variables as given, allows an analysis of the time horizon over which regulation affects economic performance. Impulse response analysis indicates that a shock in the overall level of regulation negatively impacts economic activity over a horizon of 2-11 years. Variance decompositions predict that regulation accounts for over half of the forecast error in output at a horizon of 15 years. The impact of area-specific regulations seems less noticeable on the aggregate economy, although variance decompositions suggest some areas may be related to short-run economic behavior.

Many benefits of regulation may not be measured in economic terms. Thus, finding a negative economic effect of regulation should not be taken to mean that regulation imposes a net welfare cost on society. Such a finding does establish, however, a standard which the benefits of regulation must exceed in order for it to pass the usual cost-benefit analysis.

References

Dawson, J.W., 2000, "Measuring Federal Regulatory Activity in the U.S.: 1938-1994," Working Paper, Appalachian State University.

Feige, E.L., and D.K. Pearce, 1979, "The Causal Relationship Between Money and Income: Some Caveats for Time Series Analysis," *Review of Economics and Statistics* 61, 521-233.

Granger, C.W.J., 1988, "Some Recent Developments in a Concept of Causality," *Journal of Econometrics* 39, 199-211.

_____, 1969, "Investigating Causal Relations by Econometric Models and Spectral Methods," *Econometrica* 37, 424-438.

Hahn, R.W. and J.A. Hird, 1991, "The Costs and Benefits of Regulation: Review and Synthesis," *Yale Journal on Regulation* 8, 233-78.

Hamilton, J.D., 1994, *Time Series Analysis* (Princeton, NJ: Princeton University Press).

_____, 1983, “Oil and the Macroeconomy since World War II,” *Journal of Political Economy* 91, 228-248.

Hazilla, M. and R.J. Kopp, 1990, "Social Cost of Environmental Quality Regulations: A General Equilibrium Analysis," *Journal of Political Economy* 98, 853-73.

Hopkins, T.D., 1991, *Cost of Regulation* (Rochester, NY: Rochester Institute of Technology).

Jacobi, R.L., E.E. Leamer, and M.P. Ward, 1979, “The Difficulties with Testing for Causation, *Economic Inquiry* 17, 401-413.

Litan, R.E. and W.D. Nordhaus, 1983, *Reforming Federal Regulation* (New Haven, CT: Yale University Press).

Sims, C.A., 1972, “Money, Income, and Causality,” *American Economic Review* 62, 540-552.

Stock, J.H. and M. Watson, 1993, “A Simple Estimator of Cointegrating Vectors in Higher Order Integrated Systems,” *Econometrica* 61, 783-820.

U.S. Environmental Protection Agency, 1990, "Environmental Investments: The Cost of a Clean Environment" (Washington, DC: U.S. Environmental Protection Agency).

_____, 1984, "Final Report: The Cost of Clean Air and Water: Report to Congress" (Washington, DC: U.S. Environmental Protection Agency).

Zellner, A., 1988, “Causality and Causal Laws in Economics, *Journal of Econometrics* 39, 7-21.

