The Persistent Effect of Temporary Affirmative Action: Online Appendix

Conrad Miller

Contents

List of Figures

List of Tables

A Extensions and Robustness Checks

A.1 Heterogeneity by Employer Size

In this section, I explore how the regulation's effect varies with employer size. This exercise serves two purposes. First, I assess whether estimates are sensitive to the selective attrition of establishments from the data. Second, I exploit the fact that compliance evaluations are targeted based on employer size (Leonard 1985a) to examine whether the regulation's impact is more substantial where enforcement is stronger.

The absence of pre-existing trends in the regulation event study suggests that the black share gains following AA regulation indeed reflect the causal effect. However, the event study may produce biased estimates for the causal effect if establishments selectively exit from the data. The size thresholds for who must submit EEO-1 forms magnify this concern. In particular, some firms that are near the threshold may alter their size to avoid reporting requirements.

To assess the potential role of selective attrition in producing the above results, I re-estimate both the regulation and deregulation event studies restricting estimation to establishments whose parent firms have at least 150 employees prior to their first federal contract. For eventual contractor establishments, I base this restriction on firm size in the latest year an establishment is observed prior to their regulation event. For non-contractor establishments, I use firm size in the latest year an establishment is observed prior to their *pseudo* regulation event, where pseudo event events are randomly assigned as described in section II.D, based on the year I first observe the establishment in the data and the number of years between the first and last year. These establishments are not near the size threshold, and so any manipulation to avoid reporting seems unlikely. Note that over 90% of establishments in the overlapping sample satisfy this criteria.

The results are shown in column (2) of Table A.3. Panel A presents regulation event slope estimates, while Panel B presents deregulation event slope estimates. The estimates for both event studies are very similar to those using the full overlapping sample. I conclude that selective attrition is unlikely to be an important concern here.

Leonard (1985a) studies the targeting of compliance evaluations conducted by the Department of Defense over the late 1970's. He finds that contractor establishments that are part of multi-establishment firms are substantially more likely than singleton contractor establishments to be subject to a compliance evaluation. The likelihood of review is generally increasing in establishment size, though the relationship is concave. Motivated by these findings, I explore how the response to regulation depends of whether an establishment is part of a larger company, and establishment size. Note that I focus on a later period than Leonard (1985a), and the targeting of compliance evaluations has likely changed over time. Hence, these results should be interpreted with caution.

The results are presented in columns (3)-(6) in Table A.3. Columns (3) and (4) report estimates based only on singleton establishments and establishments that part of multi-establishment firms, respectively. Column (5) reports estimates based only on establishments with fewer than 100 employees, while column (6) reports estimates based on establishments with 100 or more employees. The regulation appears to have little effect on singleton establishments. Larger establishments experience larger black share gains following the regulation event, and larger gains following deregulation, though similar patterns emerge for smaller establishments. Overall, it appears that establishments that are more likely to be evaluated by regulations respond more to regulation.

Note that while only about 13% of establishments in the overlapping sample are singletons, they represent over 70% of sample firms. The significant heterogeneity found here implies that an analysis that weights firms equally, as in Kurtulus (2016; 2012), rather than establishments, as done in the present paper and previous work in the literature, will produce estimates of the regulation's impact that are substantially smaller in magnitude.[1]

A.2 Heterogeneity by Skill Level

While AA regulation generates a sharp increase in minority share growth, and most of these gains are within-occupation, it is unclear what kinds of jobs are driving this growth. To clarify this, I repeat the within-occupation event study analyses separately by occupation skill level. Following Acemoglu and Autor (2011), I divide the occupation groups defined in the EEO-1 data into three skill groups: high, middle, and low. I label officials and managers, professionals, and technicians as "high skill"; sales workers, administrative support workers, craft workers, and operatives as "middle skill"; and laborers/helpers and service workers as "low skill."

I present the results in Figure A.7. Event study patterns are similar across occupation groups. In absolute terms, the effect on black share is largest in middle skill occupations. Five years after the regulation event, the black share of employees in middle skill occupations increased by 0.8 percentage points. Estimates for high and low skill occupations are similar in magnitude at about 0.6 percentage points, though they are relatively imprecise. In the EEO-1 data, 7.0%, 12.6% and 20.5% of high skill, middle skill, and low skill workers are black. Hence, in proportional terms, the effects of AA are similar for both high and middle skill occupations.

A.3 Combined Parametric Event Studies

The event studies can also be easily combined into one parametric regression model. Using the overlapping sample, I estimate the following model:

$$\text{black share}_{it} = \alpha_i + \lambda_{d(i),t} + X_{it}\gamma + \beta t \times \mathbf{1}_{\exists \tau_i} + \beta^R (t - \tau_i^R + 1) \times \mathbf{1}_{(t \geq \tau_i^R)} + \beta^D (t - \tau_i^D + 1) \times \mathbf{1}_{(t \geq \tau_i^D)} + \epsilon_{it} \tag{A.1}$$

where τ_i^R and τ_i^D denote regulation and deregulation event years, if applicable. I use all observation years, not restricting the data to any window around the event years. I estimate a pre-regulation slope, β, of -0.037 (with standard error 0.031); a post-regulation slope change, β^R, of 0.277 (0.059); and a post-deregulation slope change, β^D, of -0.160 (0.037). The slope estimates are nearly identical if I include a quadratic post-regulation term. I discuss the slope fadeout associated with deregulation in more detail in section A.4.

[1] Weighting by establishment size yields results similar to those presented here.

With slight modification, I also estimate A.1 excluding non-contractors from estimation. In this case, the regulation and deregulation effects are identified using only variation in the timing of events among eventual contractors. This approach is appealing in that it does not rely on non-contractors to identify the counterfactual black share for eventual contractors. However, as McCrary (2007) points out, the trend break model A.1 is not identified using only eventual contractors. To circumvent this issue, I include observations more than 6 years prior to the regulation event, augment the model with an indicator for years more than 6 years preceding the regulation event, and limit the pre-regulation slope to apply to 6 years preceding regulation and thereafter. Reassuringly, the results are similar. I estimate a pre-regulation slope, β, of -0.009 (with standard error 0.056); a post-regulation slope change, β^R, of 0.282 (0.065); and a post-deregulation slope change, β^D, of -0.153 (0.039). The coefficient on the indicator is -0.295 with standard error 0.355, statistically insignificant at the 10% level.

A.4 Slope Fadeout

The deregulation event study results suggest that while the black share of employees continues to increase following deregulation, this persistence is not complete. For example, in Panel B of Figure 3, the post-deregulation event slope is about 35% smaller than the pre-event slope. Moreover, Figure 4 suggests the degree of persistence may depend on an establishment's experience as a contractor. In this section, I explore possible fadeout in more detail.

Though deregulation is associated with a slope decrease in black share gains, it is not clear whether this decrease is due to deregulation *per se* or if this decrease would occur in the absence of deregulation. The slope must decrease at some point given that the black share is bounded a, where the ceiling depends on the availability of black workers. To assess whether the slope declines are caused by deregulation, I construct the following falsification test. First, I reassign 'pseudo' deregulation event years to one-time contractors. I do this by conditioning on the number of years between an establishment's regulation event and its last year in the data, and then randomly assign an 'age' for each establishment's pseudo event, taking draws from the conditional age distribution for the actual events. Then I re-estimate the deregulation event study using these pseudo events. If the slope change in Panel B of Figure 3 is due to 'age' rather than deregulation *per se*, the same slope change should be evident in the pseudo event study. If slope change is due to deregulation, the slope change should be significantly less pronounced.

I plot the coefficients for the pseudo deregulation event study in Panel A of Figure A.8. There is no discernible slope change following the pseudo event, which suggests that the slope change in Panel B of Figure 3 is indeed due to the deregulation.

Next, I test whether the degree of persistence depends on an establishment's experience as a contractor. I split contractors into two groups, those with 6 or fewer years between their regulation and deregulation events and those with more than 6 years between events, and estimate deregulation event studies for each group. Establishments in the first group have had an average of 2 years as contractors prior to their deregulation event, while establishments in the second group have had an average 8 years. In these event study models, I extend the endpoint b to 10 years following deregulation, and use the full deregulation sample for power. The results are plotted in Panel B of Figure A.8. There are two things to note. First, the initial slope is higher for the experienced group (0.286 percentage points per year) than for the novice group (0.214 percentage points). Second, black share growth following deregulation is more persistent for the experienced group in both absolute and relative terms.

