

Supplementary Materials

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ADDITIONAL LITERATURE REVIEW

Conceptualizations of the firm

Coase (1937, pg. 393) writes, “A firm, therefore, consists of the system of relationships which comes into existence when the direction of resources is based on the entrepreneur.” Firms are *inherently relational organizations of production* that Coase (1937) notes lies outside of the price mechanism. Importantly, even Coase (1937) concedes that firms are dictatorial in the sense that entrepreneurs use their power to command their employees to produce a certain amount of goods. In other words, the entrepreneur has property rights over the residual labor of their employees (Grossman and Hart 1986; Hart and Moore 1990).

In contrast to Coase (1937), more conventional theories of the firm maintain that the firm is a “team” of relationships to coordinate the translation of inputs into outputs (Alchian and Demsetz 1972). Rather than some exercise in dictatorial power, modern views of the firm see it as a set of free, contractual relationships between employees, managers, and owners (Alchian and Demsetz 1972; Rajan and Zingales 1998, 2002). This contractarian view of the firm rests on the argument that firms form rationally to organize production efficiently in the face of transaction costs in the marketplace. Firms are still relationships of production, but where employees rationally and voluntarily enter into contracts that specify the terms of employment. In other words, this view posits that individuals submit themselves to hierarchical authority in the workplace in the hopes that the employment contract will serve as an economic magna carta to check the arbitrary power of the king (management/ownership).

Modern theories of corporate governance now tend to emphasize shareholder value and the primacy of shareholder democracy (Berle and Means 1932; Jensen and Meckling 1976; Bebchuk 2005; Lazonick and Shin 2020; Fligstein and Goldstein 2022).¹ These theories identify the potential for rent-seeking behavior by managers and executives but instead prescribe maximizing shareholder value as the solution. While this might mitigate agency problems that generate rent-seeking behavior by executives, recent evidence on the rise of institutional investors owning shares in multiple companies demonstrates that concentration in investing generates anti-competitive practices within firms (Azar et al. 2018). This generates situations where shareholders, who are becoming increasingly concentrated (Braun 2021), themselves engage in rent-seeking behavior at the detriment of long-term firm value. But the shareholder democracy goal, even when operating at its ideal, fundamentally assumes that employees enter into employment relationships from a position of freedom.

Concerns about firm market power

First, economists have extensively documented that the concentration of employers (monopsony) limits the ability of workers to leave firms thereby reducing their bargaining power when it comes to wages or benefits and that this has become increasingly important over time (Manning 2003; Ashenfelter et al. 2010; Benmelech et al. 2018; Autor et al. 2019; Azar et al. 2018; Dube et al. 2020). While there is some “natural” tendency for labor market monopsony as a result of job search frictions, the evidence also suggests that forces such as trade policy (Benmelech et al. 2018), “superstar” firms (Autor et al. 2019), and the rise of legal arrangement such as non-competes (Marx 2011; Krueger and Ashenfelter 2018; Balasubramanian et al. 2018; Lipsitz and Starr 2018; Starr et al. 2018) have generated labor market concentration thereby tilting bargaining power away from workers and toward employers.

¹Recent work, explicates how the principle of maximizing shareholder value generates pathological behavior by corporations (Davis 2013; Lazonick and Shin 2020; Davis 2020; Fligstein and Goldstein 2022).

Labor market monopsony is not the only force shaping the balance of power between workers and their employers. Labor coercion, where workers are compelled to provide their labor, has historically been quite violent and racialized (Chwe 1990; Naidu 2010; Acemoglu and Wolitzky 2011). Though it rarely takes on such violent forms through slavery, lynching, or police prosecution, modern-day labor coercion still exists in significant ways. For instance, one estimate suggests that the total value of wage theft—where minimum wage workers are paid below minimum wage—exceeds the value of all property theft in the United States (Meixell and Eisenbrey 2014). More subtly, Hertel-Fernandez (2018) documents how employers use a combination of their coercive power to fire workers and modern surveillance techniques to pressure their employees to engage in political actions that might run counter to their own political beliefs.

Many structural features of the American economy also make coercion a near-endemic feature of labor markets. Because of the way in which social policy, most notably health insurance, has been privatized and tied to employment (Hacker 2004), workers with employer-provided health insurance are forced to choose between their health and their freedom to leave. Other scholars such as Gourevitch (2018) argue that this issue between the freedom to leave and choosing one's livelihood is in fact a defining feature of modern capitalist economies. Because workers without independent sources of wealth, which consists of the vast majority of workers, must work to survive, any participation in labor markets in a capitalist economy is fundamentally coercive. Thus while the contractarian vision of the modern firm presumes that contracts are made under fundamentally free conditions between employers and employees, the nature of wealth distribution today implies that such a decision to even engage in work at all is a product of coercion.

One way of articulating the fundamentally undemocratic nature of the modern firm revolves around republican political theorist's concept of *domination* (Pettit 1999; Anderson 2017).² Lovett and Pettit (2009, pg. 14) characterize domination as follows:

I dominate you in a particular choice to the extent that I have the capacity, not subject to your direct or indirect check, to interfere in the choice, and I can employ that capacity to make it more probable, defiance apart, that you will choose to my pleasure or taste. I may employ my capacity in any of at least three ways:

1. I may exercise the capacity in active interference, trying to push you in my desired direction.
2. I may hold the capacity in reserve against the possibility that you are not disposed to go in the direction I prefer. That is, I may “invigilate” or monitor your choice for that possibility but allow you otherwise to choose as you are inclined.
3. I may make a display of the capacity, thereby intimidating you into making preemptive adjustments in my favor. For example, you may censor your choice so that it conforms to my taste.

If employed in any of these ways, the unchecked capacity to interfere will give me a degree of arbitrary control over your choice.

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²Robert Dahl similarly critiques the nature of the firm, but from the perspective of a distributive democratic framework (Dahl 1986). Dahl argues that because humans are fundamentally equal, they deserve equal claims over decisions in the firm. While we do not take up this logic of workplace democracy in much detail in this article, this claim is testable within the empirical framework outlined in this article. Foley and Polanyi (2006) review other arguments for workplace democracy and add that workplace democracy improves employee health.

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STUDY 1

Study 1: Robustness to Weighting

Regression results in Figure S1 can be found in Tables S27 to S40.

Study 1: Robustness to Marginal Means

Regression results in Figure S2 can be found in Tables S41 to S52.

FIGURE S1. Main effects on all outcomes

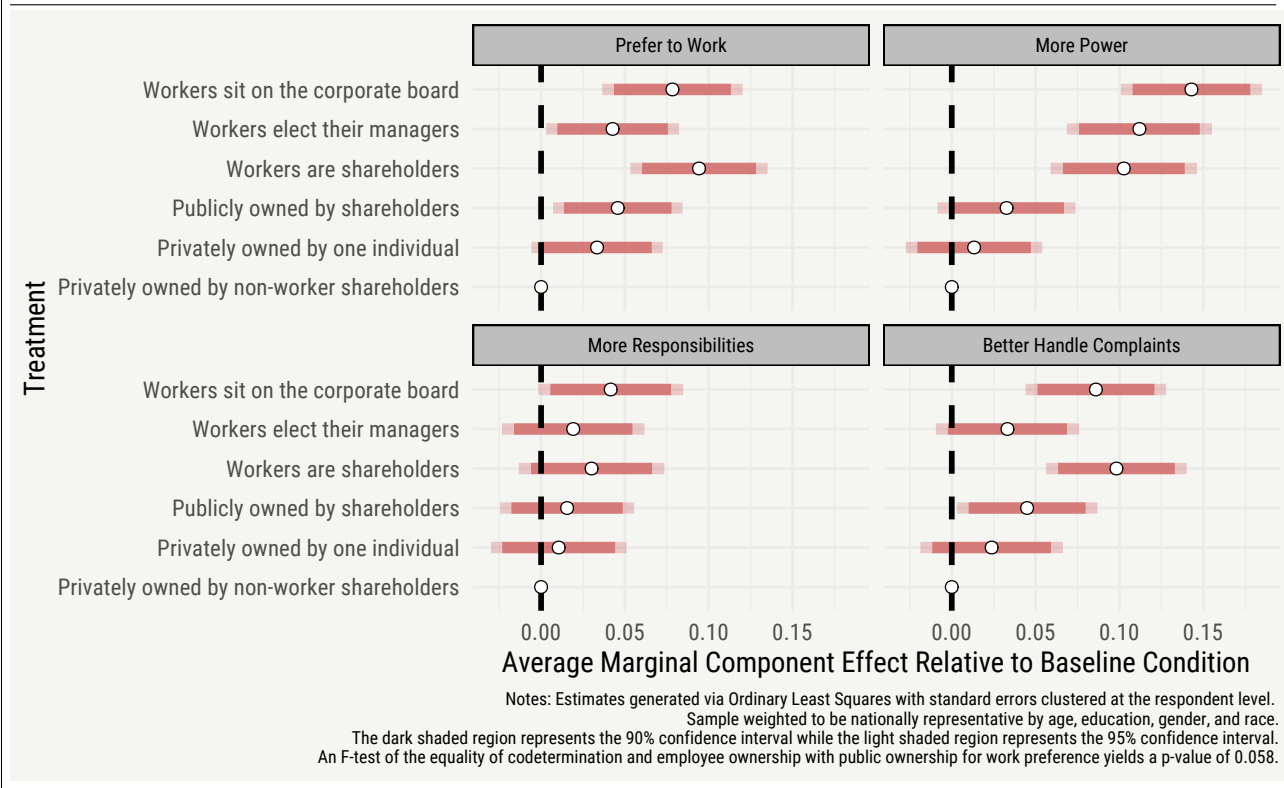
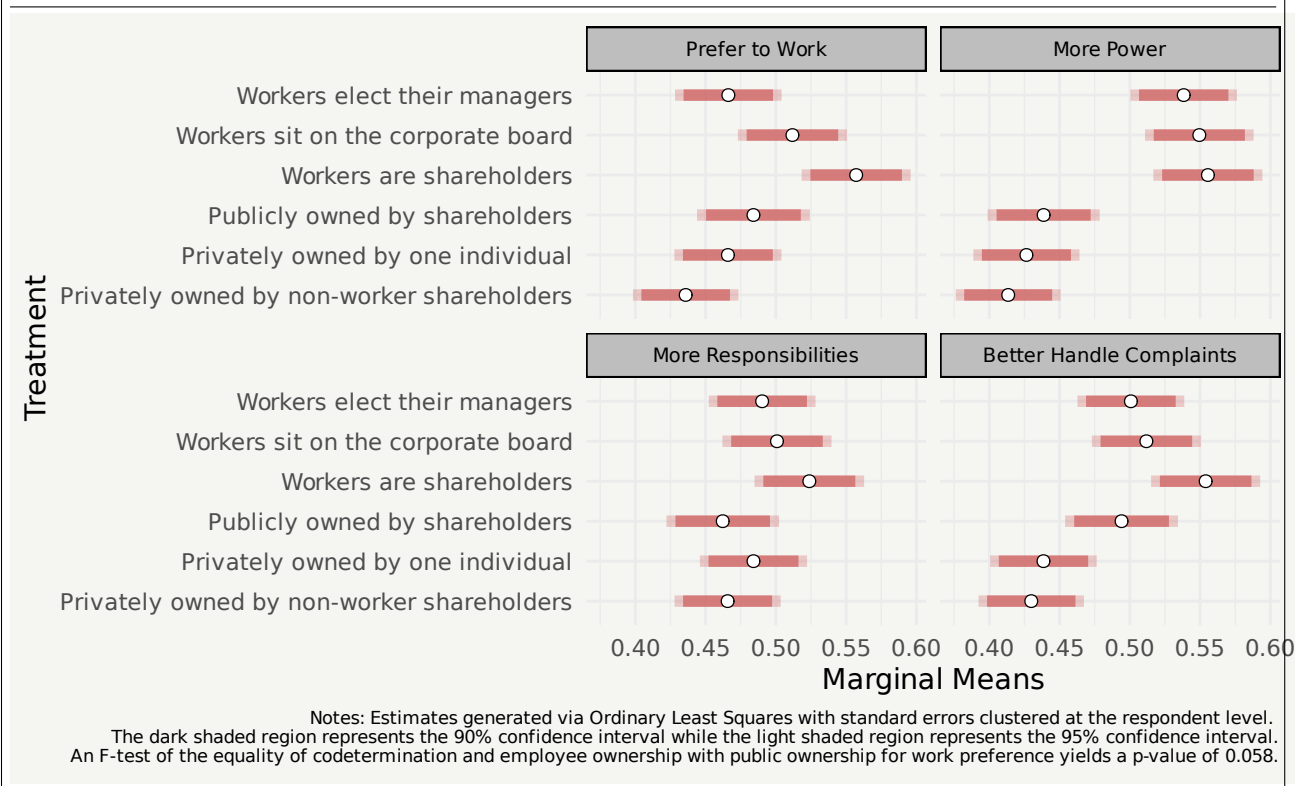


