

Regressive effects of regulation on wages

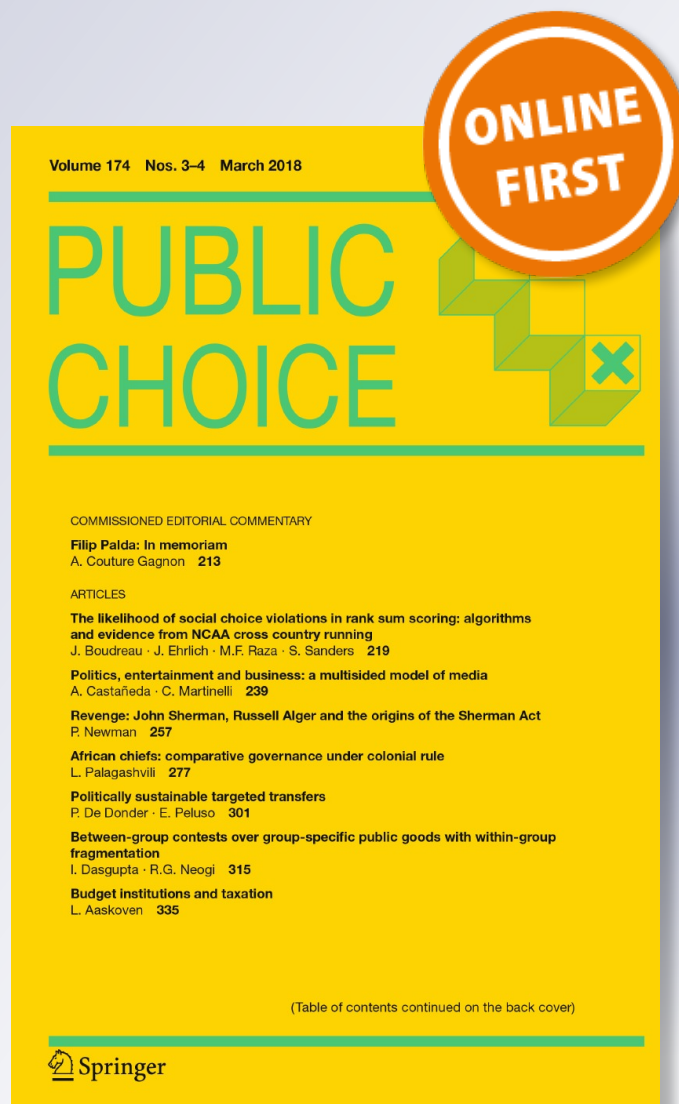
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Regressive effects of regulation on wages

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Abstract A growing body of literature analyzing the distributive consequences of regulation suggests that regulation may have particularly detrimental effects on lower-income households. Regulation can be regressive if it represents the preferences of the wealthy while imposing costs on all households. The specific channel through which regulation may impose costs on lower-income households is its effects on prices and wages. In this issue, Chambers et al. (Public Choice. <https://doi.org/10.1007/s11127-017-0479-z>, 2017) investigate the impact of regulation on prices. They find that regulation raises consumer prices; regulatory interventions therefore are regressive because lower income consumers tend to spend larger percentages of their budgets on regulated goods and services. In this paper, we seek to analyze the effect of regulation on wages across different income levels and occupations.

