Heterogeneity in Attitude Responses: Evidence from Bostock v. Clayton County

By CAMERON DEAL[*]

Traditional models of policy formation emphasize how political attitudes and actions affect policy outcomes. However, the converse may occur: policies are often a signal to constituents about the priorities and positions of both political actors and the government more broadly. An emerging literature has shown how these signals can both reflect and affect public opinion, but the central empirical challenge these analyses face is how to distinguish between the two effects and confirm the existence of policies altering attitudes. Given a setting where one can isolate these effects, a question of further interest is the existence of heterogeneity in attitude effects across groups. Whether effects are broad-based or vary across groups offers evidence useful to parsing the mechanisms that drive attitude change and offer political actors insight into the potential effects of their actions.

On June 15th, 2020, the U.S. Supreme Court decided that existing federal law prohibits employment discrimination based on sexual orientation or transgender status in Bostock v. Clayton County. While 22 states had already established employment protections based on sexual orientation and gender identity, 28 states gained these protections from the ruling. Overnight, they became "bound by *Bostock*". This decision came as a surprise to many given the conservative composition of the court. The architects of the court viewed it as an institution insulated from public opinion, which distinguishes the policies it imposes from those created by legislative processes

(Casillas, Enns and Wohlfarth, 2010). The Bostock decision offers an ideal setting for isolating the effect of employment protections on LGBT attitudes given that it was unanticipated, surprising, and a judicial decision rather than a product of the legislative process.

I find broad-based improvements in attitudes towards LGBT people, finding that coefficients are generally the same sign and similar magnitudes. For several groups– including Republicans and those "interested in government," I find suggestive evidence of stronger effects. However, I find that my effect is almost entirely driven by men. These findings suggest a lack of support for a *backlash* explanation for attitude change and support a *legitimization* model. However, the distinct effects by gender suggest that gendered attitudes towards LGBT people may differ significantly.

The rest of the paper proceeds as following: Section I reviews relevant literature. Section II describes the data and variables utilized in the analysis. Section III describes the empirical analysis and results. Section IV concludes.

I. Related Literature

I build on earlier work that examined the impact of policy changes on attitudes towards LGBT people in the context of same-sex marriage (Abou-Chadi and Finnigan, 2018; Aksoy et al., 2020; Flores and Barclay, 2015). Additionally, a related analysis finds positive impacts of Bostock on public opinion towards LGBT people, but does not leverage state variation in the existence of employment protections prior to the decision (Thompson, 2022). I build most directly on Deal (2022), which uses *Bostock v. Clayton County* and found modest improvements in attitudes towards LGBT people.

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This analysis extends Deal (2022) by providing further evidence and discussion of the state-level shift in attitudes and heterogeneity in the public opinion effects previously identified.

There are several reasons why might we expect heterogeneous effects of political signals on attitudes towards LGBT people. Generally, applied microeconomics work that examines treatment effects tends to conduct subgroup analyses to probe heterogeneity and the external validity of the estimates produced (Heckman and Vytlacil, 2001; Angrist, 2004). In the specific case of attitudes, factors such as prior attitudes towards the topic at hand (which can be highly correlated with demographic and socioeconomic characteristics) can affect the reception of political signals and their subsequent effects on attitudes (Skipworth, Garner and Dettrey, 2010). Additionally, characteristics of the sender (such as their ideological similarity to the recipient and their perceived credibility) can also play a role in determining attitudinal effects (Gabel and Scheve, 2007). Theoretical models have also shown how varying levels of wealth and political ideology predict different voting outcomes and effects of political signals coming from news consumption (Enke, Polborn and Wu, 2022; Chan and Suen, 2008). Further literature has shown that mass media (a common form of political signal) can change beliefs and these effects depend on individual characteristics and experiences (Gilliam, Valentino and Beckmann, 2002; Hajnal, 2001). These empirical findings and theoretical predictions motivate my examination of heterogeneity in the attitude effects of a policy change.

