



Employment discrimination and labor market protections for sexual minorities in Brazil[☆]

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ABSTRACT

This study estimates the impacts of an employment non-discrimination act (ENDA) for sexual minorities in Brazil. We contribute new evidence on ENDA impacts for sexual minorities in a distinct context from the current literature. Additionally, this is one of few studies to document labor market gaps by sexual orientation in Latin America. In this paper, we first present evidence of labor market gaps in employment, formal sector employment, hours worked, income and hourly earnings for lesbian and gay individuals in Brazil. For the same groups, we then estimate the impact of Brazilian legislation forbidding employment discrimination based on sexual orientation on these same labor market outcomes. Regarding initial pre-policy labor market gaps, conditioning on individual and work-related controls, we see that lesbian women experience wage premiums compared to straight women, while gay men experience wage penalties compared to straight men. We then estimate robust evidence that the implementation of employment protections increased income for gay men. For lesbian women, we estimate evidence of post-policy reductions in employment, but higher income and hourly earnings for those still working. Focusing on a Latin American country, we find many impacts consistent with the more recent existing evidence on ENDAs for sexual minorities.

1. Introduction

Over the years, there has been a general shift toward lesbian, gay, bisexual, transgender, and queer (LGBTQ⁺) equality across the globe by means of government legislation (Waldijk, 2009). However, up until recently, there has not been much empirical analysis to support the policy decisions being made by legislators. Evidence on labor market gaps by sexual orientation for diverse geographies is particularly scarce,¹ and what little evidence exists on employment non-discrimination acts (ENDA) for sexual minorities comes exclusively from a single-country context (the United States). This study is the first to evaluate the impacts of an ENDA for sexual minorities in Brazil. This

work therefore contributes new evidence on these policy impacts in a distinct country context from the current literature. Additionally, it is one of few studies to document labor market gaps by sexual orientation in Latin America. In this paper, we therefore first present evidence of labor market gaps in employment, income, hours worked, hourly earnings and formal employment for lesbian and gay individuals in Brazil. For the same groups, we then estimate the impact of Brazilian legislation forbidding employment discrimination based on sexual orientation on these same labor market outcomes.

While evidence on ENDAs by sexual orientation to date only exists for the United States, the shift toward government legislation to prohibit discrimination based on sexual orientation is not a U.S. phenomenon.

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¹ Most current evidence studies the United States. Some exceptions include estimation of discrimination and labor market gaps for gay and lesbian individuals in Sweden (Ahmed et al., 2013), the Netherlands (Plug and Berkhout, 2004), Greece (Drydakis, 2009), the UK (Bridges and Mann, 2019) and in Chile and Uruguay (Brown et al., 2019).

Specifically, in Latin America, Argentina became the first country to legalize same-sex marriage in 2010, followed by Brazil and Uruguay in 2013, Colombia in 2016, and Ecuador in 2019 (Kennon, 2020). As of 2011, Brazil, Ecuador, and Peru had already implemented employment non-discrimination acts (ENDAs) (Encarnación, 2011). We focus on Brazil both to fill a notable gap in the academic literature and in part because the data availability paired with policy variation makes it a good context for estimating a causal effect of ENDA legislation. These data and policy considerations are important in this line of research as the current scarcity of evidence is at least in part due to a lack of large datasets that differentiate individuals based on sexual orientation. Some studies have overcome this obstacle by using the assumption that same-sex cohabitating adults can be classified within their samples as same-sex couples (e.g., Gates 2009, Antecol et al. 2008, Baumle and Poston 2011) and in fewer studies researchers have been able to directly identify sexual orientation (e.g. Carpenter and Gates 2008). In line with this first set of studies, we make use of cohabitating couples in our analysis.