Keywords Regulation · Wages · Inequality · Employment

JEL Classification K23 · L51

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1 Introduction

Regulatory intervention in the economy has expanded dramatically since the 1970s. For people living in advanced economies, few aspects of life remain unregulated. Al-Ubaydli and McLaughlin (2015) show that just between 1997 and 2012, the regulatory burden (as measured by the *Code of Federal Regulations*) has risen by more than 28% to a staggering number in excess of one million individual restrictions. While regulation theoretically can benefit consumers and employees (see Pigou 1920; Akerlof 1970), the public choice literature, building on Tullock (1967) and Stigler (1971), has long questioned the benevolent intentions of government intervention. This economic theory of regulation suggests that regulation, more often than not, benefits producers at the expense of consumers and the public at large. Recent contributions to the literature suggest that an additional negative, unintended consequence of regulation may be that it aggravates and reinforces unequal distributions of income and employment in the economy, especially when the rules in question represent the preferences of wealthy elites, but come at the expense of all households or lower-income households in particular (Stigler 1971; Baumol and Oates 1988; Thomas 2012; Hoffer et al. 2017; Bailey and Thomas 2017; Jones and Cullis 1986; Thomas 2017; Moore 1978; Chambers et al. 2017, 2018). Regressivity emerges whenever regulation is designed to achieve an outcome higher-income households desire, such as certain environmental quality or safety standards, while at the same time resulting in production- and compliance-cost increases passed on to all consumers, in the form of higher prices, or workers, in the form of lower wages. Take, for example, the 2014 rearview camera mandate: Starting in 2018, the National Highway Traffic Safety Administration will require automakers to install rearview cameras in all passenger cars. That feature formerly was limited to luxury models, suggesting that the demand for such camera was confined to higher-income households. It is expected to reduce back-over accidents by 32% from a current risk of 1 in every 1.5 million people to roughly 1 in every 2.2 million people (see Thomas 2012). While the precise effect of the mandate on the number of accidents remains difficult to estimate ex ante, its effect will most certainly be to increase the prices of vehicles (both used and new).

Regulation based on the preferences of high-income households will be regressive either when the costs of such regulation are borne by all households equally or when lower-income households bear disproportionate cost burdens. Existing empirical contributions investigating the channel through which regulation may have regressive effects report evidence suggesting that regulation has regressive effects on the prices of goods and services both because items consumed by lower-income households tend to be affected more significantly by the regulation's costs and because lower-income households spend larger-than-average shares of their overall budgets on regulated goods and services in general and "necessities" in particular (Chambers et al. 2017; Hoffer et al. 2017). In this special issue, Chambers et al. report, for example, that the bottom income quintile of households spend about twice as much of their budgets on electricity as do the top income quintile of households (just over 4% for the bottom quintile, compared to just over 2% for the top quintile).

In this paper, we seek to investigate the empirical effects of regulation on wages. As is the case with prices, regulation may have disparate impacts on the wages of different workers. The overall effect of regulation on wages will be regressive if the wages of low-wage workers are affected more negatively than the wages of high-wage earners. Intuitively, imposing more regulations on the private sector should require reallocations of resources toward compliance activities and away from production-related activities. As a result, resources are diverted away from lower-paying, production-related occupations and instead

are directed toward higher-paying, compliance-related occupations, such as lawyers and accountants. If that is indeed the case, the effects of regulation on wages may be regressive because of greater employer demand for higher-paid compliance-related workers and less demand for lower-paid production workers.

Our paper contributes to the literature on the economic theory of regulation by parsing how regulatory rents, when they accrue to employees in the form of higher wages, are distributed across different occupations and income levels. Regressive effects result from the tendency of the political process to design regulations in ways that benefit incumbent producers by imposing requirements on the manufacturing process that deter new entrants (Tullock 1967; Stigler 1971; Peltzman 1976), while at the same time responding to the preferences of wealthy consumers and workers (Chambers et al. 2017; Hoffer et al. 2017). The consequences of such regulation can be regressive even if its costs are borne by all consumers and workers,¹ but existing empirical research suggests that lower-income households often bear disproportionately large burdens (Thomas 2012; Hoffer et al. 2017; Chambers et al. 2017).

A related literature investigating the intra-industry distribution of regulatory rents finds that a similar pattern holds for the ability of large firms to extract regulatory rents at the expense of smaller competitors in the same industry (Williamson 1968; Marvel 1977; Maloney and McCormick 1982).

The paper proceeds as follows. We first review the existing literature on regulation and its effects in Sect. 2. Section 3 describes our dataset and its sources. We explain our empirical specification in Sect. 4. Section 5 summarizes our results; Sect. 6 concludes.

