

Gender Identity Nondiscrimination Laws in Public Accommodations: a Review of Evidence Regarding Safety and Privacy in Public Restrooms, Locker Rooms, and Changing Rooms

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Article

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Abstract

Legislation, regulations, litigation, and ballot propositions affecting public restroom access for transgender people increased drastically in the last three years. Opponents of gender identity inclusive public accommodations nondiscrimination laws often cite fear of safety and privacy violations in public restrooms if such laws are passed, while proponents argue that such laws are needed to protect transgender people and concerns regarding safety and privacy violations are unfounded. No empirical evidence has been gathered to test such laws' effects. This study presents findings from matched pairs analyses of localities in Massachusetts with and without gender identity inclusive public accommodation nondiscrimination ordinances. Data come from public record requests of criminal incident reports related to assault, sex crimes, and voyeurism in public restrooms, locker rooms, and dressing rooms to measure safety and privacy violations in these spaces. This study finds that the passage of such laws is not related to the number or frequency of criminal incidents in these spaces. Additionally, the study finds that reports of privacy and safety violations in public restrooms, locker rooms, and changing

rooms are exceedingly rare. This study provides evidence that fears of increased safety and privacy violations as a result of nondiscrimination laws are not empirically grounded.

Keywords

Transgender Gender identity Discrimination Safety Restroom
Public accommodations Law

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Notes

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Compliance with Ethical Standards

Ethical Approval

This article does not contain any studies with human participants or animals performed by any of the authors. An IRB exemption was obtained by the authors for use of de-identified criminal record data (IRB#15-001060).

Conflict of Interest

Amira Hasenbush declares that she has no conflict of interest. Andrew Flores declares that he has no conflict of interest. Jody Herman declares that she has no conflict of interest.

Appendix: Placebo Matched Pairs Analysis

The analysis was re-conducted using a second matching procedure. Localities with clear GIPANDOs were matched to localities that clearly did not have a GIPANDO, and localities with limited GIPANDOs (i.e., Brookline and Cambridge) were also matched to localities that clearly did not have a GIPANDO (see Table 2). The limited GIPANDOs offer a type of placebo comparison, where a policy was introduced but not clearly inclusive of the protections that are afforded in localities with clear GIPANDOs.

Table 4 provides a contingency table showing the average annual number of incidents, similar to the analysis in the report. For this analysis, there were three levels of treatment: a group of localities with clear GIPANDOs, a limited GIPANDO group that introduced a gender identity policy, but made exceptions or lacked clarity on restrooms, and the matched localities group without GIPANDOs. There were fewer overall incidents in the group with clear GIPANDOs when compared to the matched localities, but there were no apparent patterns of an increase in victimization in the timeframe after passage. These differences were also not significantly different from one another. A Fisher's exact test indicated that there was no significant relationship between GIPANDOs and restroom crimes. An estimate of the before-and-after changes between the localities with clear GIPANDOs and their matched pairs of the average proportion of monthly incidents in locations also showed no statistically significant difference. There does not appear to be a relationship between policy introduction and restroom incidents. Again, here, even if there were many more localities, a statistical power analysis found that it is unlikely that there would be a statistically significant difference between GIPANDO localities and matched localities. If there was a sample with 50 matched pairs with observed effect size at 90% power, then a one-tailed alpha would be 0.85, suggesting that the null hypothesis of no difference would also fail to be rejected with a greater number of matched pairs.

Table 4

Average number of incidents per year as documented by police departments by localities with clear GIPANDOs, limited GIPANDOs and matched localities before-and-after policy passage

| | Localities with clear GIPANDOs | Localities with limited GIPANDOs | Matched localities without GIPANDOs | Difference per 100,000 (clear- matched) |
|--|---|---|---|---|
| Before passage | 1.0 (0.26 per 100,000) [- 0.91 per 100,000, 1.44 per 100,000] | 1.5 (2.55 per 100,000) [- 0.07 per 100,000, 5.18 per 100,000] | 2.5 (1.07 per 100,000) [- 0.00 per 100,000, 2.15 per 100,000] | - 0.81 per 100,000 [- 2.40 per 100,000, 0.78 per 100,000] |
| After passage | 1.5 (0.63 per 100,000) [- 0.54 per 100,000, 1.81 per 100,000] | 0.5 (0.85 per 100,000) [- 1.78 per 100,000, 3.48 per 100,000] | 3 (1.32 per 100,000) [0.24 per 100,000, 2.39 per 100,000] | - 0.68 per 100,000 [- 2.27 per 100,000, 0.91 per 100,000] |
| Change per 100,000 (after- before) | 0.37 per 100,000 [- 1.29 per 100,000, 2.03 per 100,000] | - 1.70 per 100,00 [- 5.42 per 100,000, 2.01 per 100,000] | 0.24 per 100,000 [- 1.27 per 100,000, 1.76 per 100,000] | 0.13 per 100,000 [- 2.12 per 100,000, 2.38 per 100,000] |
| Total annual average | 1.25 (0.45 per 100,000) [0.05 per 100,000, 0.85 per 100,000] | 1.0 (1.70 per 100,000) [0.24 per 100,000, 3.16 per 100,000] | 2.75 (1.19 per 100,000) [0.29 per 100,000, 2.10 per 100,000] | |