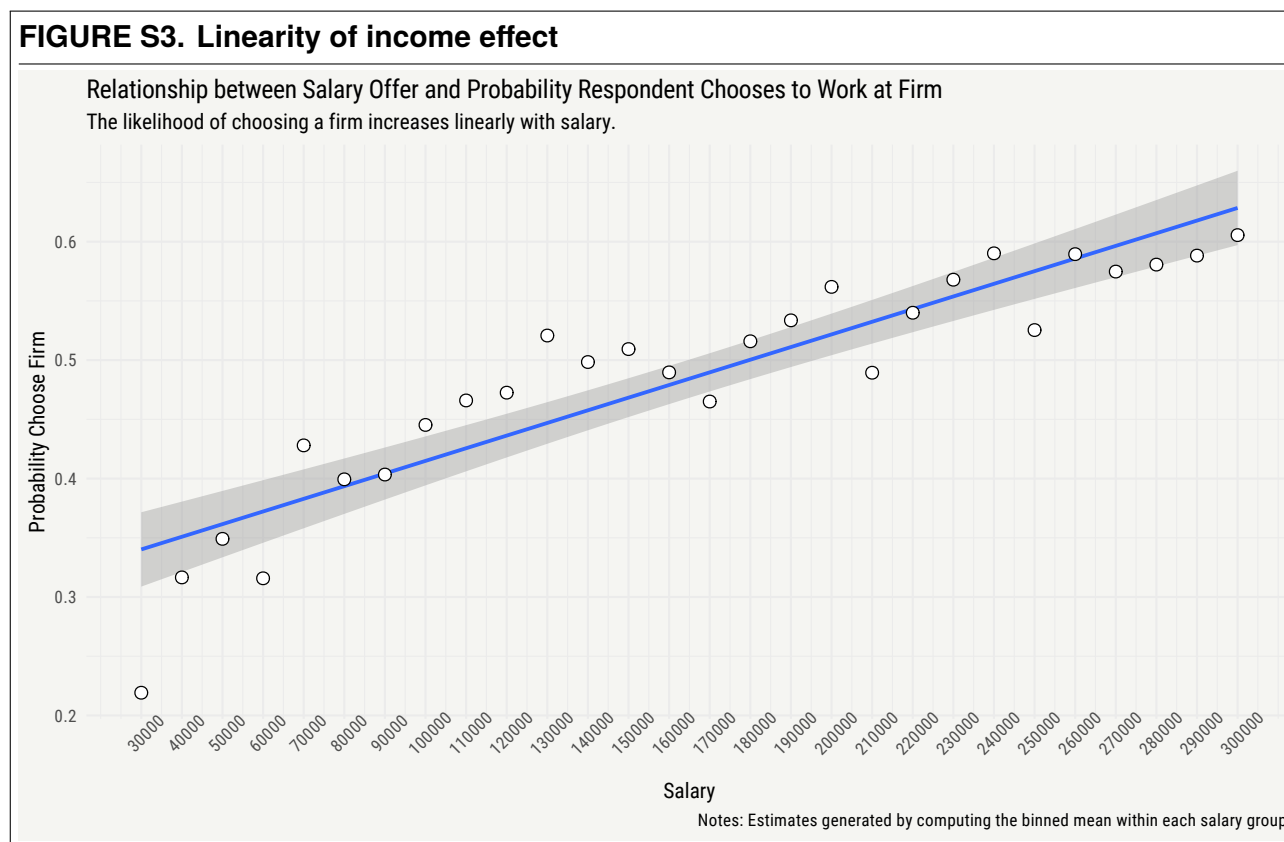
FIGURE S2. Main effects on all outcomes



Study 1: Benchmarking effects to McConnell et al. (2018)

We standardized our prefer to work variable and their wage variable so they were comparable in units of standard deviations. We then compared our firm copartisan donation patterns coefficient against their copartisan coefficient to baseline our workplace democracy coefficients. We focus on the copartisan estimate because they emphasize the coefficient in their work. We then reconvert the standard deviations back to dollars using the standard deviation of respondents' wage demands. We found that respondents value codetermination, manager elections, and ESOPs at \$1.27, \$0.69, and \$1.72 an hour, respectively. In other words, respondents value economic democracy in the range of \$1,400 to \$3,600 in annual income. We redo this same procedure when we subset to only income attributes of \$80,000 or less to rule out the possibility that large incomes drive our results. In this case, our estimates grow to \$1.83, \$1.82, and \$2.70, an hour, for codetermination, management elections, and ESOPs, respectively. We caution readers from over-interpreting the estimate because we use different samples and their study was conducted in the context of online labor markets, like Amazon Mechanical Turk. Additionally, our research designs are very different with weakly comparable control groups. However, the exercise is helpful for mapping our estimate to real-world reservation wages.

Study 1: Linearity of Income Assumption



Study 1: Assessing Mediation Assumptions

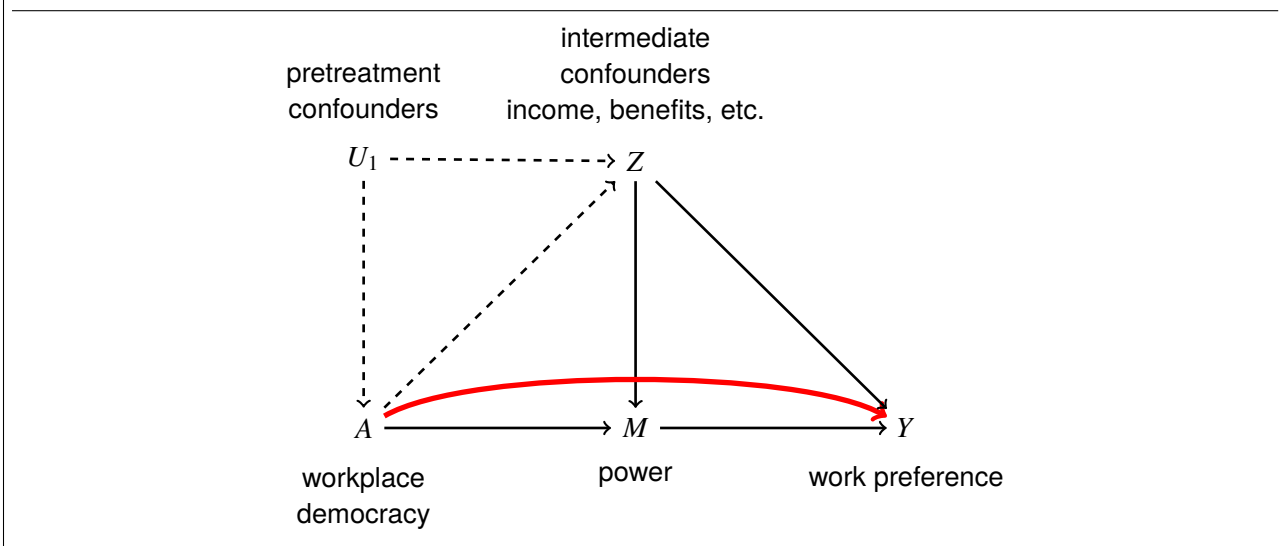
More formally, we conduct a mediation analysis with our identifying assumptions captured in Figure S4 to assess the degree to which intrinsic preferences for more power explain the relationship between workplace democracy and work preferences. Particularly, we aim to estimate the *Average Controlled Direct Effect* (ACDE) as well as the *Average Natural Indirect Effect* (ANIE) of each workplace democracy treatment on the outcome as way to assess whether there remains any direct effect of the treatment after accounting for its impact on perceptions