2 Literature review

For three theoretical reasons, the regulatory process may systematically reward higher-income households at the expense of lower-income households. The first reason is that higher-income individuals are more likely to participate in the political process (Jones and Cullis 1986), which ensures a disproportionate representation of their preferences in the political arena and, by extension, the regulatory process more specifically. Second, higher-income individuals, along with interest groups and lobbyists, may be better able to capture the regulatory process by means other than their votes. Third, government agents implementing regulation are more likely to be elites themselves and therefore ignorant of the preferences and circumstances of lower-income households (Thomas 2017).

None of these potential explanations for the existence of regressive effects offers much insight regarding the channel through which regulation harms low-income consumers and workers. The existing literature on the regressive effects of regulation suggests that regulation may have regressive effects because it tends to raise consumer prices. Hoffer et al. (2017) as well as Chambers et al. (2017) report empirical evidence on the burdens regulation imposes on consumers. Hoffer et al. (2017) do so by dividing

¹ Thomas (2012) argues that regulation often targets the mitigation of low-probability, high-cost risks, reflecting the preferences of high-income households. In the absence of intervention, low-income households would mitigate higher probability, lower-cost risks first. Regulation displaces some private risk mitigation efforts and, moreover, public action aimed at reducing overall exposure to environmental and safety risks mainly indulge the risk-mitigation preferences of wealthier households. Because all households bear the costs of regulation in the form of higher prices and lower wages, the effects of public regulation of health and safety risks are regressive.

the population into two groups: those receiving food stamps and those who do not. They then investigate the spending patterns of the two groups on a variety of food and drink consumption choices. They find that both food stamp recipients and those not participating in the food stamp program exhibit relatively small income-expenditure elasticities for the goods studied, meaning that both groups react to reductions in income with only small (and similarly small) adjustments in spending on the items in question. In addition, the authors find that both low- and high-income households respond similarly to changes in consumer prices. Hoffer et al. (2017) conclude that because consumption spending usually accounts for larger shares of low-income households' budgets, low-income households bear larger burdens of selective sales or excise taxation.

Chambers et al. (2017) reach a similar conclusion about regulation's effects on consumer prices. Using data across different industries, they find a positive, statistically significant relationship between regulation and inflation-adjusted consumer prices. Chambers et al. results imply that some of the costs associated with regulatory intervention are passed on to consumers in the form of higher prices. They also find that low-income individuals actually spend larger percentages of their incomes than do high-income earners on goods produced by heavily regulated industries. As with Hoffer et al. (2017), Chambers et al. conclude that regulation-induced price increases burden low-income households more than high-income households. Both of these contributions suggest that regulation may indeed have regressive effects channeled through its effects on consumer prices.

Unlike the papers summarized above, the goal herein is to add to the literature on regulation by conducting an empirical analysis of the channels through which regulation may have regressive effects by estimating its effect on wages. In doing so, we provide an answer to the question whether the political-regulatory process rewards specific subgroups of the population by redistributing wage income to them at the expense of other subgroups. As noted above, the economic theory of regulation suggests that regulatory intervention into the economy usually will be captured by special interest groups representing producer interests and will, as a result, generally benefit the regulated firms themselves (Tullock 1967; Stigler 1971; Peltzman 1976). The economic theory of regulation has shed some light on how rents will be distributed among different producers in one industry, among different groups within one firm, or among the population at large. Existing contributions illuminating this question generally conclude that the actual distribution of the benefits and costs of a regulatory rule will depend on the design of the specific rule in question. Analyzing the effects of trucking regulation under the old Interstate Commerce Commission, later superseded by the Surface Transportation Board, Moore (1978, p. 336) finds, for example, that the main beneficiaries of regulation were unionized truck drivers and their managers, while unregulated, non-unionized drivers suffered a decline in their wages as a result of regulation of surface transportation (prices and operating authority). Investigating the factors that may have led to a wave of deregulation in the late 1970s and early 1980s, Peltzman et al. (1989) describes how regulatory rents were distributed among producer and consumer interests in various regulated industries. He shows that under railroad regulation, for example, short-haul shippers benefited at the expense of long-haul shippers, shippers of inelastically demanded commodities were penalized by commodity-based price discrimination to the benefit of shippers of elastically demanded commodities, and that some related industries—most importantly, agriculture—benefited as a result of both of those effects. A long list of similar empirical studies—including Keeler (1972), Douglas and Miller (1974), Paul (1982), Shepard (1978) and Kamath (1989)—identify the specific beneficiaries of different kinds of regulation across various industries and nations.