Notably, Brazil has generally had a wider average acceptance of the LGBTQ⁺ community than many countries, including neighboring Latin American countries (e.g., see Appendix Fig. 1). However, it is also a country of contradictions in terms of LGBTQ⁺ acceptance. While Brazil hosts the largest Pride event in the world, it is also a traditionally Catholic society with a growing evangelical population and has recently seen more conservative leadership with openly anti-LGBTQ⁺ sentiment. Concerningly, Brazil has also been recently marked by increases in violence toward the LGBTQ⁺ community.²

Using detailed household data, we estimate initial pre-policy labor market gaps conditioning on individual and work-related controls. We observe that lesbian women experience wage premiums compared to straight women, while gay men experience wage penalties compared to straight men. Current evidence, mostly from the United States and Europe, generally finds labor market premiums for lesbian women and labor market penalties for gay men. We provide some of the first evidence that this general pattern still holds in the notably different context of a Latin American country.³

We make use of variation across states in Brazil over time in enactment of ENDAs, making employment discrimination based on one's sexual orientation illegal. We compare similar individuals in same-sex and different-sex couples to identify the impacts of the policy on various labor market outcomes for gay and lesbian individuals. A priori, it is not clear what effect ENDAs will have on a discriminated-against group. These formal protections should, in theory, result in improved labor market outcomes for a discriminated-against group, as the cost of such discrimination increases when it is legally banned. However, the opposite can occur if employers view such groups as more costly to hire due to the potential for legal action. Causal empirical evidence on the effects of ENDAs is therefore important for policymakers who want to enact evidence-based policy to understand how pro-LGBTQ⁺ legislation impacts individuals.⁴

² Regarding gay pride events, see for example: <https://www1.folha.uol.com.br/folha/colunas/destaques/ult10009u406368.shtml> (accessed: July 8, 2022). For violence and changing rhetoric against the LGBTQ⁺ community, see for example: <https://www.theguardian.com/world/2018/jan/22/brazil-lgbt-violence-deaths-all-time-high-new-research>; <https://www.reuters.com/article/us-latam-lgbt-killings-idUSKCN1UY2GM>; <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC6836409/> (accessed: July 8, 2022).

³ Brown et al. (2019) document that gay men are more likely to work and lesbian women are less likely to work conditional on controls for Chile and Uruguay.

⁴ Effects may also extend beyond the individual. Consistent with reduced efficiency caused by discrimination in the labor market, Badgett et al. (2019) studies the relationship between the Global Index on Legal Recognition of Homosexual Orientation and GDP and finds a positive correlation between LGBTQ⁺ inclusion and macroeconomic development.

This study finds robust evidence that the implementation of employment protections increased income for gay men. As we document pre-policy wage penalties for gay men in Brazil, this finding of increased income can be considered a successful outcome of employment protections. Nonetheless, the policy does not change the higher levels of informality in employment experienced by gay men relative to their straight counterparts. In contrast, for lesbian women we document pre-policy wage premiums that grow under the ENDA policy, but our estimation also shows evidence of post-policy lower engagement of lesbian women in the labor market through reduced employment. Our findings contribute to the still relatively small literature that documents labor market gaps experienced by sexual minorities and to the even smaller emerging literature on the role that ENDAs might have in closing such gaps. In addition to extending research on sexual minorities, this study speaks to the larger literature on the effectiveness of ENDAs for correcting labor market discrimination more generally.⁵

The remainder of this paper is structured as follows: in Section 2, we provide a review of the most relevant literature; in Section 3 we describe the data source for this paper; in Section 4, we present baseline labor market gaps by sexual orientation; in Section 5 we discuss our empirical strategy; followed by the results and a discussion of their interpretations in Sections 6 and 7; finally, Section 8 provides concluding remarks and suggestions for future research.

2. Literature

While there is an abundance of literature studying wage disparities between various advantaged and disadvantaged groups in society, less has been studied for the LGBTQ⁺ community. To our knowledge, the earliest study to document wage gaps by sexual orientation is Badgett (1995) using data from 1989 to 1991. Later studies have continued to find wage gaps for sexual minorities.⁶ Particularly relevant to our work are the few studies that directly aim to estimate the impacts of policies banning labor market discrimination based on sexual orientation.