TABLE S1. Labor Elasticities with Respect to Income

	Work	Work	Power	Power	Responsibilities	Responsibilities	Complaints	Complaints
Intercept	0.33*	0.22*	0.42*	0.37*	0.38*	0.31*	0.42*	0.37*
	[0.31; 0.35]	[0.18; 0.25]	[0.40; 0.44]	[0.33; 0.40]	[0.36; 0.40]	[0.28; 0.35]	[0.40; 0.44]	[0.33; 0.40]
Salary	0.01*		0.00*		0.01*		0.00*	
	[0.01; 0.01]		[0.00; 0.01]		[0.01; 0.01]		[0.00; 0.01]	
Log(Salary)		0.11*		0.05*		0.07*		0.05*
		[0.10; 0.12]		[0.04; 0.06]		[0.06; 0.08]		[0.04; 0.06]
R ²	0.03	0.03	0.01	0.01	0.01	0.01	0.01	0.01
Adj. R ²	0.03	0.03	0.01	0.01	0.01	0.01	0.01	0.01
Num. obs.	8016	8016	8016	8016	8016	8016	8016	8016
RMSE	0.49	0.49	0.50	0.50	0.50	0.50	0.50	0.50

* 0 outside the confidence interval Column titles correspond to each respective dependent variable.

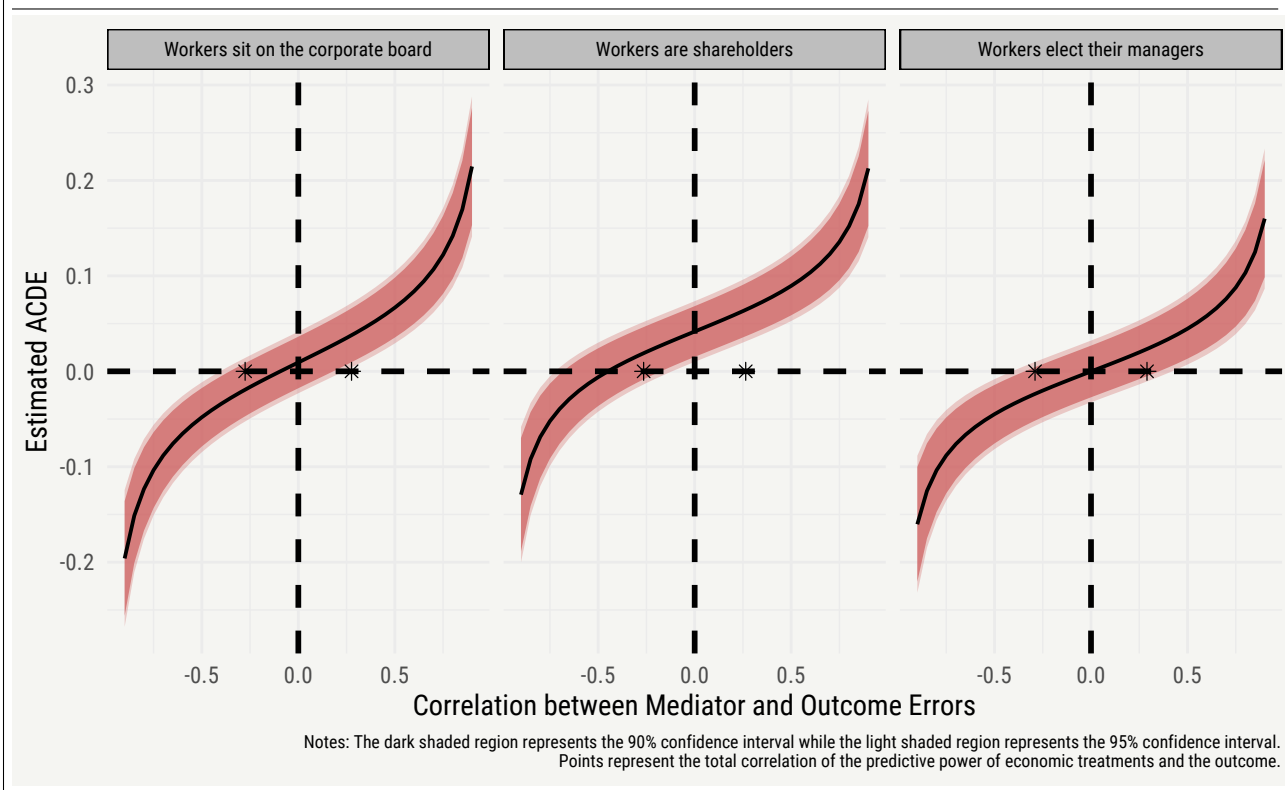
of power (Acharya et al. 2016).³ Our experimental design allows us to recover unbiased estimates of both the ACDE and ANIE (assuming no treatment-mediator interaction) by measuring and randomizing a wide set of potential Z thereby making the sequential unconfoundedness assumption plausible in our setting.

FIGURE S4. Directed Acyclic Graph of Identifying Assumption for Controlled Direct Effects

³The AMCE of each workplace democracy item can be decomposed into the ACDE, the ANIE, and an interaction between the treatment and the moderator. In Table S2 in the Online Appendix, we demonstrate that there is no significant effect moderation by power perceptions for co-determination and management elections thereby allowing us to estimate the ANIE for those treatments. We do find some evidence of effect modification for ESOPs and account for this in our estimates of the ANIE by re-centering as suggested by (Acharya et al. 2016).

TABLE S2. Assessing the interaction effect assumption for causal mediation estimation

	<i>Dependent variable:</i>		
	Codetermination (1)	Work ESOP (2)	Management elections (3)
Workplace dem. proposal	-0.002 (0.023)	0.008 (0.023)	-0.004 (0.023)
Power question	0.512*** (0.024)	0.512*** (0.023)	0.512*** (0.024)
Workplace dem. proposal × Power question	0.017 (0.033)	0.066** (0.033)	0.004 (0.033)
Constant	0.218*** (0.015)	0.218*** (0.015)	0.218*** (0.016)
Observations	2,678	2,652	2,686
R ²	0.273	0.304	0.265
Adjusted R ²	0.272	0.303	0.264
Residual Std. Error	0.426 (df = 2674)	0.417 (df = 2648)	0.428 (df = 2682)
F Statistic	333.915*** (df = 3; 2674)	384.822*** (df = 3; 2648)	322.194*** (df = 3; 2682)
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01		

FIGURE S5. Sensitivity Analysis of Sequential Unconfoundedness Assumption Needed for Mediation Analysis

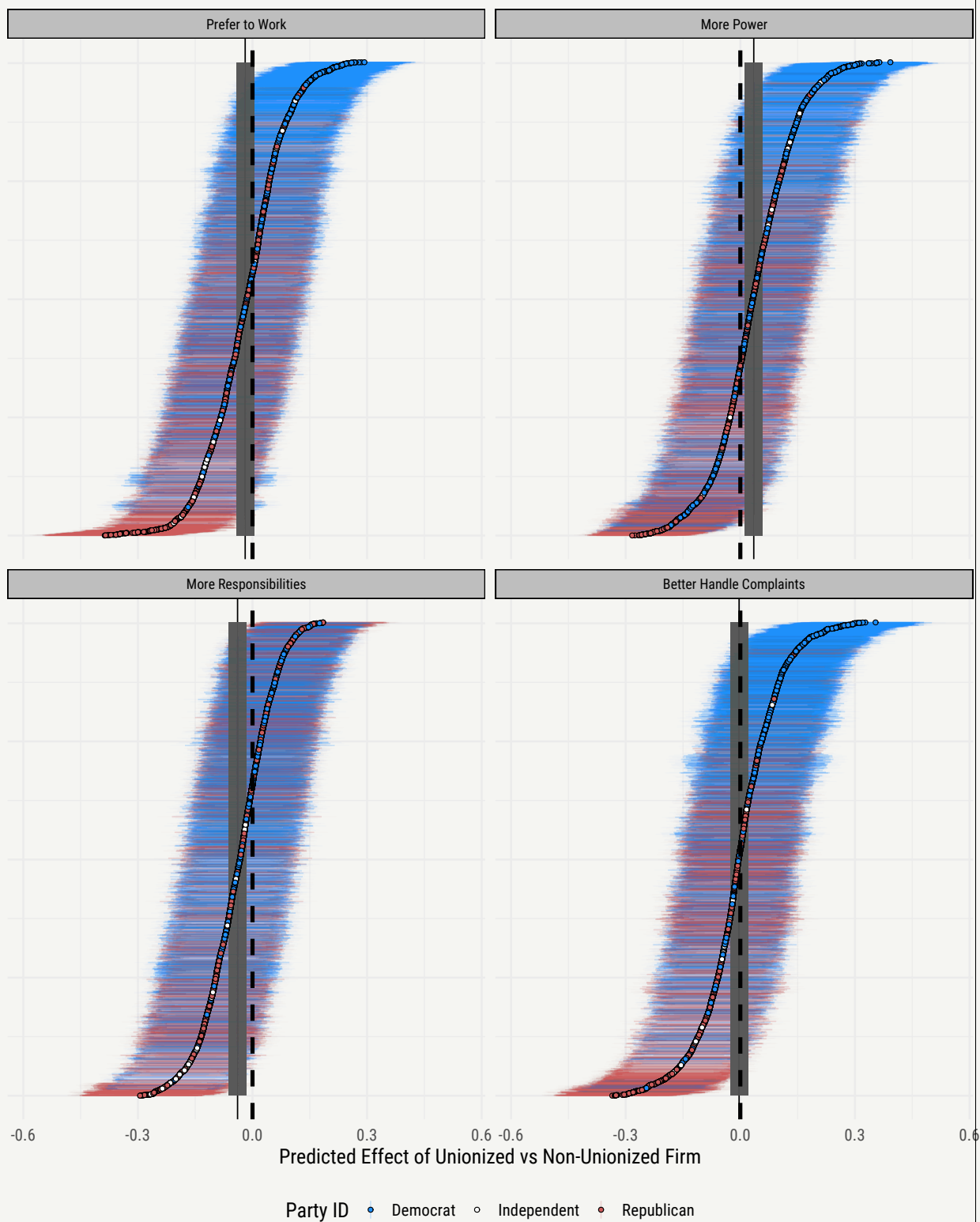
Study 1: Americans Today are Slightly Less Likely to Prefer Union Shops