McKenzie (2017) offers a theoretical model analyzing choices between different regulatory policy instruments. He shows that politicians, as rent extractors, are pivotal in selecting policy instruments and that, under certain circumstances, inefficiently strict environmental standards will be applied to generate the rents that potentially can be extracted by regulation's beneficiaries and by the politicians and agencies that craft regulatory regimes (McKenzie 2017, p. 149).² In line with McChesney (1987), McKenzie's model suggests that politicians represent key collectors of regulatory rents and that the prospects for rent extraction and rent seeking influence choices between different policy instruments. Aidt (2010) similarly shows that, under certain circumstances, voters, through corresponding income tax reductions, can become the beneficiaries of revenue from taxes designed to penalize polluters.³

Finally, a related literature shows that specific firms within an industry can use regulatory intervention to improve their relative position within the industry. Williamson (1968) shows, for example, that relatively capital-intensive firms in an industry can improve their profitability compared to more labor-intensive rivals by supporting collective bargaining arrangements that result in wage increases, which impose a disproportionate burden on the more labor-intensive producers in the industry. Maloney and McCormick (1982), show that environmental quality regulation designed to address externality problems can be used by subgroups of larger producers in an industry to gain a cost-advantage over smaller producers, because they can spread the fixed cost of compliance over a larger amount of output. According to Marvel (1977), the British Factory act, which regulated hours worked by women and children, benefited some mill operators in the British textile industry over others.

In what follows, we analyze the patterns of rent distributions among different subgroups within a regulated industry. More specifically, we ask whether regulation systematically rewards higher-earning workers at the expense of lower-income workers—that is, we analyze whether regulation has regressive effects on wages.

3 Data

We obtained hourly wage data from the Bureau of Labor Statistics' Occupational Employment Statistics (OES). To create the OES dataset, the Bureau of Labor Statistics surveys about 200,000 establishments every 6 months, then aggregates the responses to protect privacy before making them public. The OES provides data on hourly wages and employment aggregated by occupation and industry from 2002 to 2015. The OES classifies workers into one of 859 occupations and publishes wage data on those occupations for each of the 15 industries in the two-digit North American Industry Classification System (NAICS).

We obtained industry-level regulation data from Mercatus Center's *RegData* 2.2 database, which quantifies the regulatory burden at the industry level using NAICS codes. The

² Aidt (2003) shows that distributive programs that offer inefficient subsidies are unlikely to be contested politically. Programs that employ inefficient means of taxation are, on the contrary, likely to be contested. He also shows, relying on an extended version of Becker's (1983) pressure group model, that contrary to Becker's predictions, on the whole, political competition increases rent seeking activity and that in the absence of such competition, the total social costs of regulatory intervention are self-limiting.

³ Hillman (1982) likewise analyzes the dynamics of rent seeking among politicians, industry representatives and all other voters.

database parses the *Code of Federal Regulations*, counts restrictive words such as “shall” or “must”, and catalogs the number of restrictions according to the regulation’s relevance to each industry.⁴ This dataset assembles regulatory data from 1970 to 2014.

Pairing these data sources yields a panel dataset combining regulations and hourly wages by industry and occupation from 2002 to 2014 (13 years). All wage data are inflation adjusted to 2014 dollars.

4 Methodology

To test our hypotheses on regulation’s regressive wage effects, we run several versions of the following model with various dependent variables. In all of the regressions, the key explanatory variable is the natural log of regulatory intervention into a given industry in a given year, measured, as discussed above, by the number of restrictive words in industry-specific regulations. The initial dependent variable is the natural log of average hourly wages in a given year, industry and occupation. Our simple OLS model is

$$wage_{it}^j = \beta reg_{it} + \alpha_j + \gamma_i + \eta_t + \epsilon_{it}^j, \quad (1)$$

where j represents occupations, i represents two-digit NAICS industries and t represents years. Controls may include industry, occupation and year fixed effects, along with industry-specific time trends.