II. Data

This analysis uses the repeated cross-sectional Democracy Fund + UCLA Nationscape survey, which collected data on political and social attitudes from approximately 6,000 Americans each week from 2019-2021 (Tausanovitch and Vavreck, 2021). Though little used by economists, political scientists have begun to use the large weekly sample sizes and rich opinion data to measure public opinion (Chan, Kim and Leung, 2021; Reny and Newman, 2021). Using sample weights, the demographic estimates obtained by Nationscape are close to government benchmarks, and I use weights throughout my analysis (Tausanovitch and Vavreck, 2021).

The outcome of interest is attitudes towards LGBT people. Respondents could say they were (1) "very favorable", (2) "somewhat favorable", (3) "somewhat unfavorable", or (4) "very unfavorable". I dichotomize this outcome to compare categories 1–3 with 4, measuring the percentage of respondents who said they were "very unfavorable" to LGBT people. This outcome is capturing respondents who felt strongly and negatively towards LGBT people, rather than neutral. I exclude those respondents who did not answer the question or who said they "had not heard enough" to form an opinion ($N = 66,082$). After restricting my sample to complete cases, my main specifications have a sample size of $N = 398,648$. I classify those states that already had employment protections (n=22) as untreated in this analysis, and those states who were "bound by Bostock" and gained employment protections for LGBT people overnight as treated (n=28). The treatment begins on June 15th, the day that the Bostock ruling was issued.

III. Empirical Analysis

A. Identifying Variation

I use kernel density graphs to characterize the variation in my outcome variable across states over time. Figure 1 displays the distribution of states corresponding to their % that reported being Very Unfavorable to LGBT people. The solid black line depicts those states that already had employment nondiscrimination protections pre-*Bostock*, while the dashed line displays the states that were "Bound by *Bostock*." Unsurprisingly, states that had pre-existing employment non-discrimination tended to have lower very unfavorable rates than those that were affected by the decision.

In Figures 2a and 2b, I show the change in these distributions after the *Bostock* deci-

sion. It is clear that the distribution of public opinion shifts left for the treated states after the decision, while the distribution for untreated states does not change. This suggests that states "bound by *Bostock*" experienced a reduction in unfavorable attitudes towards LGBT people relative to untreated states, a descriptive pattern I will formalize using a difference-in-difference model.

B. Difference-in-Differences

Following Deal (2022), I estimate the following equation (1):

$$Y_{ist} = \alpha + \gamma S_s + \lambda W_t + \delta(T_s \cdot D_t) + \beta X_i + \epsilon_{ist}$$

Where Y_{ist} is the very unfavorable indicator variable, S_s is state fixed effects, W_t is week fixed effects, T_s is treatment status, D_t is an indicator for post-*Bostock*, X_i is a vector of individual characteristics, and ϵ_{ist} is an idiosyncratic error term (clustered at the state level). I estimate the coefficient of interest, δ, by interacting the indicators for treatment and post-*Bostock*; it represents the effect of the new employment protections on attitudes towards LGBT people.

C. Heterogeneous Effects

I estimate equation (1) separately for several different groups to examine heterogeneity in public opinion effects, and Figure 3 displays the coefficients across these different subgroups. I find no evidence of different public opinion effects across race, educational attainment, and when comparing residents of states that had below-median support for LGBT people in the pre-period with residents of states with above-median support. For several comparisons, the coefficients were substantively but not statistically distinct—Republicans and respondents who were "interested in government" (denoted Interested in the figure) may have experienced stronger effects. The comparison across gender effects is statistically significant—I estimate no effects for women, but strong effects for men. This gendered response to the *Bostock* decision is distinctive—across other demographic groups, there is a broad based im-

provement in attitudes towards LGBT people.

IV. Conclusion

I record the effects of a policy change that was plausibly exogenous to political attitudes and exploit cross-state variation in pre-existing LGBT employment protections to estimate the causal effect of this policy change on attitudes towards LGBT people. Additionally, I characterize cross-state variation in attitudes towards LGBT people, examining the effect of the treatment on this variation. Finally, I probe heterogeneity in this effect and find broad-based improvements in attitudes towards LGBT people. However, I also find that the effect is gendered—driven entirely by men, and I find no evidence of a significant change in women's attitudes.