An alternative (though not necessarily a substitute) form of empowering workers has historically been unions. Do Americans also prefer to work at union shops? Americans have a slight preference away from working at union shops ($p = .12$) despite slightly recognizing that unions would provide them more power ($p = .007$). We note that these effects, again, likely provide lower bounds for the “true” union preference effect since we also manipulate many of the benefits that unions would bring to the workplace with respect to workers. Moreover, recent field experimental evidence suggests that workers not only have preferences for union vs. non-union shops in general, but the specific types of benefits they would receive such as professional benefits as well as community (Hertel-Fernandez and Porter 2021). Thus, we caution against interpreting these results as indicating that unions are not viable forms of labor organizations; instead, public opinion research shows that more Americans want to work in union shops than there are actual workers who are unionized (e.g., Hertel-Fernandez et al. 2019; Kochan et al. 2019).

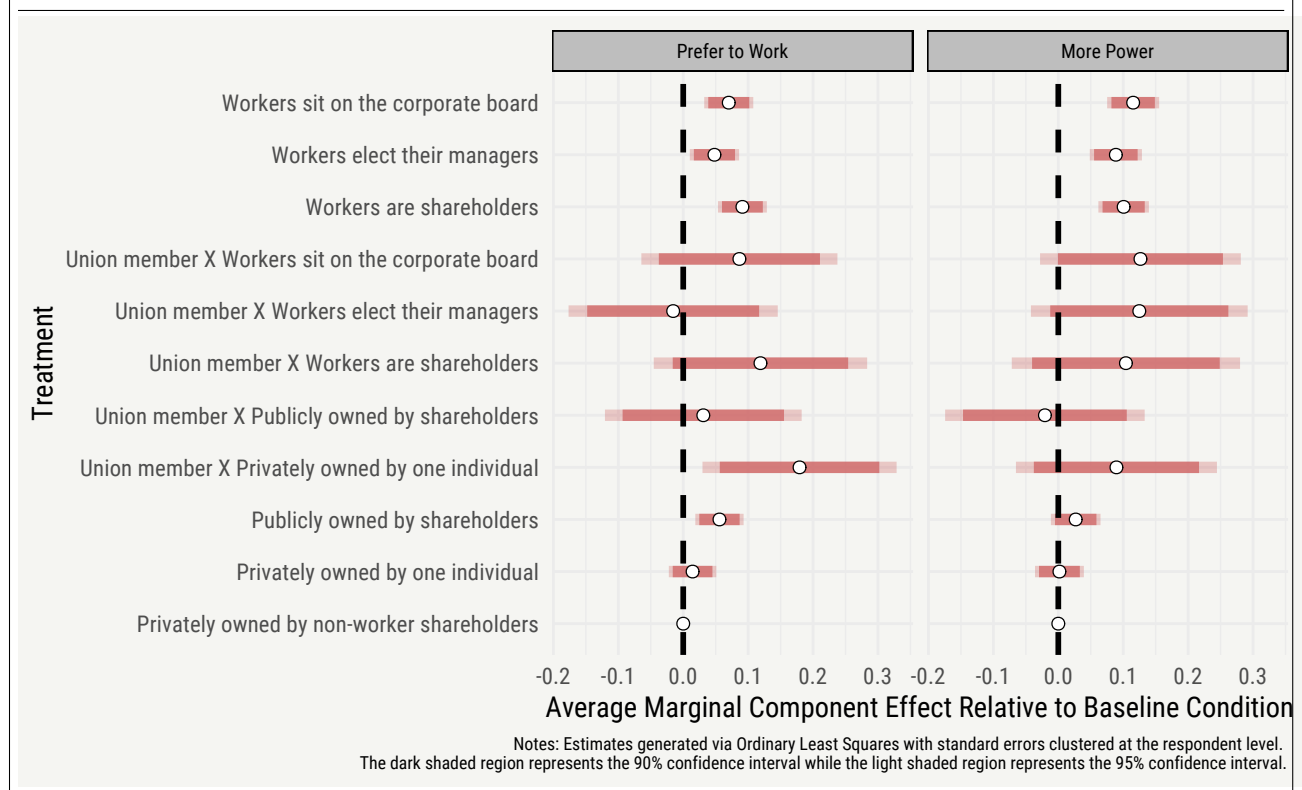
Another reason why we estimate small or even negative effects could be because Americans writ large have lost their faith in unions as an important part of the workplace. We provide evidence that this is likely not the case. These AMCEs, while substantively much lower than the AMCEs associated with workplace democracy proposals, mask a great deal of heterogeneity. The results shown in Figure S6 demonstrate that Democrats want to work at union shops and that they believe that union shops would provide them with more power while Republicans believe the reverse nearly symmetrically. Our results suggest that the partisan polarization around unions relative to other workplace democracy proposals might hinder the mass adoption of labor unions relative to the policies explored in this article.

Regression results for Figure S7 can be found in Tables S53 to S64.

FIGURE S6. Effect of unionization by partisanship.



Notes: Estimates for the treatment effect for each trial where the comparison is between union vs non-union workplaces are ranked by magnitude and generated via causal forests. The thin vertical line represent the estimated causal effect of each treatment with the vertical shaded regions representing the 95% confidence interval. The horizontal shaded regions represent 95% confidence intervals of individual level treatment effect estimates.

FIGURE S7. Interaction effect of different workplace regimes with union membership.

Study 1: Objective Class Moderates Support for Workplace Democracy

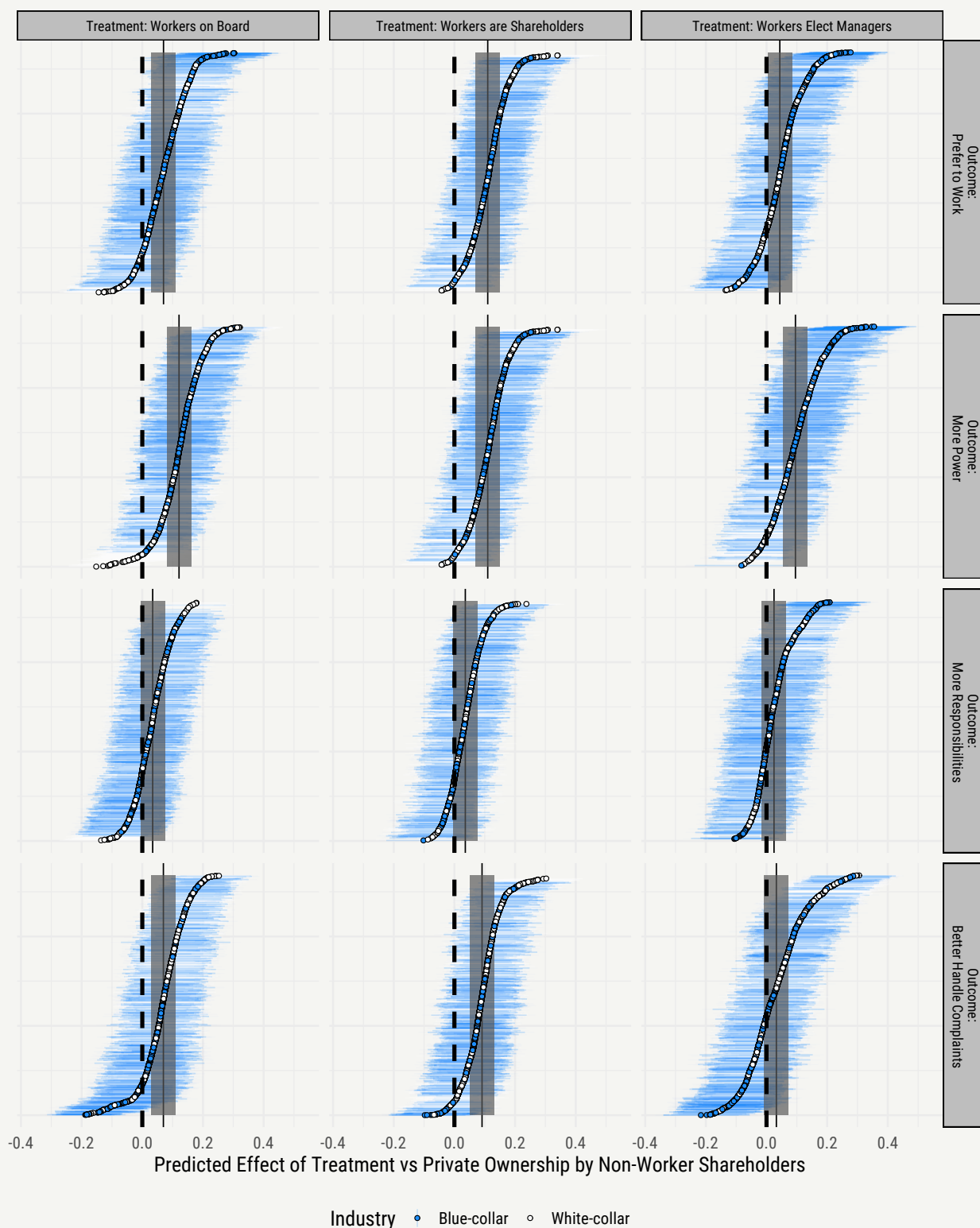
Workplace democracy preferences might vary by objective class position. We show this variation across occupation, education, and income. We present the plots in the Appendix to save space. Figure S8 shows that blue-collar workers prefer to work at democratic workplaces more than white-collar workers. Additionally, blue-collar workers were more likely to believe that democratic workplaces would empower workers more. Blue and white-collar workers similarly believed that democratized workplaces came with more workplace responsibilities and that they would better solve workplace complaints. These effects were not statistically significant, but they were in the right theoretical direction.