Table 1
OLS Estimation of Equations (5) and (8)
1949-1999

	Equation (5)		Equation (8)	
Regulation Measure	$\sum_{j=0}^{J}\phi_j$	*F*-test	$\hat{\gamma}$	*t*-test
Complete *CFR*	−0.2392	7.52**	−0.2680	−2.08**
Title 7: Agriculture	0.0365	0.12	0.0906	1.40
Title 8: Aliens and Citizenship	0.0107	0.06	−0.1096	−1.40
Title 12: Banks and Banking	−0.0661	0.64	−0.0035	−0.05
Title 13: Business Credit	0.0149	0.50	0.0894	3.88**
Title 15: Commerce	−0.0539	3.10*	−0.1149	−3.82**
Title 16: Commercial Practices	−0.0190	0.07	0.0160	0.13
Title 17: Commodity and Securities Exchange	−0.0571	1.03	−0.2181	−1.75*
Title 18: Conservation of Power	0.0021	0.002	0.1486	1.77*
Title 19: Customs Duties	−0.0832	1.48	−0.3444	−2.93**
Title 20: Employee's Benefits	−0.1230	3.12*	−0.2536	−2.35**
Title 21: Food and Drugs	−0.0742	0.80	0.1623	2.04**
Title 22: Foreign Relations	−0.1175	4.57**	−0.3326	−7.19***
Title 23: Highways	−0.0061	0.18	−0.0419	−2.24**
Title 24: Housing Credit	−0.0656	34.53***	−0.0897	−12.24***
Title 26: Internal Revenue	0.0179	0.04	0.7489	13.59***
Title 28: Judicial Administration	−0.0378	0.93	0.2115	2.95**
Title 29: Labor	−0.0847	3.18*	−0.0314	−0.30
Title 30: Mineral Resources	−0.1068	8.62**	−0.2433	−5.01***
Title 31: Money and Finance: Treasury	−0.0498	1.97	−0.0758	−0.71
Title 33: Navigation and Navigable Waters	−0.1024	2.72	−0.1026	−0.83
Title 36: Parks and Forests	−0.0593	1.82	−0.2516	−5.60***
Title 37: Patents and Copyrights	−0.0788	2.92*	−0.3114	−7.57***
Title 38: Pensions, Bonuses, and Veteran's Relief	−0.0735	1.29	−0.3416	−4.54***
Title 40: Protection of the Environment	0.0388	0.90	−0.0131	−1.15
Title 41: Public Contracts	0.0072	0.35	0.0237	1.70
Title 42: Public Health	−0.0531	4.54**	−0.1530	−3.10**

Title 43: Public Lands: Interior	0.0129	0.07	−0.2106	−2.80**
Title 45: Public Welfare	−0.0111	0.21	0.0431	1.19
Title 46: Shipping	0.0157	0.04	0.0766	0.82
Title 47: Telecommunications	−0.0427	0.35	−0.1162	−0.91
Title 49: Transportation and Railroads	−0.1007	3.55*	−0.1702	−1.79*
Title 50: Wildlife and Fisheries	−0.0713	13.45***	−0.1568	−14.63***

Notes: *F*-test is the *F*-statistic for the null hypothesis that the sum of current and lagged coefficients on the regulation variable in equation (5) equals zero. J=5 in the estimation of (5); estimates include a correction for first-order autocorrelation in the error process. *t*-test is the *t*-statistic for the null hypothesis that the parameter γ in equation (8) equals zero. k=3 in the estimation of (8); *t*-statistics include a non-parametric correction for serial correlation. The estimation for Title 40 is limited to 22 observations because environmental regulations were not assigned to Title 40 until 1971. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels, respectively.

Table 2
Bivariate Granger-Causality Tests
1950-1999

Null Hypothesis	q	N	$F(q,N\text{-}9)$	p-value
$\Delta r \nrightarrow \Delta y$	4	46	2.43	0.0652
$\Delta y \nrightarrow \Delta r$	4	46	0.30	0.8763
$\Delta r \nrightarrow U$	4	46	2.61	0.0511
$U \nrightarrow \Delta r$	4	46	1.89	0.1324
$\Delta r \nrightarrow \Delta^2 w$	4	46	3.03	0.0293
$\Delta^2 w \nrightarrow \Delta r$	4	46	0.63	0.6419
$\Delta r \nrightarrow \Delta n$	4	46	0.97	0.4372
$\Delta n \nrightarrow \Delta r$	4	46	1.96	0.1206
$\Delta r \nrightarrow \Delta^2 k$	4	46	1.66	0.1812
$\Delta^2 k \nrightarrow \Delta r$	4	46	2.77	0.0412
$\Delta r \nrightarrow \Delta^2 p$	4	46	0.83	0.5151
$\Delta^2 p \nrightarrow \Delta r$	4	46	0.79	0.5394

Notes: See variable definitions in the text. Δ is the first difference operator; q is the number of lagged variables in the estimated equation; N is the number of observations.