To investigate the first hypothesis (i.e., regulation reduces the average wages of all workers), we estimate Eq. 1 with the log of average hourly wages (*LnHourlyWage*) as the dependent variable. We investigate the second question (i.e., how regulation affects lower-wage workers relative to their higher-wage counterparts) by replacing our initial dependent variable with estimates of the 10th, 25th, 50th, 75th and 90th percentiles of wages for each industry and occupation (also taken from the OES). Specifically, we would expect the relationship between regulation (*LnRegulation*) and the wages of the lowest quartile of workers to be more strongly negative than the relationship between regulation and the wages of the top quartile of workers if regulation does indeed have a regressive effect.

5 Results

The summary statistics are shown in Table 1.

To determine the effect of regulation on overall average wages, Table 2 reports the results of estimating Eq. 1 with varying control variables. In columns 1 and 2, regulation is associated with a statistically significant reduction in wages, with a doubling of regulation resulting in a 0.49–1.2% decline in wages. Given that the mean hourly wage in our sample is \$19.41, this implies that a doubling of regulation is associated with a wage decline of 10–23 cents per hour for an average worker. When industry fixed effects or industry-specific time trends are entered (columns 3–5), regulation no longer has a statistically significant effect on wages. That reduction in statistical significance difference may indicate that the initially observed correlation of regulation with low wages is

⁴ See Al-Ubaydli and McLaughlin (2015) for details on the database’s construction.

Table 1 Summary statistics

| Variable | Observations | Mean | SD | Min. | Max. |
|------------------|--------------|--------|--------|--------|-----------|
| Year | 112,992 | – | – | 2002 | 2015 |
| Industry | 112,992 | – | – | – | – |
| Occupation | 112,992 | – | – | – | – |
| Regulation | 71,275 | 334.02 | 476.56 | .5526 | 1980.68 |
| Total employment | 107,053 | 16,922 | 92,800 | 30 | 4,311,040 |
| Hourly wage | 109,607 | 19.41 | 10.02 | 5.61 | 98.82 |
| LnRegulation | 71,275 | 4.711 | 1.876 | –.5930 | 7.591 |
| LnHourlyWage | 109,607 | 2.855 | .4584 | 1.725 | 4.593 |
| 10th Percentile | 109,606 | 11.412 | 5.3084 | 4.881 | 65.112 |
| 25th Percentile | 109,557 | 14.200 | 7.0295 | 5.119 | 66.53 |
| 50th Percentile | 109,236 | 18.06 | 8.9591 | 5.523 | 71.096 |
| 75th Percentile | 108,424 | 22.805 | 11.011 | 5.917 | 71.832 |
| 90th Percentile | 105,708 | 27.375 | 12.467 | 6.155 | 71.968 |

Table 2 Effect of regulation on log average hourly wages

| Model | (1) | (2) | (3) | (4) | (5) |
|-------------------------------|----------------------|----------------------|------------------|------------------|------------------|
| lnRegulation (t – 1) | –.0118*** (.0017) | –.0049*** (.0006) | .0053 (.0060) | .0007 (.0057) | .0011 (.0073) |
| Year fixed effects | Yes | Yes | Yes | No | Yes |
| Occupation fixed effects | No | Yes | Yes | Yes | Yes |
| Industry fixed effects | No | No | Yes | No | Yes |
| Industry-specific time trends | No | No | No | Yes | Yes |
| Adjusted R ² | 0.004 | 0.90 | 0.92 | 0.92 | 0.92 |
| Observations | 64,148 | 64,148 | 64,148 | | 64,148 |

Robust standard errors clustered on occupation are in parentheses

* $p < .10$ ** $p < .05$ *** $p < .01$

explained by lower-wage industries attracting more regulation (Hillman 1982). Alternatively, the change in the precision of the coefficient’s estimate may simply indicate a loss of efficiency from including industry fixed effects on the right-hand side because those effects are almost perfectly collinear with regulation. A regression of industry fixed effects on regulation yields an R² of 0.995. As such, we omit industry fixed effects from the remaining regressions, focusing on the specification with year and occupation fixed effects; we later apply first-differencing.