Klawitter and Flatt (1998) and Klawitter (2011) use cross-sectional census data to document earnings of individuals within same-sex relationships living in areas with policies prohibiting discrimination based on sexual orientation and in areas without these protections, concluding that the policies either had no effect or only for gay males and not lesbian women. Martell (2013) applies a difference-in-differences method to study ENDA policy effects over time, finding higher returns to experience post-policy for behaviorally gay men. Tilcsik (2011) uses a resume study design to find that openly gay men still faced barriers to employment in regions with laws prohibiting discrimination based on sexual orientation. In more recent work, Burn (2018) explores the specific provisions of ENDA laws, finding different impacts of ENDAs depending on the specific provisions of the laws.⁷ Not all studies have considered lesbian women and have only focused on gay men, but those that do study both often find differences in wage gaps and in the impact of ENDA laws based on gender of the individual (Klawitter, 2011; Baumle and Poston, 2011; Burn, 2018; Delhomme and Vamossy, 2024). A meta-analysis done by Klawitter (2015), notes that most studies that distinguish by gender of the individual in documenting discrimination

⁵ Results vary by the group being discriminated against. For example, evidence shows that labor market protections have resulted in improvements in labor market outcomes for black men, but not generally for women (e.g. Neumark and Stock 2006) or for older workers and handicapped individuals (Lahey, 2008; Baldwin and Johnson, 1994).

⁶ See for example: Black et al. (2003), Baumle and Poston (2011), Carpenter (2007) and Carpenter and Gates (2008).

⁷ Burn (2018) finds that compensatory damages provisions in the law results in increased wages, as well as longer statutes of limitations resulting in higher employment. This motivates our use of various labor market outcomes in this study.

(baseline discrimination, not policy impacts) find that gay men experience a wage penalty and that lesbian women experience a wage premium in the labor market.

Both gays and lesbians have higher education than straight men and women. Burn and Martell (2020) show that educational choices of gay and lesbian individuals are consistent with efforts to offset future discrimination. For example, they show that gay men choose college majors that entail lower levels of prejudice. Plug et al. (2014) find that this happens in the labor market as well, where gays and lesbians shy away from prejudiced occupations. In addition to educational and occupational differences, Klawitter (2015) notes that work experience could explain the lesbian advantage measured in empirical studies.⁸ Lesbian versus straight women may experience differences in human capital due to different expectations regarding work in the future. This is supported by Daneshvary et al. (2009) who find that lesbians who had been previously married had lower wage premiums than those who had never been married. Klawitter (2015) also notes that “*The level of expected earnings from a same-sex partner could ... explain the lesbian premium as well as the gay male penalty. Lesbians expecting their own earnings to not be balanced by higher earnings from a male partner might work more, work in more intensive jobs and might invest in more human capital*” (pp.24). These arguments highlight the importance of intra-household decisions.

An alternative explanation for different wage penalties/premiums by the gender of the lesbian or gay individual is based on differential discrimination on the part of those making hiring and promotion decisions. Note that if men are predominantly in the position of making the hiring decisions and they have more negative attitudes toward gay men than lesbian women, this could explain gender differences in estimates. The idea that employer prejudice could be particularly important is supported by evidence from Burn (2020) who shows that wage penalties for gay men are driven more by prejudice of managers than of customers or co-workers.

Notably, Delhomme and Vamossy (2024) finds evidence in support of both mechanisms based on household specialization and on societal attitudes. The study uses variation in implementation of anti-discrimination laws over time to show that such policies narrowed gaps between gay and straight men and between lesbian and straight women. The narrowing of the gap between lesbian and straight women is consistent with observed increases in having children among lesbians, resulting in household specialization patterns that more closely resemble those experienced by straight women. He also provides evidence that passage of ENDAs impacts support for same-sex marriage, highlighting how policy changes can change societal sentiment. To the extent that this includes labor market attitudes, especially of those in charge of hiring and promotions, this could prove an important mechanism through which such laws affect gaps between gay and straight men.