More highly educated workers seem to prefer workplace democracy. Figures S10 and S9 show that more highly educated workers tend to prefer to work at democratized workplaces. They also seem to believe that democratized workplaces will empower workers and better handle complaints while still recognizing that these workplaces will place more responsibilities on workers. However, most of these effects are not statistically significant.

Higher-income respondents generally opposed democratizing workplaces. Figure S11 shows that higher-paid workers strongly oppose working at firms with codetermination and manager elections compared to lower-paid workers, however, higher-paid workers are just as likely to prefer working for firms using ESOPs as lower-paid workers. Interestingly, they were not more likely to believe that democratizing the workplace would empower workers more. Additionally, they were not more likely to think that democratizing the workplace would lead to firms burdening workers with responsibilities or would better handle workplace complaints. All of the results, besides firm preference, were not statistically significant. In sum, we found that objective class seemed to moderate preferences for workplace democracy.

Regression results for Figure S10 are reported in Tables S65 to S88. Regression results for Figure S11 are reported in Tables S89 to S112. Regression results for Figure S12 are reported in Tables S113 to S124. Regression results for Figure S13 are reported in Tables S125 to S134. Regression results for Figure S14 are reported in Tables S135 to S146.

FIGURE S8. Interaction effect of different workplace regimes by industry.



Notes: Estimates for the treatment effect for each trial where the comparison is against private ownership by non-worker shareholders are ranked by magnitude and generated via causal forests. The thin vertical line represent the estimated causal effect of each treatment with the vertical shaded regions representing the 95% confidence interval. The horizontal shaded regions represent 95% confidence intervals of individual level treatment effect estimates.

FIGURE S9. Interaction effect of different workplace regimes by college education.

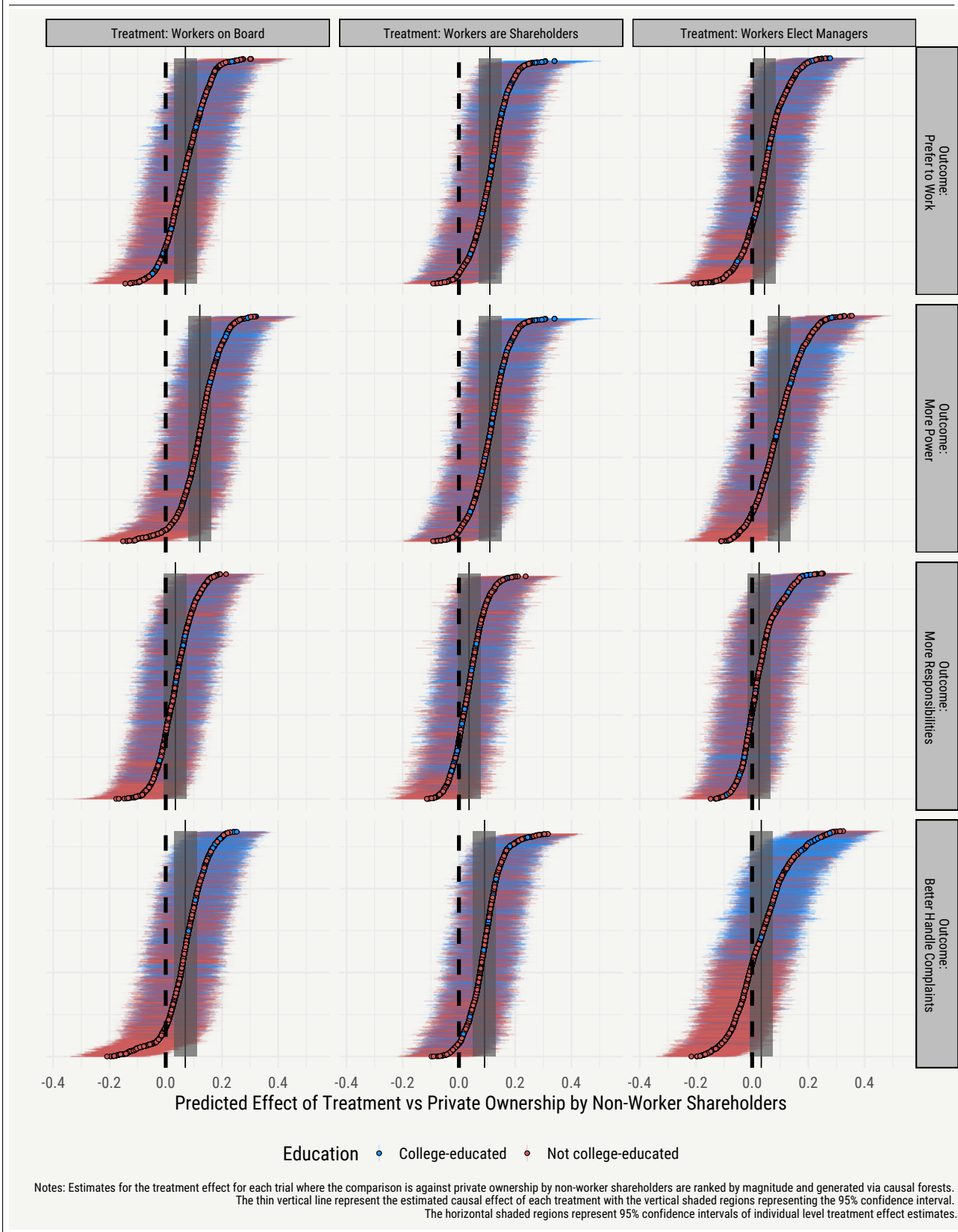


FIGURE S10. Interaction effect of different workplace regimes by education level.

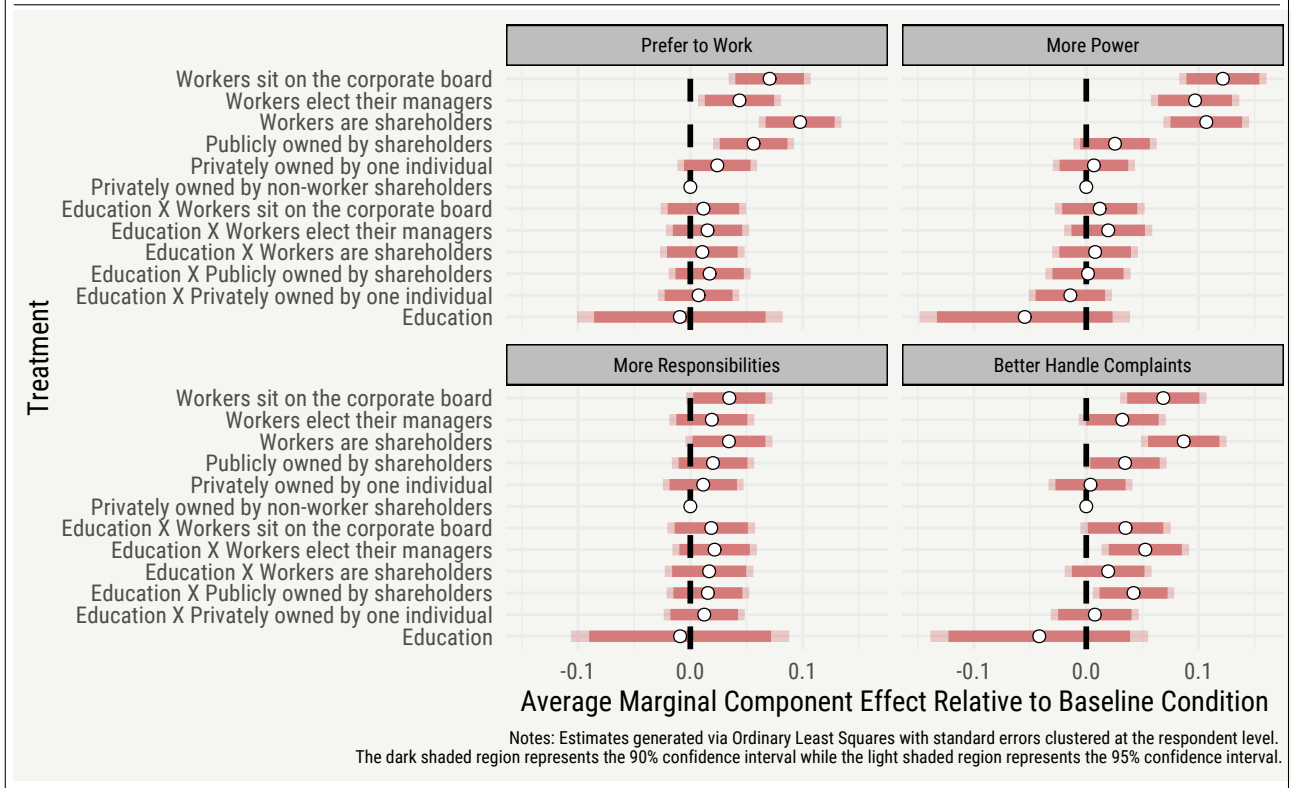


FIGURE S11. Interaction effect of different workplace regimes by income level.