Notes: Average annual crime rate in incidents per 100,000 people are in the parentheses; 90% confidence intervals are in the brackets; $\chi^2_2 = 1.42$; $p = 0.49$; Fisher's exact = 0.658 . Difference-in-difference = 0.41, bootstrapped S.E. = 1.05, $p = 0.699$

Similar to before, we assessed trends in crime rates between these localities. This way, it could be assessed whether trends in crime rates increased in clear GIPANDO localities and limited GIPANDO localities, as compared to their matched localities. The figure limits the timeframe to 12 months before and 12 months after the passage of the local GIPANDOs. A 12-month window was chosen because some localities in this analysis were asked to provide incidents within a two-year timeframe, so we restrict the plot to the timeframe common to all localities.

In Fig. 3, the model included differences between localities with clear enforceable GIPANDOs that applied to restrooms and their matched localities (black line), and differences between the limited GIPANDOs with unclear enforceability or restroom exceptions and their matched localities (gray line). The local regressions showed a lot of overlap between and across these three groups. As opposed to the analysis in the body of the report, which showed slightly lower crime rates in the GIPANDO localities as compared to their matched pairs after policy introduction, there was no statistically significant difference in the average monthly proportion of criminal incidents in restrooms both over time and across contexts.

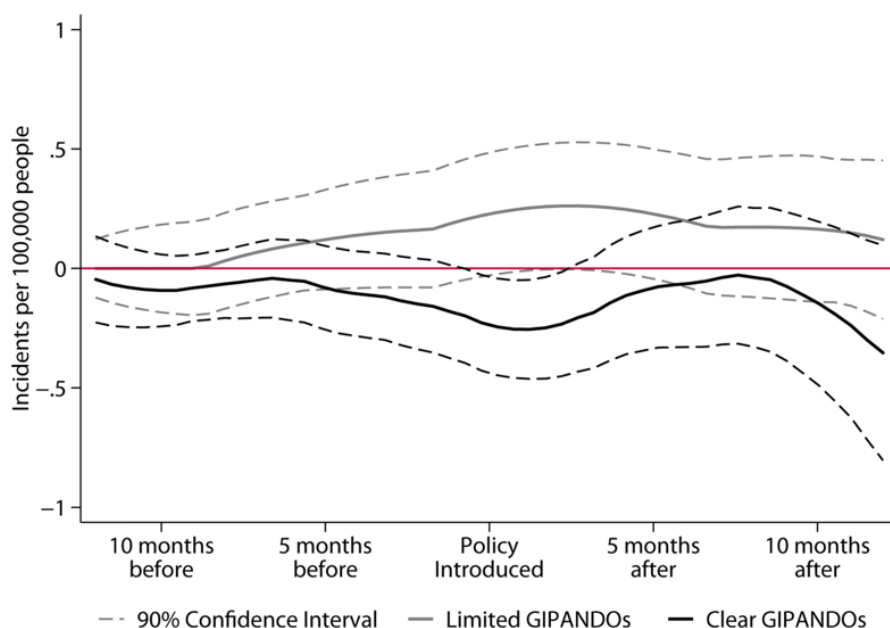


Fig. 3

Differences in the average monthly rate of criminal incidents in public restrooms, locker rooms and changing rooms among localities with clear GIPANDOs and limited GIPANDOs compared to matched localities without GIPANDOs. Notes: 90% confidence intervals represented by dashed lines; negative values show lower rates of victimizations in GIPANDO localities compared to matched localities before, during, and after policy introduction.

These results indicate that changes in the average rate of criminal incidents are not related to the passage of GIPANDOs. The limited GIPANDOs provide another source of comparison, and these additional comparisons indicate that clear GIPANDOs are not uniquely related to increases in average rates of criminal incidents.

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