The results for the test of our second hypothesis—that regulation has a disproportionately negative effect on lower-wage earners by occupation—are shown in Table 3.

The results are very similar to those of the models using average wages as the variable to be explained. In the simpler, initial model, we find strong negative effects of regulation on wages. Those effects also are strongly regressive: we estimate that a doubling of regulation leads to a 0.73% decline in wages for the bottom 10% of wage earners, but only a 0.36% decline in wages for the top 10% of wage earners (a difference that is statistically significant at the 1% level).

Table 3 Effect of regulation on the distribution of wages

| | 10th percentile | 25th percentile | 50th percentile | 75th percentile | 90th percentile |
|--------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| lnRegulation (t - 1) | -.0073*** (.0007) | -.0070*** (.0007) | -.0064*** (.0007) | -.0051*** (.0007) | -.0036*** (.0007) |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Occupation fixed effects | Yes | Yes | Yes | Yes | Yes |
| Adjusted R ² | 0.85 | 0.87 | 0.87 | 0.87 | 0.87 |
| Observations | 64,148 | 64,109 | 63,854 | 63,245 | 61,201 |

Robust standard errors clustered on occupation are in parentheses

p* < .10; *p* < .05; ****p* < .01

Table 4 Lags-and-leads test

| | 10th percentile | 25th percentile | 50th percentile | 75th percentile | 90th percentile |
|-------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| Variables | lnHourly10 | lnHourly25 | lnHourly50 | lnHourly75 | lnHourly90 |
| lnRegulation (t + 2) | .111*** (.0425) | .0366 (.0444) | -.0277 (.0415) | -.0389 (.0411) | -.0543 (.0417) |
| lnRegulation (t + 1) | -.0287 (.0409) | .0333 (.0426) | .0810** (.0409) | .0895** (.0412) | .112*** (.0417) |
| lnRegulation (t) | -.00735 (.00956) | -.0131 (.00854) | -.0102 (.0085) | -.00768 (.0091) | -.0025 (.0098) |
| lnRegulation (t - 1) | -.0303*** (.0096) | -.0157* (.0087) | -.0109 (.0071) | -.0126* (.0075) | -.0248*** (.0082) |
| lnRegulation (t - 2) | -.0498*** (.0081) | -.0464*** (.0080) | -.0373*** (.0076) | -.0341*** (.0074) | -.0323*** (.0078) |
| Adjusted R ² | 0.870 | 0.885 | 0.894 | 0.896 | .888 |
| Observations | 38,282 | 38,256 | 38,094 | 37,690 | 36,334 |

Robust standard errors clustered on occupation are in parentheses. Regressions include year and occupation fixed effects

p* < .10; *p* < .05; ****p* < .01

One natural concern given the 2002–2014-time span of our data is that the Great Recession occurs in its midst and may drive some of our empirical results. To address that concern, we repeat the analysis of Table 3 using only data from before 2008, when the labor market effects of the recession became substantial. The results, shown in “Appendix” Table 9, are very similar to those reported in Table 3.

Table 4 shows the results of a lags-and-leads test of the effects of regulation on wages. The test reruns the regressions reported in Table 3, but augments the model with measures of regulation’s burden 2 years before and 2 years after the current year, rather than entering only a one-year lagged measure of regulation. The test is intended to capture the effects of regulations over time: If regulations in future years affect wages in the present year significantly, that result indicates a problem with the specification (usually over-rejection of null hypotheses, or endogeneity). In our case, the effect of past regulation on present wages holds up, with statistically significant coefficients of larger magnitude. The regressive

Table 5 Effect of regulation on the distribution of wage growth

| | 10th percentile | 25th percentile | 50th percentile | 75th percentile | 90th percentile |
|--------------------------|-----------------------|----------------------|--------------------|--------------------|------------------|
| InRegulation (t – 1) | – .0004*** (.0001) | – .0003** (.0001) | – .0002 (.0001) | – .0001 (.0001) | .0001 (.0002) |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Occupation fixed effects | Yes | Yes | Yes | Yes | Yes |
| Adjusted R ² | .04 | .03 | .03 | .03 | .03 |
| Observations | 63,849 | 63,802 | 63,520 | 62,850 | 60,548 |

Robust standard errors clustered on occupation are in parentheses. The dependent variable is the differenced natural log of wages, so coefficients can be interpreted as the percentage change in wage growth

* $p < .10$; ** $p < .05$; *** $p < .01$

effect remains when comparing wages in the lower quartiles to the middle, but not when comparing the top wage quartile to the middle one.