Finally, I assess the long run black share gains associated with AA regulation. I estimate a regulation event study with the endpoint b extended to 20 years and using the full regulation sample. Figure A.9 displays the results. The point estimates are increasing up to 16 years after the regulation event, and then bounce around 2.4 percentage points, though the confidence intervals are relatively wide in this range. The black share gains associated with regulation remain quite apparent in the long run.

A.5 Robustness of Size and Black Share Relationship

In section III.B, I document a strong relationship between employer size and black share at both the establishment and job level. In this section, I explore the robustness of this result. I test several alternative explanations for the positive relationship between employer size and black share found here. First, I test whether this relationship is an artifact of the business cycle. For example, if establishments tend to grow during expansions, and black job seekers make up a larger fraction of the applicant pool during expansions, then employers will tend to increase their black share as they grow. Second, I test whether this relationship is due to AA. I find above that AA causes the black share of employees to increase. If this is primarily driven by regulated employers increasing their black share of new hires, for example, then the black share may increase more for growing establishments.

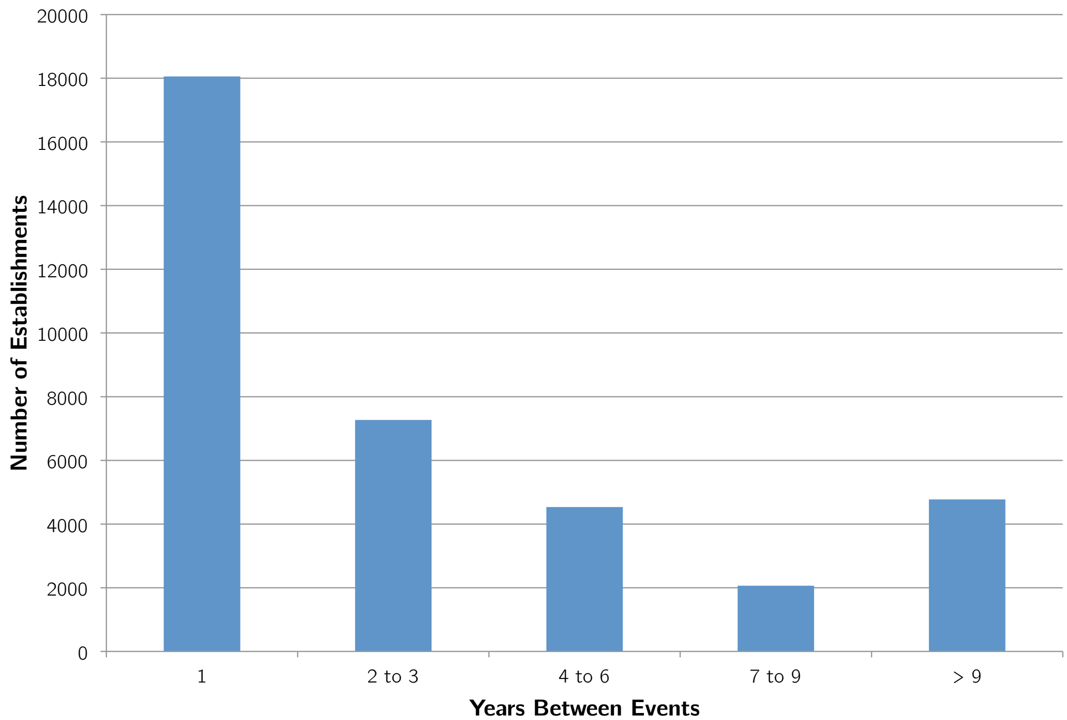
I test these alternative hypotheses, focusing on within-job changes in black share. I estimate models of the form

$$\Delta(\text{black share})_{iot} = \alpha + \lambda_{d(i),t} + \beta \Delta \log(\text{establishment size})_{it} + \epsilon_{iot} \tag{A.2}$$

where $\Delta x_{it} = x_{i,t} - x_{i,t-1}$. I estimate this model using all the data, separately for recession and expansion years, and separately for contractors or one-time contractors and establishments with no contractor experience. Note that I measure size changes at the *establishment* level, not the job cell level.

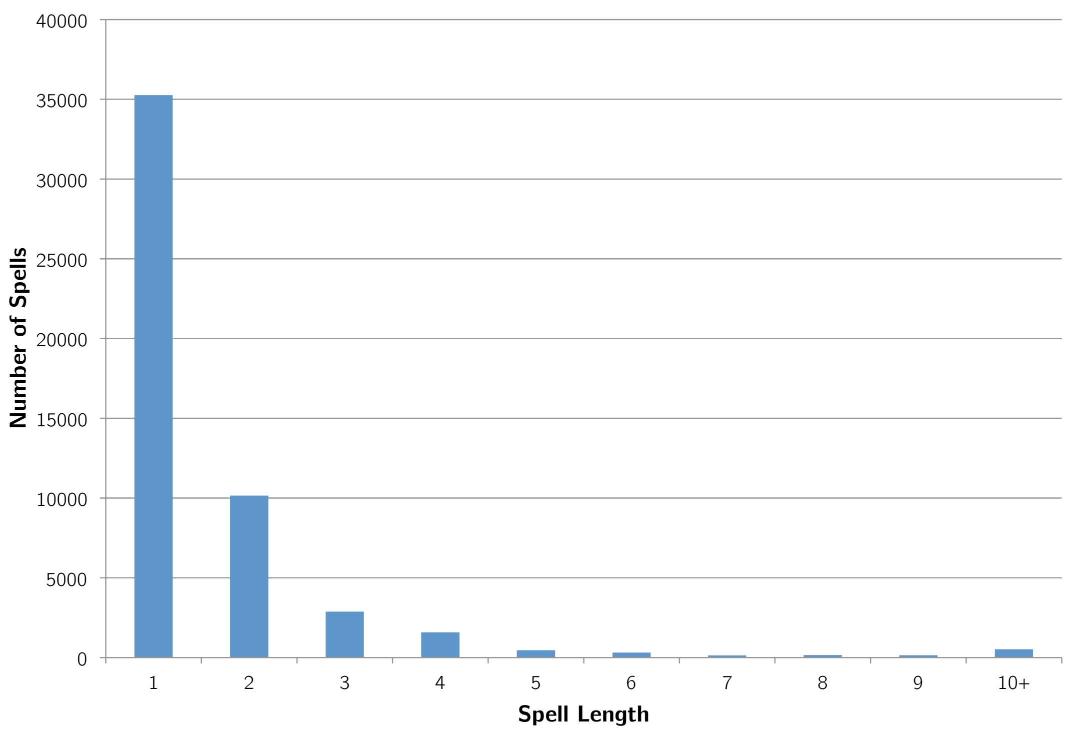
The results are presented in Table A.5. Using the full data, the first difference model produces a β coefficient of 0.781-that is, a 10% increase in establishment size predicts about a 0.08 percentage point increase in the black share of employees within jobs. Estimates are comparable during both economic recessions and expansions, contradicting the business cycle hypothesis. Finally, the relationship between employer size and black share is even *larger* for establishments with no contractor experience. This is consistent with AA regulation inducing employers to make screening investments they would otherwise make as larger establishments.