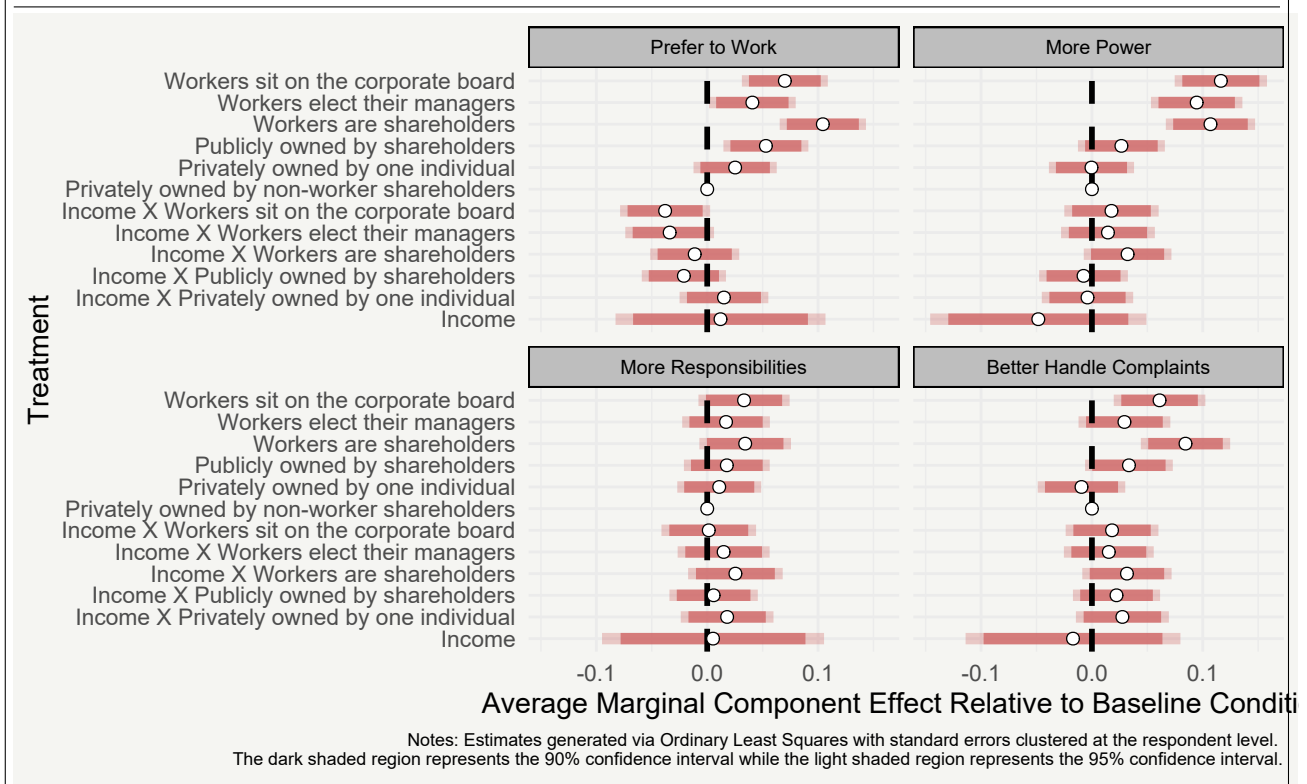


FIGURE S12. Main effects when subsetting out income conjoint attributes over \$200,000.

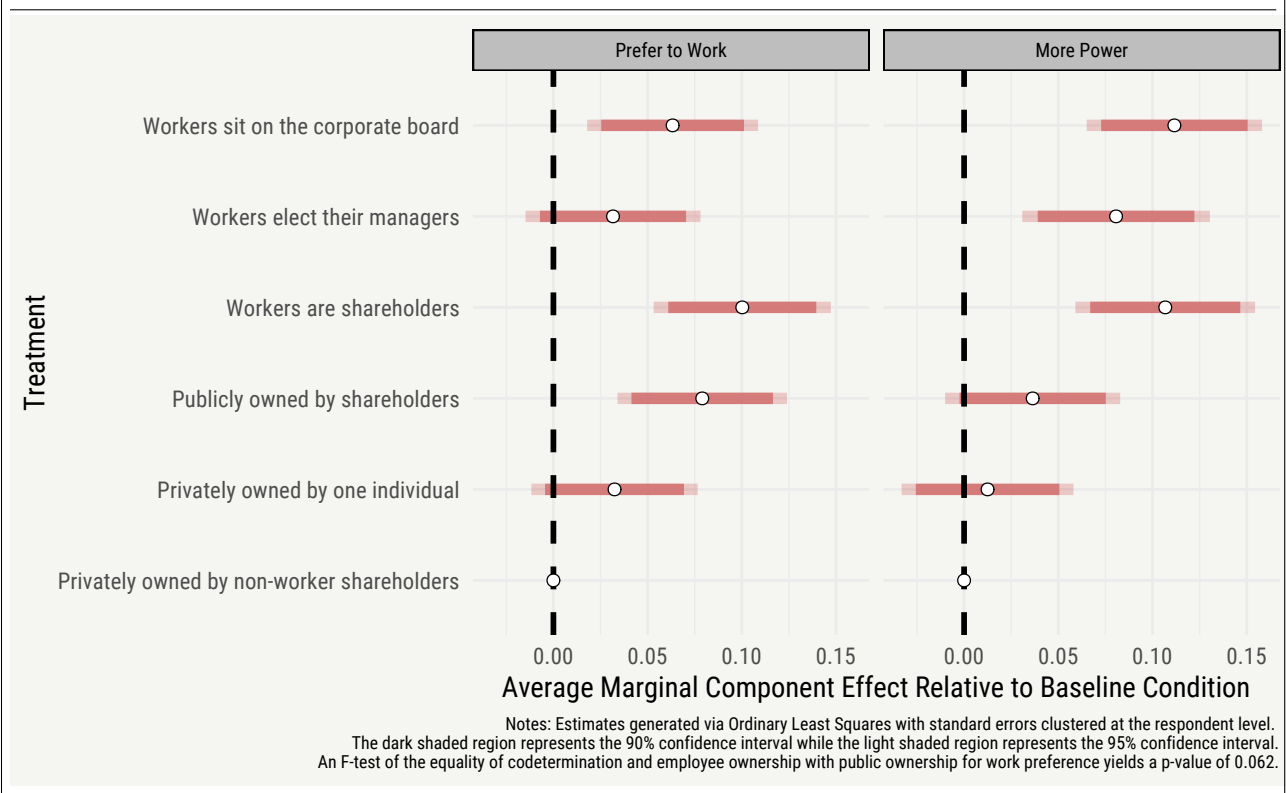


FIGURE S13. Main effects when subsetting out income conjoint attributes over \$80,000.