In four of ten cases, we find that future regulation raises present wages. That result likely indicates an endogeneity problem, perhaps suggesting that fast-growing industries both raise wages and attract regulatory attention. We take some comfort in the fact that all of the significant future coefficient estimates are positive, while all significant past coefficients are negative, so that the endogeneity would bias our estimates toward zero.

In light of the endogeneity concerns raised by the lags-and-leads tests, we now consider first-difference models that use wage growth rather than wage levels as a dependent variable. That redefinition has other benefits in addition to reducing the scope for endogeneity and omitted-variable problems. A Hadri test performed on a strongly balanced subset of the data indicates that some panels of wage levels have unit roots, but that all panels of wage growth are stationary. The first-difference model also passes the lags-and-leads test that the original model failed.

Table 5 shows how regulation affects the distribution of wage growth. It shows a regressive effect of regulation, with statistically significant reductions in wage growth for the 10th and 25th percentiles and no statistically significant effects at the 50th percentile or higher. However, the magnitude of the regressive effect is quite small. A doubling of regulation is associated with only a 0.04 percentage-point reduction in annual wage growth at the 10th percentile. Wage growth during the 2002–2014 period is quite small overall, though: 0.21% per year at the mean and actually shrinking by 0.19% per year at the 10th percentile.

Table 6 reports the results of the lags-and-leads test for the first-differenced model. It finds no statistically significant leads for regulation's effects. The test also finds no significant lagged effects, with the only significant effects being contemporaneous. The sign pattern provides further evidence of regressive effects, with magnitudes much larger than in Table 5: a 4.7 percentage-point decrease in wage growth at the 10th percentile and a 3.8 percentage-point reduction at the 25th percentile. Given that mean hourly wages are \$11.41 at the 10th percentile and \$14.20 at the 25th, this implies wage reductions of 54 cents per hour at both percentiles.

While it seems that regulation does not robustly affect wages in general, it may still affect the wages of specific occupations. We test whether occupations most associated with regulatory compliance experience a wage boost as a result of regulation. The results, shown

Table 6 Lags-and-leads test with wage growth

| | 10th percentile | 25th percentile | 50th percentile | 75th percentile | 90th percentile |
|-------------------------|----------------------|----------------------|--------------------|-------------------|-------------------|
| Variables | lnHourly10 | lnHourly25 | lnHourly50 | lnHourly75 | lnHourly90 |
| F2.lnRegulation | .0248 (.0273) | .0013 (.0261) | -.0086 (.0242) | -.0067 (.0235) | -.0145 (.0267) |
| F.lnRegulation | .0258 (.0278) | .0312 (.0267) | .0209 (.0243) | -.0045 (.0238) | -.0132 (.0268) |
| lnRegulation | -.0472*** (.0129) | -.0383*** (.0112) | -.0184* (.0095) | .00415 (.0107) | .0223* (.0124) |
| L.lnRegulation | -.00769 (.0135) | .00948 (.0113) | .0110 (.0100) | .00904 (.0117) | .00016 (.0130) |
| L2.lnRegulation | .0045 (.0077) | -.0037 (.0075) | -.0052 (.0059) | -.0025 (.0061) | .0048 (.0070) |
| Adjusted R ² | 38,149 | 38,116 | 37,936 | 37,477 | 35,967 |
| Observations | .056 | .047 | .039 | .038 | .037 |

Robust standard errors clustered on occupation are in parentheses. Regressions include year and occupation fixed effects. The dependent variable is the differenced natural log of wages, so coefficients can be interpreted as the percentage change in wage growth