Figure A.1: Years Between Regulation and Deregulation Events



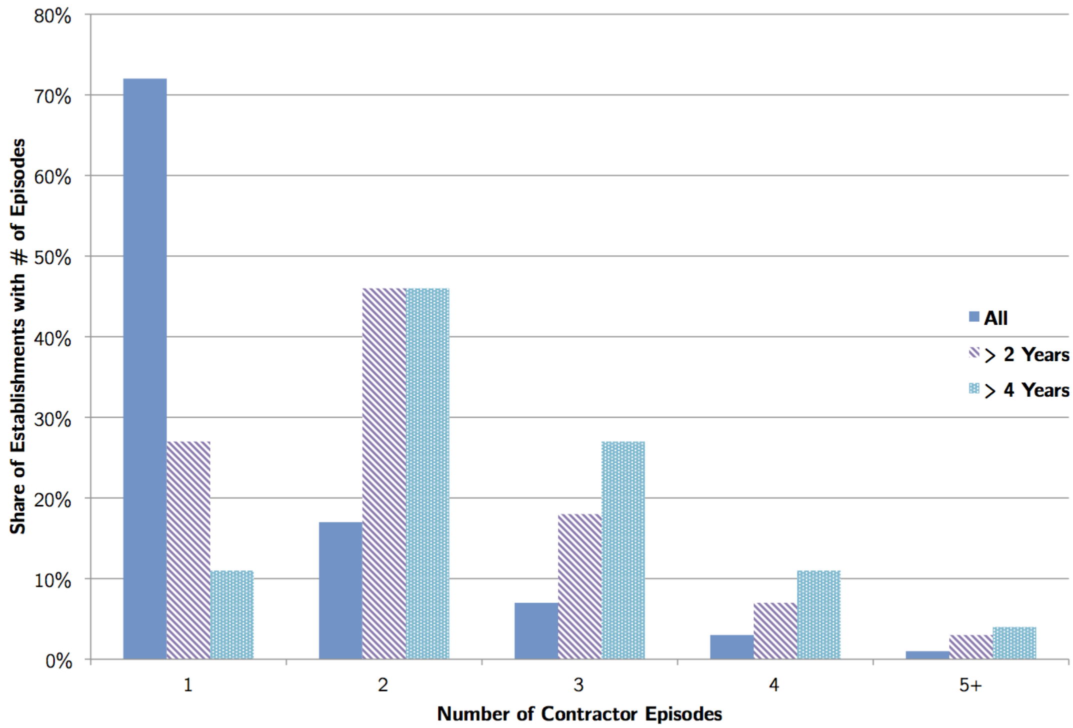
Notes: This figure plots the histogram for the number of years between an establishment's regulation event and deregulation event in the overlapping sample.

Figure A.2: Distribution of Contractor Spell Length



Notes: This figure plots the histogram for contractor spell length in the overlapping sample. A contractor spell is any period of consecutive years when an establishment is a contractor.

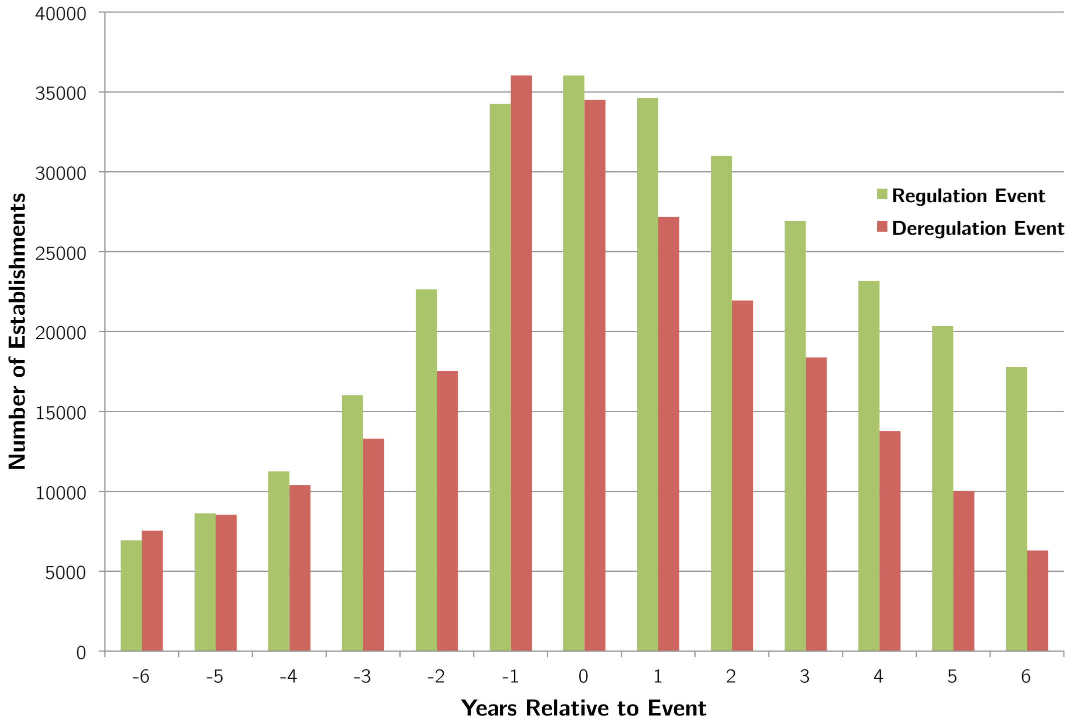
Figure A.3: Number of Contractor Episodes by Establishment



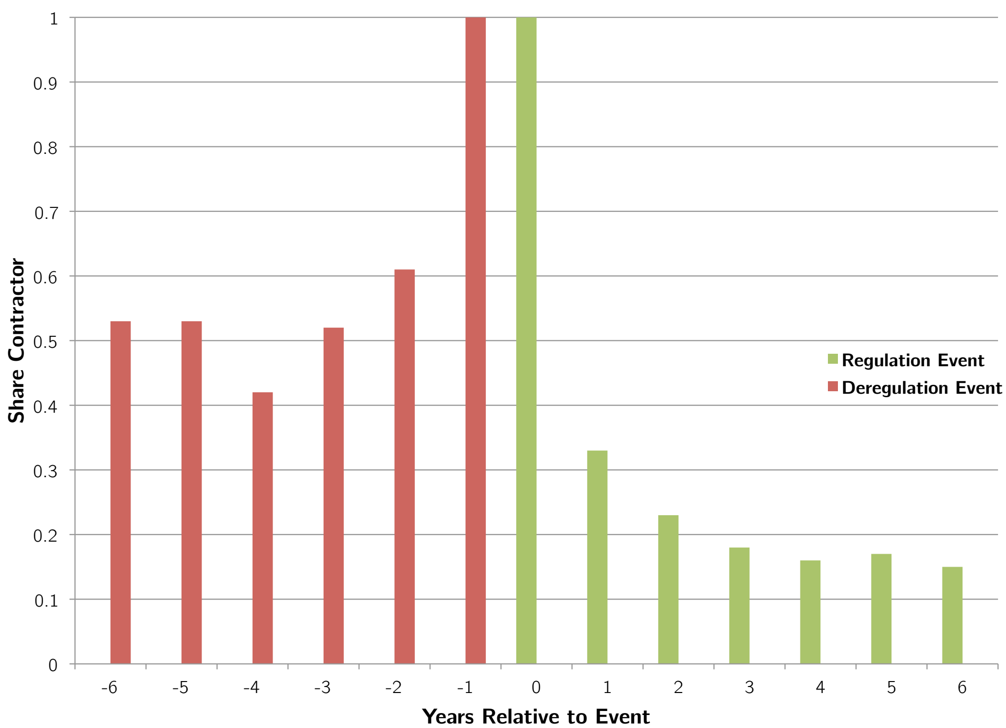
Notes: This figure plots the frequencies for the number of contractor episodes experienced by establishments in the overlapping sample. The '> 2 Years' and '> 4 Years' bars refer to eventual contractors with more than 2 or 4 years between their regulation and deregulation events.