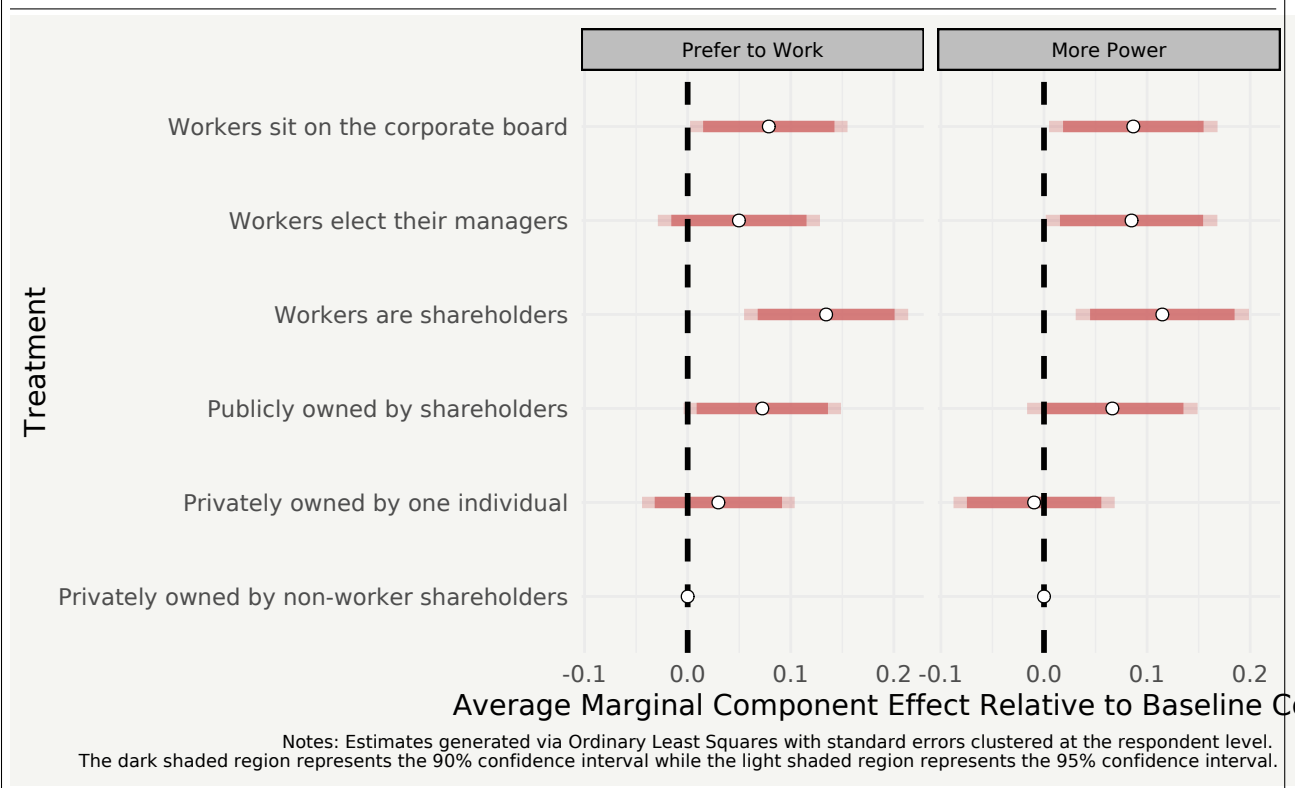
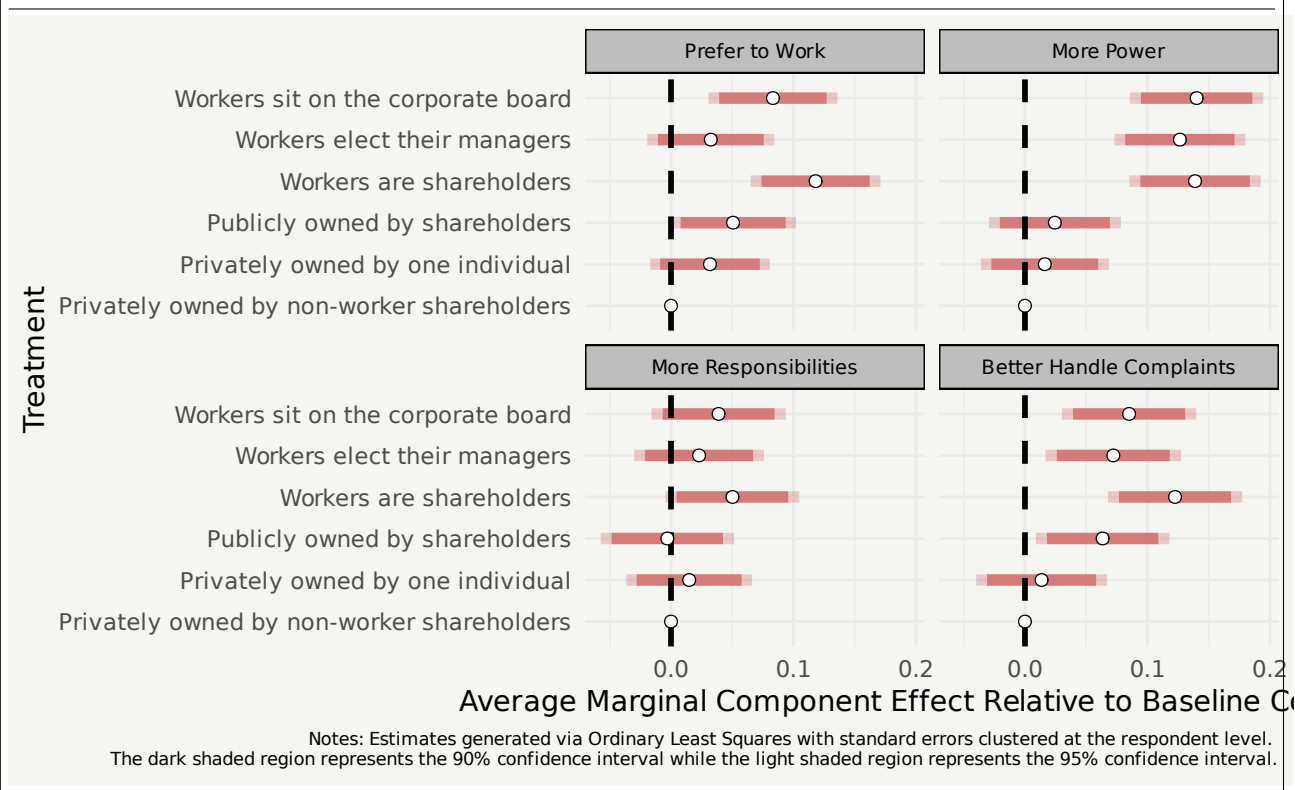
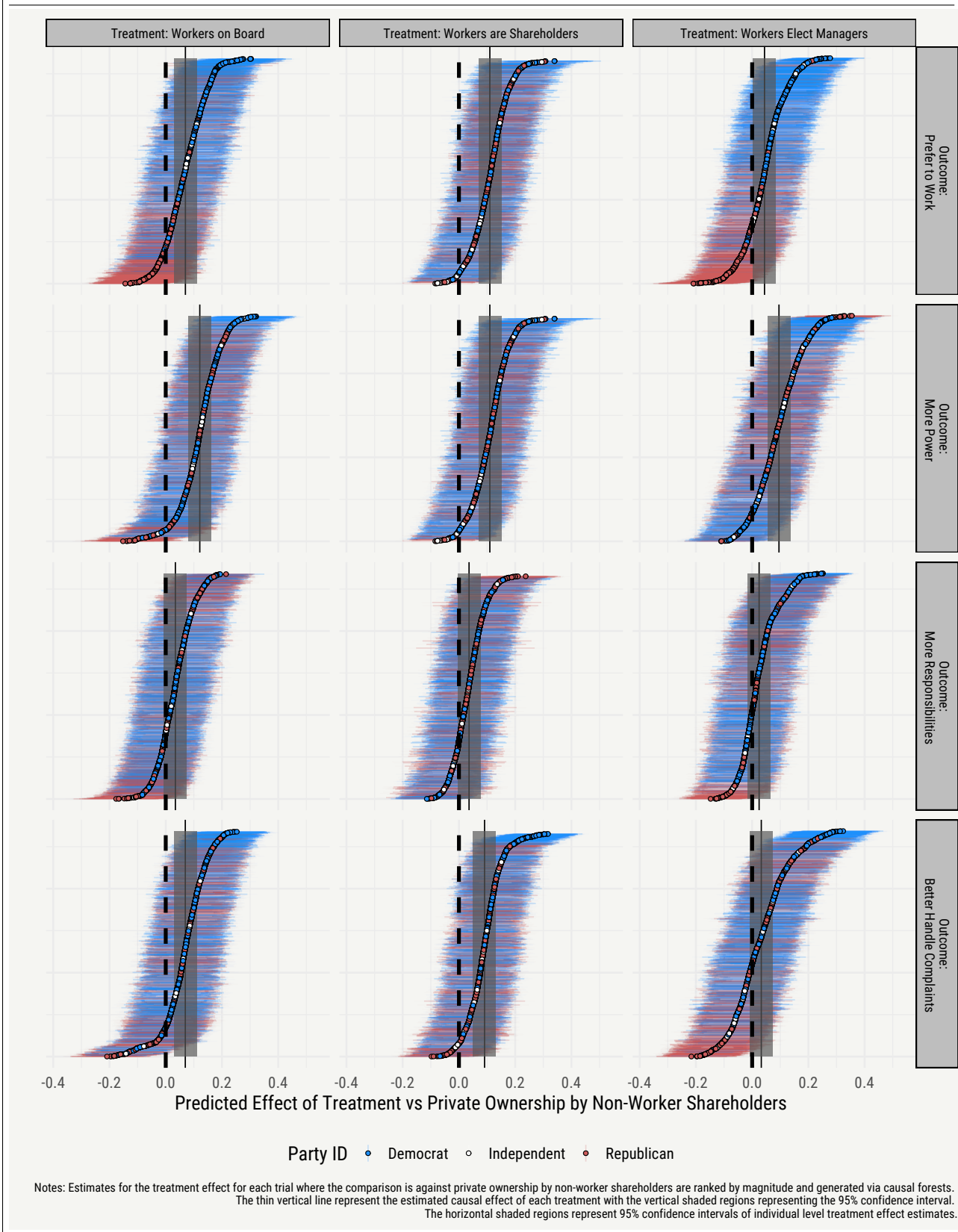


FIGURE S14. Main effects when subsetting to employed respondents and respondents looking for work.



Study 1: Additional Results

FIGURE S15. Effect of different workplace regimes.



STUDY 2

Study 2: Policy knowledge

Table S3 shows that most respondents regardless of policy and condition understood what the workplace democracy policy entailed. There was lower levels of understanding for codetermination, however, the next most selected option was “Codetermination is where workers and managers collectively decide how to run the company.” It seems like respondents may have interpreted that answer to mean that workers sit on the board with management to decide the firm’s operations.

TABLE S3. Average proportion of correct knowledge about workplace democracy policies.

Policy condition	Experimental condition	Average correct answer	Standard error
Codetermination	Control	63.93%	2.1%
Codetermination	Cost	58.35%	2.15%
Codetermination	Benefit	58.66%	2.14%
Codetermination	Cost and benefit	58.54%	2.12%
Manager elections	Control	78.5%	1.81%
Manager elections	Cost	81.39%	1.68%
Manager elections	Benefit	83.36%	1.62%
Manager elections	Cost and benefit	81.16%	1.68%
ESOPs	Control	83.4%	1.63%
ESOPs	Cost	83.8%	1.59%
ESOPs	Benefit	82.34%	1.63%
ESOPs	Cost and benefit	80.86%	1.7%

Study 2: Policy framing effects were not larger for workplace democracy proposals compared to other proposals

We have shown that policy framing changed a host of attitudinal outcomes related to workplace democracy, however, respondents remained supportive of workplace democracy. We investigate whether workplace democracy attitudes are more vulnerable to change than attitudes towards other policies. Given the relatively low salience of workplace democracy proposals, people may have fewer considerations to draw on when reporting their preferences. In turn, policy frames may have disproportionately large effects on support.

Recall, that we asked respondents to evaluate a ban on fracking and paid family leave. To test whether there were policy differences, we interacted our treatment variables with a dummy variable indicating whether the policy was a workplace democracy proposal (1) or another policy (0). Additionally, we standardize our dependent variables by mean centering and then dividing by the standard deviation at the policy level. This allows for direct comparability.

Table S4 shows that there is little variation in policy framing effects on policy support between workplace democracy proposals and other policies. We will focus on the interaction effects because we are interested if the type of policy substantively changes the policy framing effect. The cost frame interaction effect was 0.07 ($p = .11$) and benefit frame interaction effect was 0.07 ($p = .18$). Both estimates are substantively small, suggesting that there is little evidence that policy framing effects are different for workplace democracy compared to other policies.

We also found little difference in policy framing effects on work preferences. Remember that we asked respondents whether they would want to work at a firm with paid family leave. We use the same model as before with work preferences. We again found little difference. The cost frame interaction was -0.09 ($p = .19$) and the benefit frame interaction was -0.06 ($p = .42$). In sum, we found little variation in policy framing effects by

TABLE S4. Comparing policy framing effects between workplace democracy proposals versus other policies - Standardized measure

	Support All policies	Work All policies
Benefit	-0.011 (0.039)	0.107* (0.059)
Cost	-0.183*** (0.039)	-0.022 (0.060)
Both	-0.093** (0.039)	0.017 (0.061)
Workplace policy	-0.047 (0.036)	0.053 (0.049)
Partisan ID (Republican)	-0.606*** (0.076)	-0.416*** (0.086)
Ideology (Conservative)	-0.405*** (0.086)	-0.249** (0.100)
Benefit × Workplace policy	0.069 (0.051)	-0.056 (0.069)
Cost × Workplace policy	0.083 (0.053)	-0.092 (0.070)
Both × Workplace policy	0.039 (0.051)	-0.060 (0.069)
Benefit × Partisan ID	-0.107 (0.086)	-0.057 (0.104)
Cost × Partisan ID	-0.228** (0.092)	-0.124 (0.111)
Both × Partisan ID	-0.219** (0.092)	-0.249** (0.105)
Benefit × Ideology	0.014 (0.098)	0.021 (0.117)
Cost × Ideology	0.241** (0.103)	0.111 (0.128)
Both × Ideology	0.065 (0.104)	0.123 (0.122)
Constant	0.071** (0.028)	-0.026 (0.043)
N	10,690	8,552
R ²	0.127	0.059
Adjusted R ²	0.126	0.057

*p < .1; **p < .05; ***p < .01

Notes: Cluster-robust standard errors at the respondent level are in the parentheses.