Table 3
Johansen Cointegration Tests
($y-k$), ($n-k$), and ($r-k$)

Lags in VAR model: k=1

	λ-Max		Trace	
H_0: h=	Test Statistic	90% C.V.	Test Statistic	90% C.V.
0	44.98	13.39	53.70	26.70
1	8.72	10.60	8.72	13.31
2	0.004	2.71	0.004	2.71

Lags in VAR model: k=2

	λ-Max		Trace	
H_0: h=	Test Statistic	90% C.V.	Test Statistic	90% C.V.
0	27.29	13.39	37.01	26.70
1	8.13	10.60	9.72	13.31
2	1.59	2.71	1.59	2.71

Lags in VAR model: k=3

	λ-Max		Trace	
H_0: h=	Test Statistic	90% C.V.	Test Statistic	90% C.V.
0	20.15	13.39	31.66	26.70
1	9.71	10.60	11.51	13.31
2	1.80	2.71	1.80	2.71

Lags in VAR model: k=4

	λ-Max		Trace	
H_0: h=	Test Statistic	90% C.V.	Test Statistic	90% C.V.
0	18.23	13.39	29.88	26.70
1	8.27	10.60	11.65	13.31
2	3.38	2.71	3.38	2.71

Notes: The cointegration tests assume a p-dimensional VAR model with k lags, where p is the number of stochastic variables among which the investigator wishes to test for cointegration. The λ-max statistic tests the null hypothesis of h cointegrating relationships against the alternative of h+1 cointegrating relationships. The trace statistic tests the null hypothesis of h cointegrating relationships against the alternative of p=3 cointegrating relationships. The test assumes a linear trend in the data and a constant in the cointegrating relationship. The results also hold under the assumption of no trend in the data and a constant in the coitegrating relationship.

Table 4
Variance Decompositions for (y−k)

		Percentage of Variance Attributable to:		
Horizon j=	Standard Error	$(y-k)$	$(n-k)$	$(r-k)$
1	0.0242	100	0	0
2	0.0316	96.6	2.9	0.5
3	0.0358	86.3	8.0	5.6
4	0.0395	77.2	10.4	12.4
5	0.0430	70.4	10.1	19.6
6	0.0463	64.7	9.0	26.4
7	0.0493	59.3	7.9	32.8
8	0.0523	54.3	7.2	38.6
9	0.0551	49.7	6.9	43.3
10	0.0579	45.7	7.2	47.1
11	0.0606	42.0	7.9	50.1
12	0.0633	38.7	9.0	52.3
13	0.0658	35.9	10.2	53.9
14	0.0684	33.3	11.6	55.1
15	0.0708	31.1	13.0	55.9

Notes: Variance decompositions describe the percentage of the forecast error in $(y-k)$ due to other variables in the model during the last j years. Results obtained from the estimation of the error-correction model (9) using k=2 lags and the aggregate measure of regulation over the period 1949-1999. Percentages may not add to 100 due to rounding.