Figure A.4: Summary Statistics by Event Study Year

(A) Number of Establishments in Event Studies



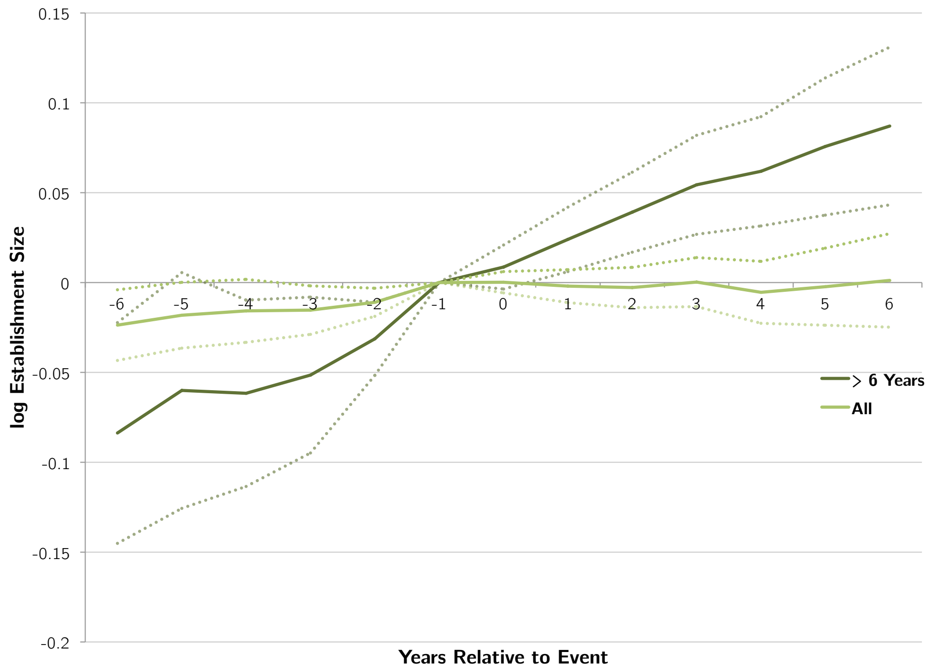
(B) Share Contractor by Year in Event Studies



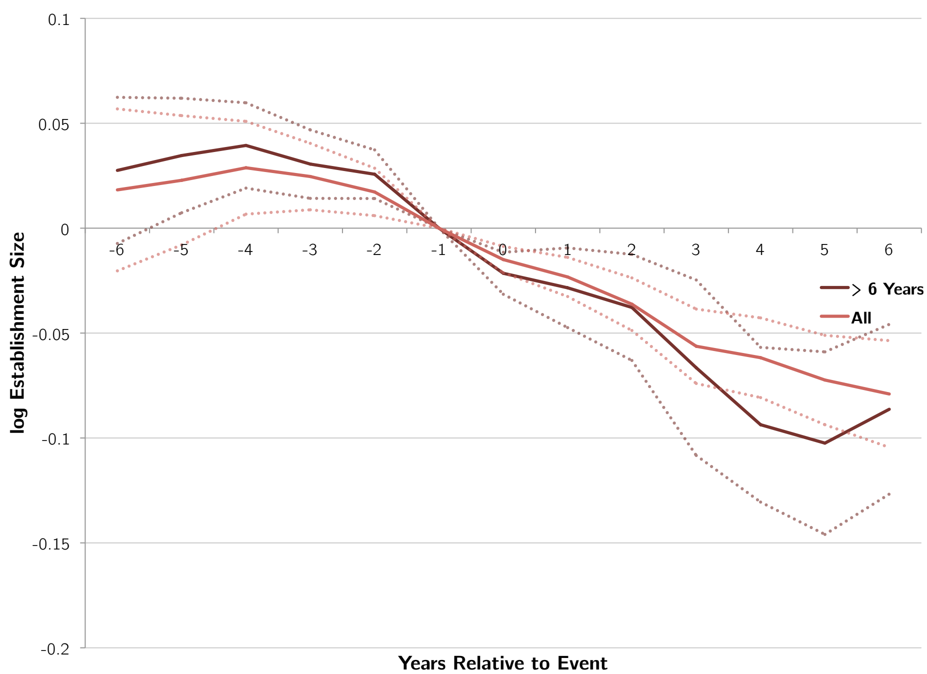
Notes: These figures graph summary statistics for sample used to construct the main event study plots presented in Figure 3. In Panel A I tabulate the number of establishments used to identify each lead and lag in the regulation and deregulation event studies. In Panel B I show the fraction of eventual contractors that are contractors at each node of the event studies.