* $p < .10$; ** $p < .05$; *** $p < .01$

Table 7 Effects on specific occupations

| | (1) | (2) |
|-----------------------------------|---------------------|---------------------|
| lnRegulation (t - 1) | -.0062 (.0063) | -.0152** (.0068) |
| Manager * lnRegulation (t - 1) | .1097*** (.0176) | .1705*** (.0172) |
| Accountant * lnRegulation (t - 1) | .0644* (.0339) | .0607* (.0313) |
| Legal * lnRegulation (t - 1) | -.0280 (.0418) | -.0291 (.0412) |
| Year fixed effects | Yes | Yes |
| Occupation fixed effects | Yes | Yes |
| Industry fixed effects | Yes | Yes |
| Industry time trend | No | Yes |
| Observations | 64,148 | 64,148 |

Robust standard errors clustered on occupation are in parentheses

* $p < .10$; ** $p < .05$; *** $p < .01$

in Table 7, are that doubling regulation leads to a statistically significant 11–17% percent increase in the wages of managers and a borderline-significant 6% increase in the wages of accountants, but does not affect wages overall for legal occupations. An F-test indicates that regulation and its occupation interaction terms are jointly significant at the 1% level.

Table 8 shows that while regulation leads to declines in overall employment, it leads to very large increases in employment for the managerial, accounting and legal professions.

Table 8 Effects on employment in specific occupations

| | (1) | (2) |
|-----------------------------------|----------------------|---------------------|
| InRegulation (t - 1) | -.1088*** (.0338) | -.086*** (.0298) |
| Manager * InRegulation (t - 1) | .2514*** (.0859) | .3020*** (.0820) |
| Accountant * InRegulation (t - 1) | .4766*** (.0783) | .5548*** (.1181) |
| Legal * InRegulation (t - 1) | .7027*** (.1620) | .7667*** (.1648) |
| Year fixed effects | Yes | Yes |
| Occupation fixed effects | Yes | Yes |
| Industry fixed effects | Yes | Yes |
| Industry-specific time trends | No | Yes |
| Observations | 58,048 | 58,048 |

Robust standard errors clustered on occupation are in parentheses

* $p < .10$; ** $p < .05$; *** $p < .01$

6 Conclusion

Our findings support the theory that the benefits of regulation to the regulated industry in the form of wages and job opportunities accrue mostly to managers and workers employed in compliance-relevant positions, such as accountants and lawyers. We find some evidence that the costs associated with regulation lead to slower wage growth and that the burden is borne disproportionately by lower-wage workers. Overall, our results point to regulation's regressive effects on wages in the sense that even if low-wage workers do not experience nominal wage reductions as regulation expands, their wages fall relative to those of managers and accountants in regulated industries. Our results for the effect of regulation on employment support the theory that regulation's impacts on different occupational groups may be regressive because managers, accountants and lawyers escape the general downward trend and instead find themselves in greater demand when regulatory interventions become more onerous. One reason why regulation may have a disproportionate regressive effect on the wage structure is that higher regulatory hurdles raise the value-added of workers who can navigate increasingly complex legal environments and compliance requirements, while also forcing firms to lower production costs in other areas to compensate for more burdensome regulatory compliance. Taken together, those two pressures may push regulated firms to hire more and better-paid managers and accountants while holding the wages of other workers, and their total numbers, constant to cope with the higher costs of that follow regulation.

Appendix

See Table 9.

Table 9 Pre-recession effects of regulation on the distribution of wages

| | 10th percentile | 25th percentile | 50th percentile | 75th percentile | 90th percentile |
|--------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| InRegulation (t - 1) | -.0070*** (.0008) | -.0067*** (.0008) | -.0061*** (.0008) | -.0048*** (.0008) | -.0032*** (.0009) |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Occupation fixed effects | Yes | Yes | Yes | Yes | Yes |
| R ² | .84 | .86 | .87 | .87 | .86 |
| Observations | 24,614 | 24,594 | 24,514 | 24,282 | 23,500 |

Robust standard errors clustered on occupation are in parentheses. Years included are 2002–7

* $p < .10$; ** $p < .05$; *** $p < .01$

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