Figure 1: Federal Regulation in the U.S., 1949-2000

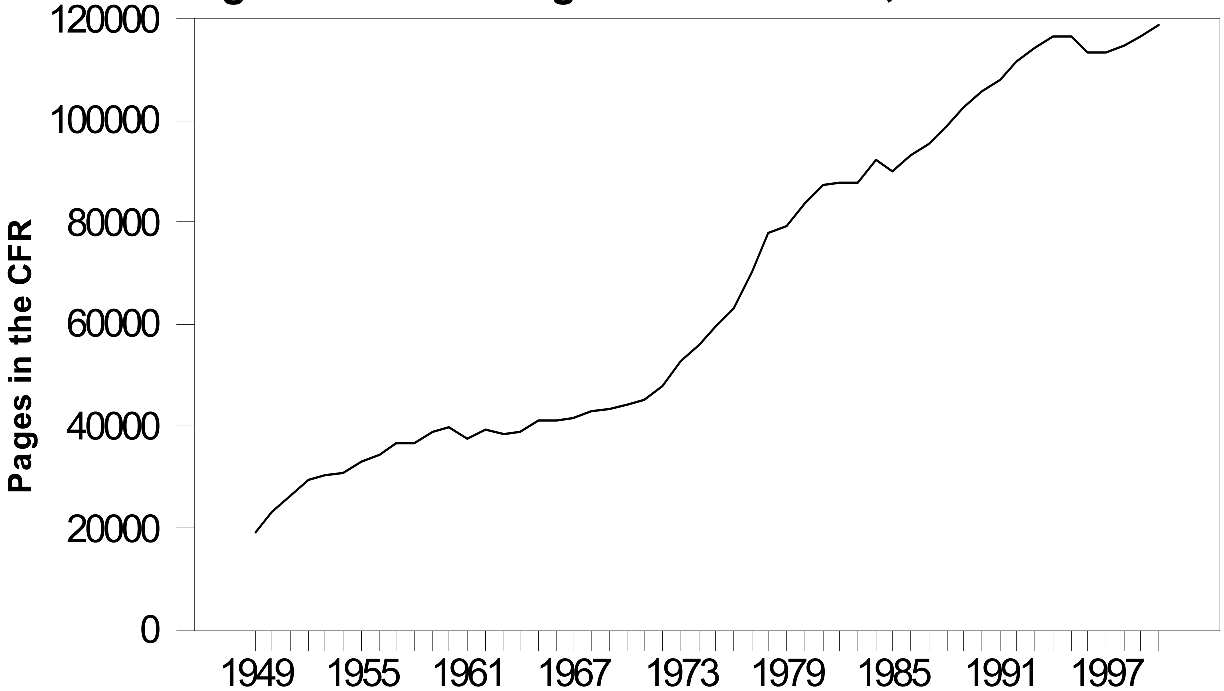

Figure 2: Regulation in Selected Areas, 1970-2000

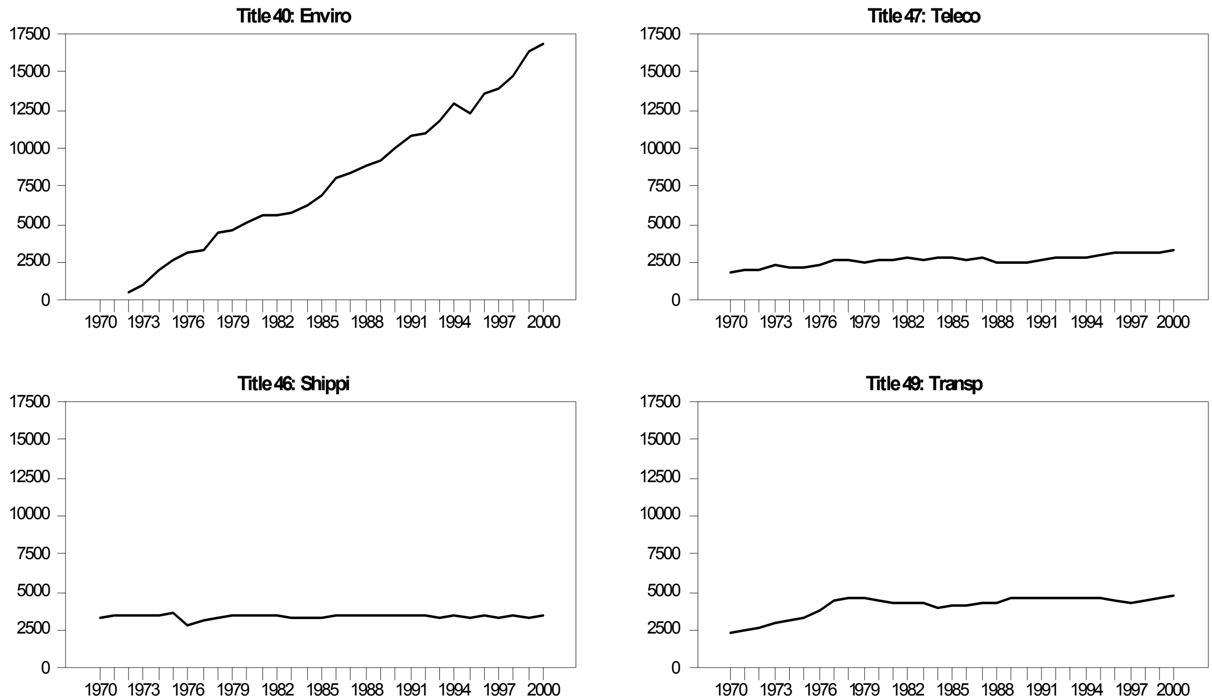

Figure 3: Impulse Responses