Figure A.5: Establishment Size and Regulation and Deregulation Events

(A) Regulation Event



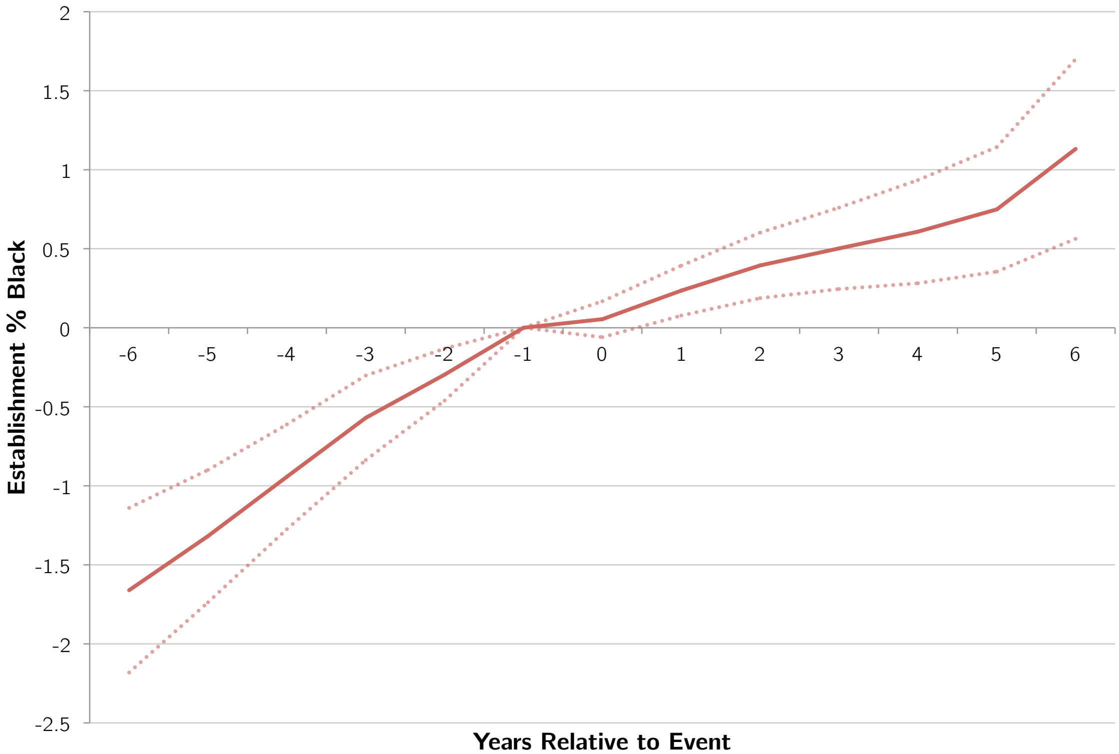
(B) Deregulation Event



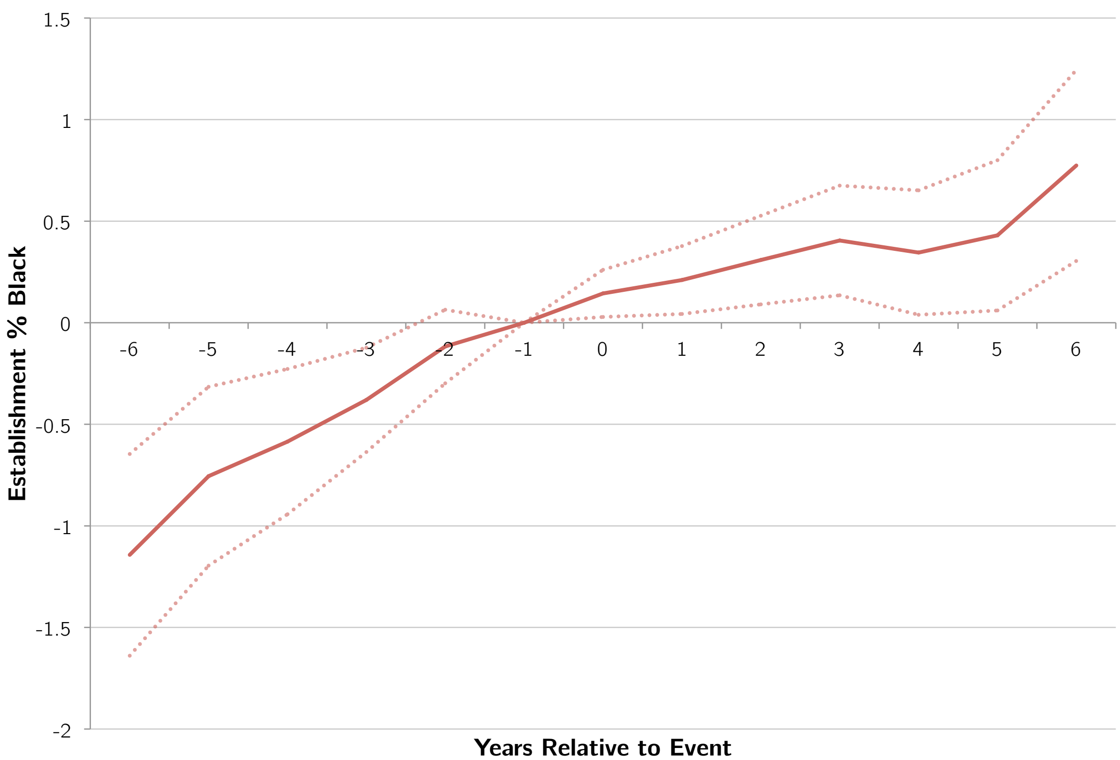
Notes: These figures plot the event study coefficients and 95% confidence intervals (dotted) estimated using model (1) and the overlapping sample, where the outcome variable is log establishment size. Panel A depicts the regulation event study; Panel B depicts the deregulation event study. The definitions of regulation and deregulation events are described in section II.B. The '> 6 Years' line restricts eventual contractors to those with more than 6 years between their regulation and deregulation events. The coefficient for the year prior to the event (θ_{-1}) is normalized to zero. Estimated models include Census division by year fixed effects. Standard errors are clustered at the firm level.

Figure A.6: Deregulation Event Study, by Subsequent Growth

(A) Shrinking Establishments

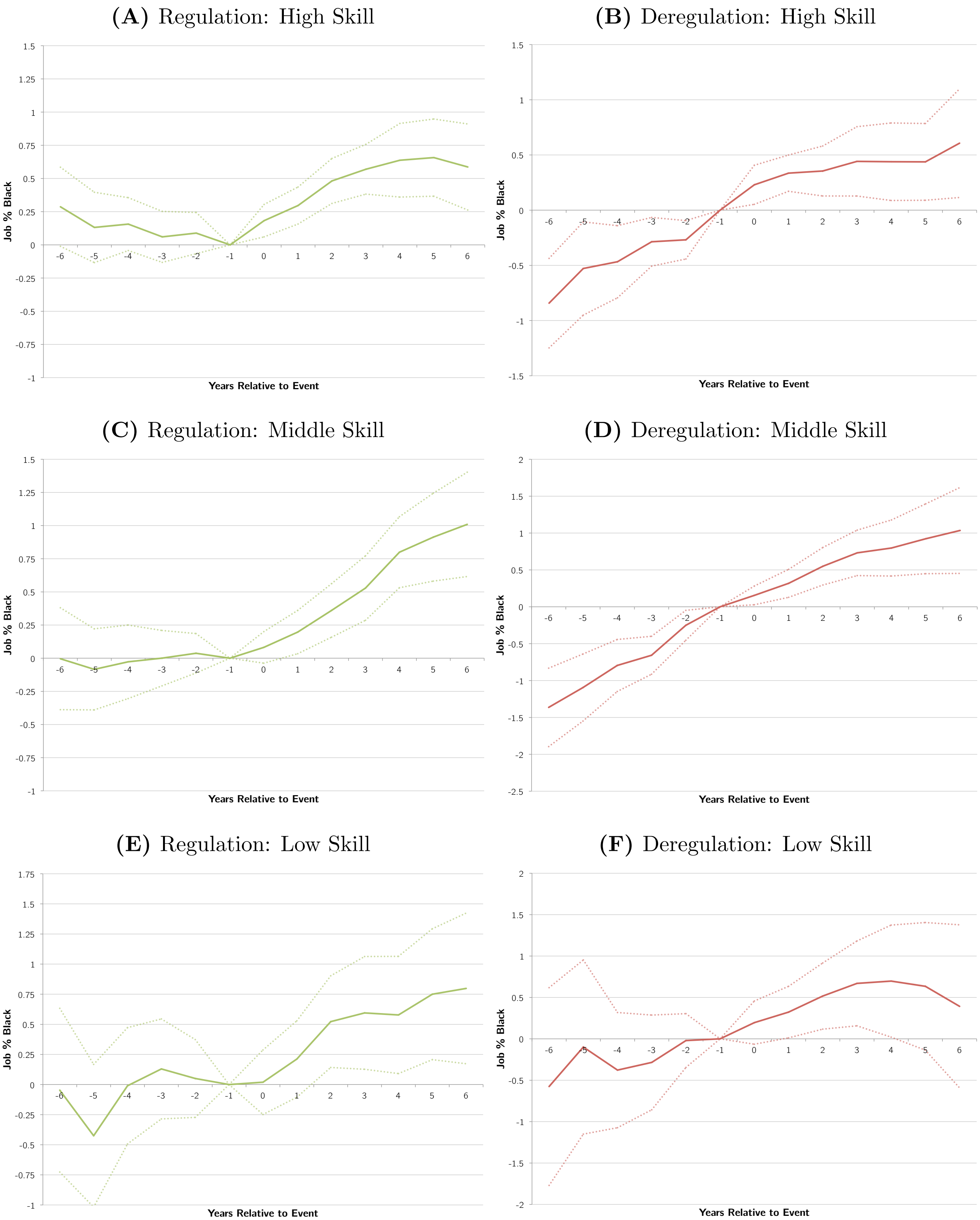


(B) Growing Establishments



Notes: These figures plot the deregulation event study coefficients and 95% confidence intervals for various outcome variables. The definition of deregulation events is described in section II.B. Panels A and B plot estimates of model (4) using only establishments in the overlapping sample that shrink and grow following the deregulation event. See section II.D for details. Pseudo event years are assigned to non-contractors as described in section II.D. The outcome variable for these two panels is the percent black of employees. In all models the coefficient for the year prior to the event (θ_{-1}) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.

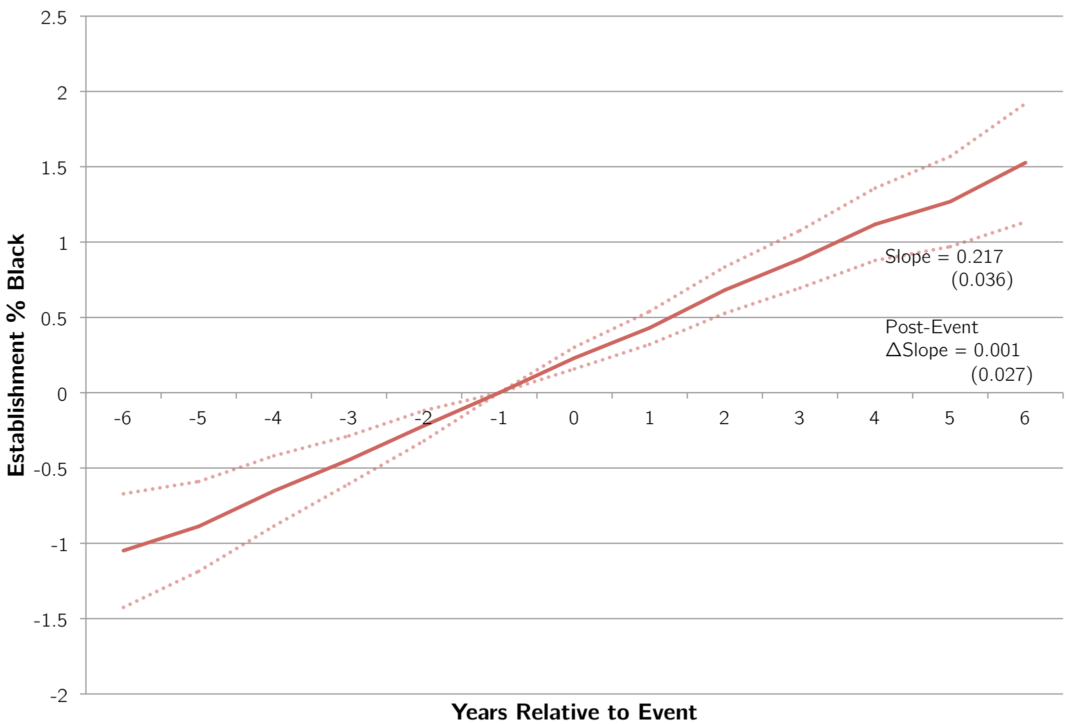
Figure A.7: Regulation and Deregulation Event Studies, by Skill Level