Thompson (2022) uses *Bostock* and a Before-After design to estimate changes in public opinion, and finds that improvements in attitudes were concentrated among Democrats and the religiously unaffiliated, suggesting that effects were strongest in groups that may have already had favorable views. This stands in market contrast to the gendered effect I report—men consistently report worse attitudes towards LGBT people (Woodford et al., 2012). It is possible that Thompson's identification strategy (focused on the weeks surrounding the decision) is predominantly picking up the effects of news coverage, which may have been concentrated among left-leaning outlets. In contrast, I leverage cross-state variation in policies, which may reflect the effects the policy change more broadly (and in particular the signal of a conservative male Supreme Court Justice authoring a pro-LGBT opinion). Future work should further examine how the sender of a political signal can affect its reception among specific groups and what these heterogeneous effects can tell us more broadly about the determinants of attitudes.

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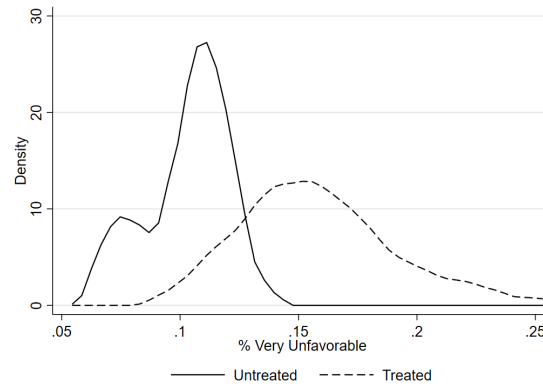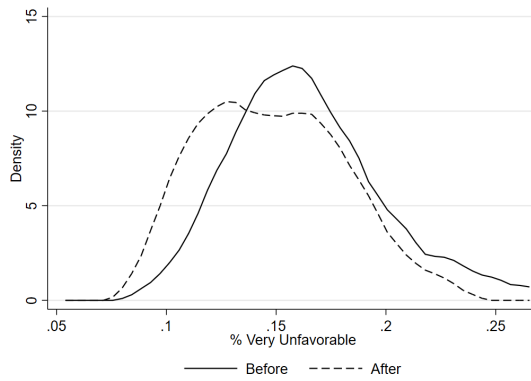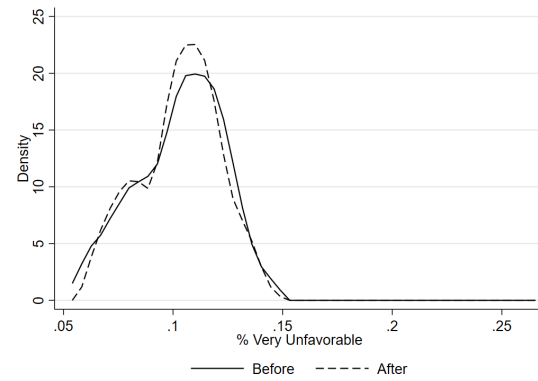
Figure 1. : Distribution of % Very Unfavorable Towards LGBT People.

(a) Treated States

(b) Untreated States

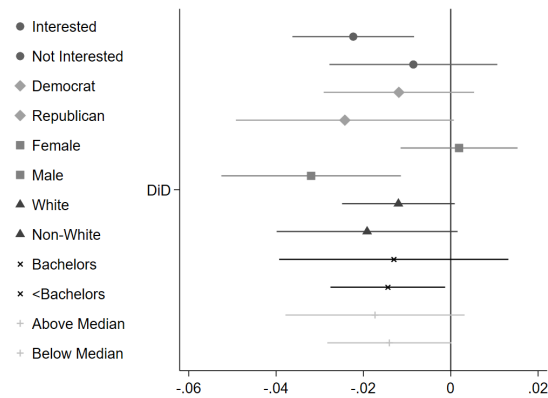Figure 2. : Shift in % Very Unfavorable Towards LGBT People.

Figure 3. : Heterogeneity in Public Opinion Effects of *Bostock v. Clayton County.*