We mean-centered and divided by the standard deviation at the policy level for each outcome variable.

The workplace policy variable is 1 when the policy is related to workplace democracy and 0 otherwise.

We fielded the survey on Lucid from January 19th to February 11th, 2022.

policy issue.

Study 2: There is little evidence for heterogeneous policy framing effects across attitudinal moderators

We now investigate whether policy framing effects vary depending on the following moderators: support for democracy, position in the workplace hierarchy, working class identity strength, and social dominance orientation. We build on our original analysis by interacting each moderator with each treatment condition in one model. This model, though less parsimonious, guards against potential potentially statistically significant interactions that are a function of another interaction. We also tested whether working class identity moderated policy framing effects in another model by subsetting to only working class identifiers. We report all of these models in the Appendix in Table S5 and S6 in the interest of space.

We found little evidence suggesting that policy framing effects varied according to our moderators. For policy support, most of our interaction effects are close to zero with moderately sized standard errors. We find a similar pattern with respondents' work preferences. In short, policy framing effects show little variation conditional on moderators.

We found some evidence suggesting that policy framing effects vary depending on working class identity strength. The interaction effects suggest that strong working class identifiers may respond more positively to the benefit frame and are less affected by the cost frame, but the estimates are noisy. Future researchers will have to recruit larger samples to more carefully explore these moderators.

Study 2: Acquiescence bias

Average support for each policy by policy order and experimental condition. We can see there is no consistent increase in support for workplace democracy as policy order increases.

TABLE S5. Investigating heterogeneous policy framing effects - Standardized measure

	Support	Work
	Workplace democracy policies	
Benefit	0.054* (0.031)	0.050 (0.033)
Cost	-0.098*** (0.032)	-0.113*** (0.034)
Both	-0.060* (0.032)	-0.044 (0.033)
Support for democracy	-1.601*** (0.141)	-0.958*** (0.146)
Position in workplace	0.210*** (0.072)	0.363*** (0.076)
Class ID strength	0.136 (0.146)	0.198 (0.150)
Social dominance orientation	-0.758*** (0.158)	-0.607*** (0.166)
Partisan ID (Republican)	-0.391*** (0.081)	-0.301*** (0.088)
Ideology (Conservative)	-0.258*** (0.094)	-0.168* (0.101)
Benefit × Support for democracy	0.078 (0.175)	0.087 (0.188)
Cost × Support for democracy	0.153 (0.187)	-0.069 (0.196)
Both × Support for democracy	0.217 (0.180)	0.230 (0.185)
Benefit × Position in workplace	-0.014 (0.091)	-0.078 (0.097)
Cost × Position in workplace	0.073 (0.093)	-0.011 (0.096)
Both × Position in workplace	0.030 (0.092)	0.017 (0.094)
Benefit × Class ID strength	0.163 (0.183)	0.004 (0.193)
Cost × Class ID strength	0.084 (0.185)	0.219 (0.195)
Both × Class ID strength	0.224 (0.192)	0.231 (0.192)
Benefit × Social dominance orientation	-0.230 (0.203)	0.032 (0.212)
Cost × Social dominance orientation	0.062 (0.202)	0.263 (0.206)
Both × Social dominance orientation	0.165 (0.204)	0.253 (0.209)
Benefit × Partisan ID	-0.109 (0.104)	-0.056 (0.117)
Cost × Partisan ID	-0.220* (0.113)	-0.077 (0.119)
Both × Partisan ID	-0.293*** (0.110)	-0.276** (0.118)
Benefit × Ideology	0.075 (0.117)	0.036 (0.131)
Cost × Ideology	0.150 (0.132)	-0.024 (0.140)
Both × Ideology	0.171 (0.129)	0.126 (0.137)
Constant	0.026 (0.027)	0.026 (0.028)
N	6,414	6,414
R ²	0.190	0.109
Adjusted R ²	0.186	0.105

*p < .1; **p < .05; ***p < .01

Notes: Cluster-robust standard errors at the respondent level are in the parentheses.

We include policy-level fixed effects but do not show them to preserve space.

We mean-centered and divided by the standard deviation at the policy level for each outcome variable.

We fielded the survey on Lucid from January 19th to February 11th, 2022.

TABLE S6. Investigating heterogeneous policy framing effects among the working class - Standardized measure

	Support Workplace dem. policies	Work Workplace dem. policies
Benefit	0.081 (0.052)	0.043 (0.057)
Cost	-0.122** (0.053)	-0.138** (0.055)
Both	-0.090* (0.054)	-0.067 (0.055)
Support for democracy	-1.374*** (0.235)	-0.633** (0.250)
Position in workplace	0.049 (0.127)	0.210 (0.136)
Class ID strength	0.228 (0.232)	0.287 (0.238)
Social dominance orientation	-0.794*** (0.280)	-0.512* (0.293)
Partisan ID (Republican)	-0.153 (0.147)	-0.159 (0.164)
Ideology (Conservative)	-0.326* (0.170)	-0.263 (0.189)
Benefit × Support for democracy	0.160 (0.294)	-0.078 (0.332)
Cost × Support for democracy	0.342 (0.304)	0.232 (0.321)
Both × Support for democracy	0.348 (0.295)	0.079 (0.305)
Benefit × Position in workplace	0.119 (0.150)	-0.118 (0.174)
Cost × Position in workplace	0.129 (0.160)	0.009 (0.169)
Both × Position in workplace	0.116 (0.172)	-0.024 (0.171)
Benefit × Class ID strength	0.472 (0.297)	0.323 (0.321)
Cost × Class ID strength	0.094 (0.320)	0.247 (0.329)
Both × Class ID strength	-0.165 (0.310)	0.044 (0.312)
Benefit × Social dominance orientation	-0.432 (0.337)	-0.514 (0.370)
Cost × Social dominance orientation	-0.161 (0.332)	0.228 (0.342)
Both × Social dominance orientation	0.178 (0.357)	0.284 (0.364)
Benefit × Partisan ID	0.086 (0.179)	0.199 (0.238)
Cost × Partisan ID	-0.396* (0.212)	-0.324 (0.221)
Both × Partisan ID	-0.301 (0.205)	-0.089 (0.231)
Benefit × Ideology	-0.130 (0.218)	-0.124 (0.281)
Cost × Ideology	0.246 (0.244)	0.172 (0.250)
Both × Ideology	0.051 (0.244)	-0.134 (0.271)
Constant	0.016 (0.043)	0.038 (0.045)
N	2,505	2,505
R ²	0.158	0.086
Adjusted R ²	0.148	0.075

*p < .1; **p < .05; ***p < .01

Notes: Cluster-robust standard errors at the respondent level are in the parentheses.

We include policy-level fixed effects but do not show them to preserve space.

We mean-centered and divided by the standard deviation at the policy level for each outcome variable.

We fielded the survey on Lucid from January 19th to February 11th, 2022.

Working class respondents only in these models.

TABLE S7. Checking Study 2 for acquiescence bias.

Policy condition	Policy order	Experimental condition	Average support	Standard error
Codetermination	First	Control	65.79%	2.37%
Codetermination	Second	Control	68.16%	2.1%
Codetermination	Third	Control	69.37%	2.18%
Codetermination	First	Cost	72.38%	1.86%
Codetermination	Second	Cost	61.58%	2.35%
Codetermination	Third	Cost	61.67%	2.44%
Codetermination	First	Benefit	73.38%	2.06%
Codetermination	Second	Benefit	69.8%	2.36%
Codetermination	Third	Benefit	70.43%	2.24%
Codetermination	First	Cost and benefit	72.81%	2.31%
Codetermination	Second	Cost and benefit	65.56%	2.29%
Codetermination	Third	Cost and benefit	59.66%	2.29%
Manager elections	First	Control	50.99%	2.63%
Manager elections	Second	Control	54.24%	2.4%
Manager elections	Third	Control	58.52%	2.59%
Manager elections	First	Cost	45.04%	2.65%
Manager elections	Second	Cost	52.91%	2.48%
Manager elections	Third	Cost	55.28%	2.46%
Manager elections	First	Benefit	55.14%	2.54%
Manager elections	Second	Benefit	59.99%	2.57%
Manager elections	Third	Benefit	58.11%	2.49%
Manager elections	First	Cost and benefit	52.47%	2.45%
Manager elections	Second	Cost and benefit	56.17%	2.6%
Manager elections	Third	Cost and benefit	54.02%	2.44%
ESOPs	First	Control	59.58%	2.31%
ESOPs	Second	Control	64.98%	2.15%
ESOPs	Third	Control	69.11%	2.05%
ESOPs	First	Cost	57.9%	2.21%
ESOPs	Second	Cost	64.37%	1.99%
ESOPs	Third	Cost	62.16%	2.21%
ESOPs	First	Benefit	63.5%	2.2%
ESOPs	Second	Benefit	62.59%	2.13%
ESOPs	Third	Benefit	65.46%	2.29%
ESOPs	First	Cost and benefit	59.36%	2.18%
ESOPs	Second	Cost and benefit	62.98%	2.19%
ESOPs	Third	Cost and benefit	59.22%	2.4%

EXTENDED APPENDIX

Our full Appendix can be found at <https://doi.org/10.7910/DVN/LOBC53> on the Harvard Dataverse.