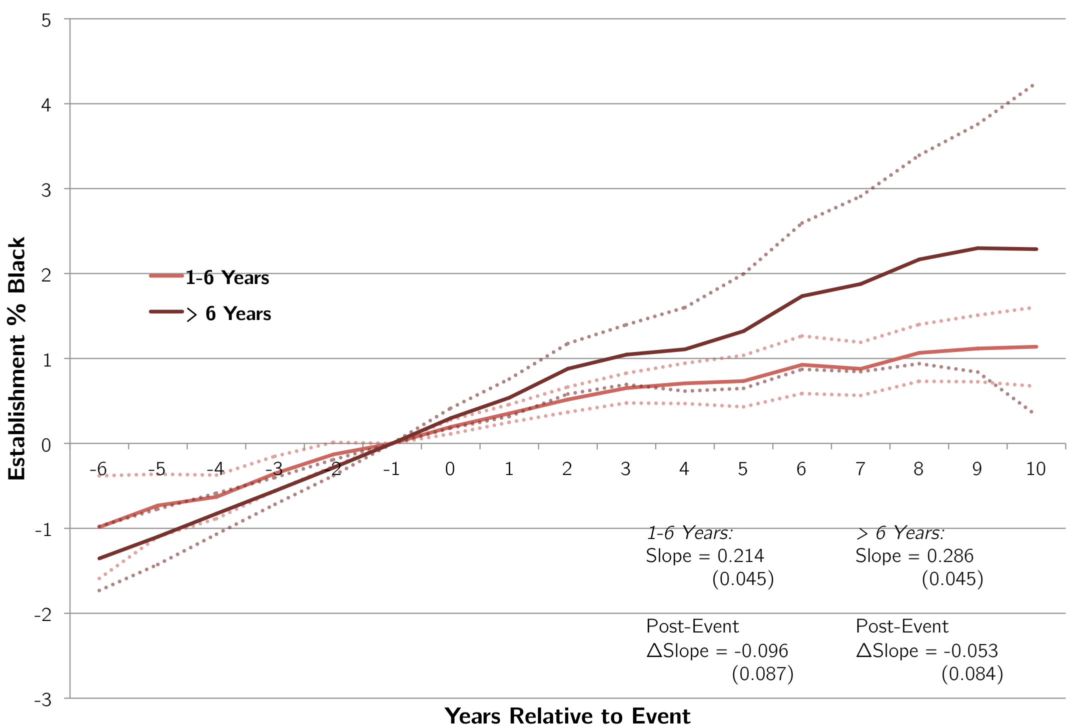
Notes: These figures plot event study coefficients and 95% confidence intervals (dotted) estimated using model (3) and the overlapping sample, where the outcome variable is the percent black of employees in an establishment by occupation cell. The event studies are estimated separately for high skill (managers, professionals, technicians), medium skill (sales workers, administrative support, craft workers, operatives), and low skill (laborers and service workers) occupations. The left column of panels depicts regulation event studies; right column depicts deregulation event studies. The definitions of regulation and deregulation events are described in section II.B. The coefficient for the year prior to the event (θ_{-1}) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level. Observations are weighted by the establishment by occupation cell's share of total establishment employment in the corresponding skill group.

Figure A.8: Fadeout Following Deregulation

(A) Pseudo Deregulation Event Study

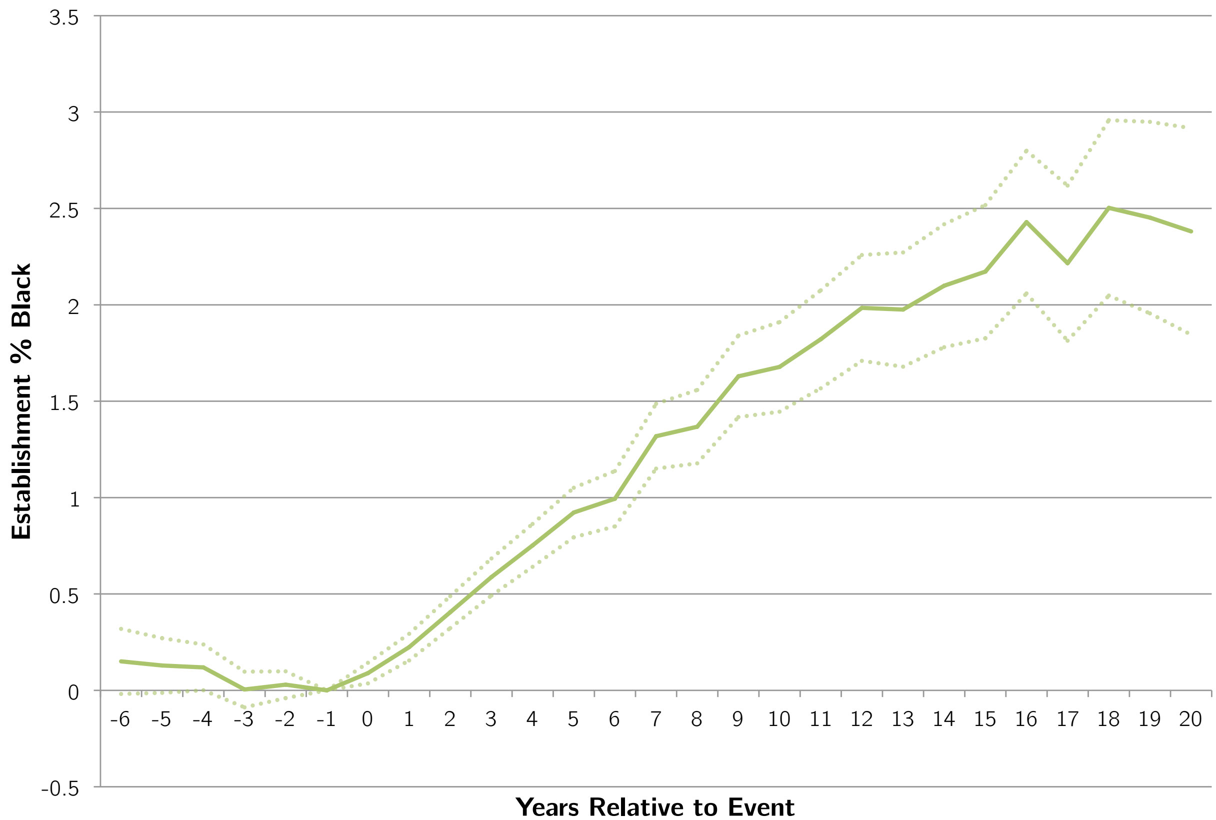


(B) Deregulation Event Study by Duration



Notes: Notes: This figure plots event study coefficients and 95% confidence intervals (dotted) estimated using model (1) and the deregulation sample, where the outcome variable is the percent black of an establishment's employees. In Panel A, one-time contractors are assigned pseudo deregulation event years, as described in section A.4. In Panel B, one-time contractors are grouped by the number of years between their regulation and deregulation events. The definitions of regulation and deregulation events are described in section II.B. The coefficient for the year prior to the event (θ_{-1}) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.

Figure A.9: Long Run Regulation Event Study



Notes: This figure plots regulation event study coefficients and 95% confidence intervals (dotted) estimated using model ((1)) and the regulation sample, where the outcome variable is the percent black of an establishment's employees and endpoint b is extended to 20. The definition of the regulation event is described in section II.B. The coefficient for the year prior to the event (θ_{-1}) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.

Table A.1: EEO-1 Reporting Rates by Industry, 1990

Industry	EEO-1 Coverage Rate (%)	Industry Size (%)
Agricultural Services	22.7	0.5
Mining	90.7	0.4
Construction	11.0	5.7
Manufacturing	63.3	18.6
Transportation, Communications, Utilities	62.3	6.3
Wholesale Trade	19.4	7.2
Retail Trade	28.8	20.5
Finance, Insurance, and Real Estate	43.9	8.3
Services	30.9	32.6
Overall	37.8	100.0

Notes: Coverage rates are calculated by dividing EEO-1 reported employment by County Business Patterns employment totals for the 204 MSAs used in the analysis. Industry size is the fraction of total County Business Patterns reported employment in that industry.

Table A.2: Likelihood of Future Regulation

	All				Experienced			
	Acquires Contract in Next							
	1 Year	3 Years	5 Years	Ever	1 Year	3 Years	5 Years	Ever
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year 1 × Former Contractor	0.048	0.114	0.138	0.127	0.084	0.181	0.218	0.166
	(0.014)	(0.015)	(0.016)	(0.012)	(0.027)	(0.023)	(0.028)	(0.018)
Year 2 × Former Contractor	0.042	0.090	0.133	0.088	0.068	0.163	0.196	0.116
	(0.012)	(0.020)	(0.021)	(0.014)	(0.019)	(0.041)	(0.044)	(0.021)
Year 3 × Former Contractor	0.054	0.072	0.110	0.057	0.095	0.130	0.181	0.065
	(0.016)	(0.017)	(0.023)	(0.013)	(0.025)	(0.038)	(0.041)	(0.019)
Year 4 × Former Contractor	-0.005	0.040	0.047	0.018	-0.003	0.016	0.064	0.002
	(0.000)	(0.019)	(0.024)	(0.014)	(0.014)	(0.040)	(0.050)	(0.026)
Year 5 × Former Contractor	0.023	0.054	0.048	0.022	0.028	0.070	0.059	0.003
	(0.016)	(0.026)	(0.027)	(0.017)	(0.039)	(0.060)	(0.060)	(0.035)
Year 6 × Former Contractor	0.031	0.048	0.041	0.039	-0.018	0.055	0.049	0.008
	(0.017)	(0.024)	(0.025)	(0.018)	(0.019)	(0.052)	(0.061)	(0.038)
Year 7 × Former Contractor	0.010	0.033	0.026	0.029	0.061	0.084	0.086	0.037
	(0.016)	(0.022)	(0.024)	(0.020)	(0.048)	(0.056)	(0.070)	(0.050)
Year 8 × Former Contractor	0.020	0.022	0.060	0.032	0.043	0.043	0.059	-0.002
	(0.015)	(0.020)	(0.027)	(0.020)	(0.034)	(0.045)	(0.062)	(0.037)
Year 2	-0.020	-0.011	-0.008	0.009	-0.020	-0.011	-0.007	0.009
	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)
Year 3	-0.016	-0.014	-0.009	0.014	-0.016	-0.014	-0.009	0.015
	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)
Year 4	-0.023	-0.030	-0.023	0.010	-0.024	-0.030	-0.023	0.011
	(0.005)	(0.007)	(0.007)	(0.006)	(0.005)	(0.007)	(0.007)	(0.006)
Year 5	-0.035	-0.047	-0.031	0.004	-0.034	-0.045	-0.028	0.008
	(0.005)	(0.008)	(0.009)	(0.007)	(0.005)	(0.008)	(0.009)	(0.007)
Year 6	-0.039	-0.068	-0.053	-0.017	-0.039	-0.065	-0.049	-0.013
	(0.005)	(0.008)	(0.011)	(0.008)	(0.005)	(0.008)	(0.011)	(0.008)
Year 7	-0.051	-0.085	-0.068	-0.026	-0.049	-0.078	-0.064	-0.022
	(0.005)	(0.010)	(0.013)	(0.009)	(0.005)	(0.09)	(0.013)	(0.010)
Year 8	-0.061	-0.083	-0.073	-0.024	-0.060	-0.079	-0.073	-0.022
	(0.007)	(0.011)	(0.013)	(0.010)	(0.006)	(0.011)	(0.014)	(0.010)
Division by Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
N Observations	836,625	736,595	646,244	836,625	698,967	616,620	546,132	698,967

Notes: Each column reports the coefficient estimates for a regression, with standard errors in parentheses clustered at the firm level. Columns (1), (4), (5), and (8) include data from 1979 to 2003. Columns (2) and (7) include data from 1979 to 2001. Columns (3) and (7) include data from 1979 to 1999. Columns (5) through (8) limit former contractors to those who have been previously observed as contractors for at least 3 years.

Table A.3: Regulation and Deregulation Event Studies, by Employer Size

	All	Firm Size ≥ 150	Single Establishment	Multi Establishment	Small Establishment	Large Establishment
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Regulation Event						
β	-0.006	-0.008	-0.027	-0.022	0.002	-0.017
	(0.029)	(0.031)	(0.032)	(0.030)	(0.042)	(0.027)
$\Delta\beta$	0.182	0.190	0.017	0.202	0.170	0.192
	(0.040)	(0.042)	(0.037)	(0.039)	(0.054)	(0.050)
Panel B: Deregulation Event						
β	0.318	0.314	-0.030	0.314	0.367	0.266
	(0.046)	(0.046)	(0.041)	(0.047)	(0.058)	(0.056)
$\Delta\beta$	-0.149	-0.141	0.014	-0.135	-0.227	-0.074
	(0.046)	(0.048)	(0.046)	(0.050)	(0.054)	(0.061)
Div. $\times$ Year FEs	✓	✓	✓	✓	✓	✓
Est. FEs	✓	✓	✓	✓	✓	✓
# of Treated Est.	36,030	33,314	4,703	31,327	18,519	17,511

Notes: Each column reports the coefficient estimates for a regression, with standard errors in parentheses clustered at the firm level. The estimated models are parametric regulation and deregulation event studies in Panel A and Panel B. The definition of regulation and deregulation events is described in section II.B. The estimation sample in column (1) includes non-contractor establishments and the overlapping sample. Column (2) restricts the analysis to establishments that are part of firms with at least 150 employees. Column (3) restricts the analysis to singleton establishments, and column (4) restricts the analysis to establishments that are part of multi-establishment firms. Column (5) restricts the analysis to establishments with less than 100 employees, and column (6) restricts the analysis to establishments with at least 100 employees. All columns include Census division by year fixed effects. For eventual contractor establishments, these restrictions are based on the latest year an establishment is observed prior to their regulation event. For non-contractor establishments, these restrictions are based on the latest year an establishment is observed prior to their *pseudo* regulation event, where pseudo event events are assigned based on the year I first observe the establishment in the data and the number of years between the first and last year. More details on how pseudo events are assigned are described in section II.D.

Table A.4: Temporary Deregulation Sample, Summary Statistics

	Temporary Deregulation Sample	Overlapping Sample
Number of Establishments	16,430	36,030
Number of Firms	3,774	8,532
Establishment Size*	192	170
	(337)	(307)
Industry (%)		
Agricultural Services	0.2	0.2
Mining	0.2	0.2
Construction	1.1	0.9
Manufacturing	13.3	12.0
Transportation, Comm., Util.	4.6	4.3
Wholesale Trade	3.7	3.4
Retail Trade	49.9	51.0
Finance, Insurance, Real Estate	6.2	7.5
Services	20.8	20.6
Black Share Quantile*	48.8	47.8
Standardized Black Share*		
Mean	-0.021	-0.020
Median	-0.325	-0.340
Black Share of Employees* (%)	13.2	13.5
Black Share of Population, 15-64* (%)	14.8	15.0

Notes: Standard deviation in parentheses.
* Quantiles and normalizations defined at level of MSA by year cell. This is quantity at last year observed before regulation event.

Table A.5: Robustness Checks: Employer Size and Percentage Black

Outcome: Δ Percentage Black	Within-Job				
	All	Recession	Expansion	Non-Contractors	Contractors
	(1)	(2)	(3)	(4)	(5)
Δ log Establishment Size	0.781	0.746	0.801	0.873	0.757
	(0.024)	(0.037)	(0.030)	(0.053)	(0.027)
MSA by Year FEs	✓	✓	✓	✓	✓
R^2	0.002	0.001	0.002	0.002	0.002

Notes: Each column reports the coefficient estimates for a regression, with standard errors in parentheses clustered at the establishment level. The outcome variable is the change in percent black of employees in a establishment by occupation cell over the previous year. All columns include MSA by year fixed effects. Column (2) includes only data from the years 1980-1982, 1990-1992, and 2001-2003. Column (3) includes only the remaining years. Column (4) includes only observations for establishments that have not previously held a federal contract. Column (5) includes only observations for establishments that have previously held a federal contract.

B Persistence Introduces Bias

The persistent effect of temporary regulation I document here has important implications for interpreting existing research in this literature, including Kurtulus (2016, 2012), Leonard (1984, 1990), Rodgers and Spriggs (1996), Ashenfelter and Heckman (1976), Goldstein and Smith (1976), Smith and Welch (1984), and Heckman and Wolpin (1976). In particular, if regulation has an impact on employers that persists even when they are no longer contractors, previous estimates may be biased. This is because the research designs applied in existing work are based on simple comparison of contractors to non-contractors, either within or across employers. In the presence of persistence these comparisons may substantially understate the causal impact of regulation because some employers that are currently non-contractors were previously contractors, and the minority share of those employers is still affected by the regulation. In this section, I explore the extent of this bias empirically, using the baseline model of Kurtulus (2016) as a motivating example.

The core specification estimated in Kurtulus (2016) is of the form

$$\text{black share}_{it} = \alpha_i + \tau_{r(i),t} + \beta I_{it}^{current} + X_{it}\gamma + \epsilon_{it} \tag{B.1}$$

where $I^{current}$ is an indicator for whether an establishment is currently a contractor. This specification models the effect of regulation as a level effect that depends only on the current period contractor status. An assumption implicit in this model is that whatever effect regulation has dissipates completely when an employer is no longer a contractor.

For the sake of comparison, I also estimate a modified version of (B.1),

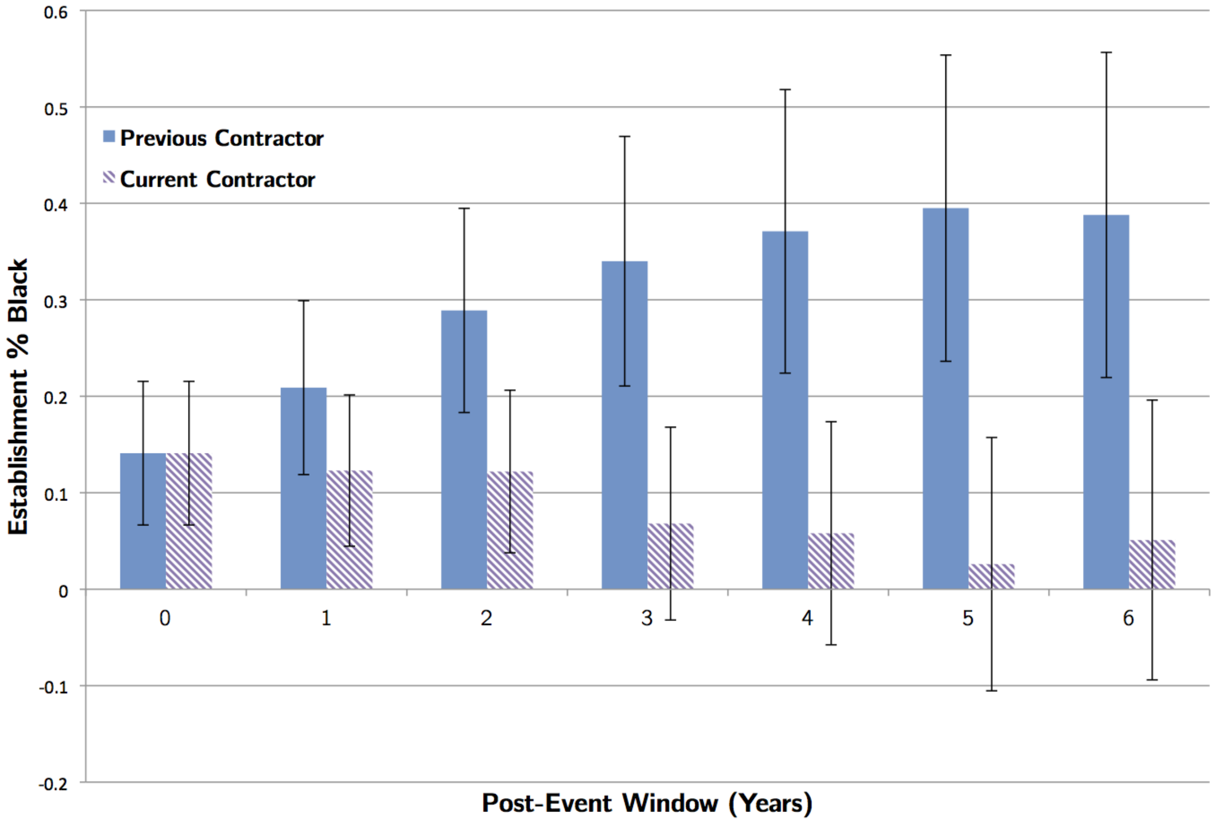
$$\text{black share}_{it} = \alpha_i + \tau_{r(i),t} + \tilde{\beta} I_{it}^{previous} + X_{it}\gamma + \epsilon_{it} \tag{B.2}$$

where $I^{previous}$ is an indicator for whether an establishment has *ever* previously been a contractor. This specification models the effect of regulation as a level effect that depends only on whether the establishment was ever previously a contractor. While this specification does not allow the effect of regulation to accumulate over time, a pattern I document in the main analysis, it does allow for a particular form of persistence. If the effect of regulation takes the form assumed in (B.1), this model will underestimate the effect of regulation.

As in the main analysis, I exclude establishments that enter the sample as a federal contractor. In addition, for the establishments that become contractors, I only include years of data that are at most 6 years prior to their regulation event. To demonstrate the influence of persistence on the results, I estimate both (B.1) and (B.2) for a series a estimation samples, moving the data window from the year of the regulation event to 6 years following the regulation event. For each sample, I also restrict the set of eventual contractors included in the estimation to those that are present for the full set of years following the regulation event. This way, the $\tilde{\beta}$ estimates reflect, in principle, the impact of regulation averaged across 0-6 years following the event, and not a more complicated weighted average that depends on the frequency with which establishments are observed at each year following regulation. Each model includes Census division by year fixed effects, and a quadratic in log establishment size.

The β and $\tilde{\beta}$ estimates for each window are displayed in Figure B.1. For the smallest window, which excludes all years following the regulation event, the estimates coincide at 0.141. As the window widens, these estimated coefficients diverge sharply. The β coefficient declines to 0.068 when the window expands to three years after the event, and is statistically indistinguishable from zero. Using the full size year window, the β coefficient declines further to 0.051. This pattern emerges despite the fact that the effect of initial regulation *increases* over time, as demonstrated in the main analysis. This discrepancy reflects the fact that many establishments are not contractors in some years following their regulation event, but their black share continues to increase. By contrast, the $\tilde{\beta}$ coefficient increases substantially as the window expands. With a three year post-event window, the coefficient has more than doubled to 0.340. Using the full size year window, the $\tilde{\beta}$ coefficient increases further to 0.388. A simple adjustment allowing for some form of persistence increases the estimated effect of regulation by an order of magnitude.

Figure B.1: Persistence and Bias



Notes: Each bar represents the coefficient estimates for a regression, along with a 95% confidence interval, with standard errors clustered at the firm level. The purple bars depict β coefficient estimates for (B.1). The blue bars depict $\tilde{\beta}$ coefficient estimates for (B.2). The estimation samples exclude establishments that enter the sample as a federal contractor. For the establishments that become contractors, I only include years of data that are at most 6 years prior to their regulation event. The 'Post-Event Window' corresponds to different estimation samples. I estimate both (B.1) and (B.2) for a series a estimation samples, moving the data window from the year of the regulation event to 6 years following the regulation event. For each sample, I also restrict the set of eventual contractors included in the estimation to those that are present for the full set of years following the regulation event. Each model includes Census division by year fixed effects, and a quadratic in log establishment size.