3. Data

Our main data comes from the National Household Sample Survey – PNAD database from The Brazilian Institute of Geography and Statistics.⁹ This is the only annually collected and nationally representative micro data survey with satisfactory employment information collected for Brazil. The data used is a repeated cross-section running from 1996 through 2011 and includes household data, which is then further disaggregated at the individual level. Because sexual orientation is not an identifier within this data, as done in much of the existing literature, we make use of cohabitating pairs to define individuals in a same-sex versus

different-sex couple for our estimation. As a natural concern in using cohabitating pairs to proxy for gay and lesbian couples is that we might be capturing roommates instead, we provide evidence throughout the paper that our results are not likely to be driven by roommates mistakenly recorded as gay and lesbian couples. We make sample selection choices, provide descriptive evidence and tests for robustness of our results with the aim of showing that our results do reflect accurate labor market gaps as well as ENDA policy impacts for partnered and cohabitating gay and lesbian individuals.

To construct our variables for gay and lesbian versus straight men and women in cohabitating couples, we start with individuals self-identifying as a couple (the reference person and spouse to reference person). These are easily included in the sub-sample of couples. However, because same-sex marriage has not always been legal within Brazil (prior to 2013), many years within the data have zero same-sex couples when using this simplistic approach. Accordingly, we expand the parameters for a potential couple to include individuals with status of “non-relative” living within the same household as the reference person when no spouse was identified within the household. In this way, it is possible to capture cohabitating couples, regardless of their legal marital status. It is important to note that our constructed sample is limited to households with at most one “non-relative.” There is no way to determine relationship statuses between multiple non-relatives within the same household. For instance, there may exist the reference person, and two non-relatives. In this case, the non-relatives may be in a relationship with each other, or one may be in a relationship with the reference person. As each are equally possible, we opt to exclude these cases from our sample.¹⁰ This, of course, means that we are excluding some alternative household structures.

While the legal working age in Brazil is 18 (Institui o Código Civil, 2002), we choose to restrict our sample to those aged 30 or older. We do this, first, to specifically avoid common ages where living with roommates (non-family and non-couple) is likely highest, and second, for our labor market analysis, to observe individuals at an age where higher education is likely fully completed and individuals are working under “normal” conditions (rather than possible student jobs).¹¹ Likewise, we also limit the age difference between individuals in a presumed couple to a maximum of 10-years to avoid potentially miss-labeling other forms of non-relative relationships that are less likely to reflect couple status. Given limited evidence on the distribution of actual age differences between gay and lesbian couples in Brazil, this age range is largely chosen based on evidence from research on ranges of desired age differences in partnership.¹²

¹⁰ We also do not consider domestic employees, and relatives of the domestic employees (as they are categorized separately) as potential partners.

¹¹ In Brazil, only 33 % of those enrolled in a bachelor’s program had finished by the expected time (4-5 years), which would correspond with graduating at age 22 or 23, rising to 50 % by 3 years later (25/26 years old). Brazil has a longer than average time to degree and below average completion rate (OECD, 2019). Roughly 8-9 % of adults aged 25-29 are still both in school and working (data from 2004 to 2014, PNAD) (Sposito et al., 2018).

¹² Research using online and newspaper dating advertisements quantify preferences regarding desired age gaps. This research finds a preference centered at dating people of similar ages (zero age gap) (Conway et al., 2015; Harry, 1982). Evidence for gay and lesbian individuals regarding their upper and lower limits of requested age in a potential partner typically fall within a range of 8-12 years gap (e.g. for the UK, Conway et al., 2015; for the US, Kenrick et al., 1995; Burrows, 2013). Studying only heterosexual preferences in Brazil, de Sousa Campos, Otta and de Oliveira Siquiera (2002) find similar results to the literature on gay and lesbian couples. Kenrick et al. (1995) finds desired age ranges are quite similar for gay and straight men and for lesbian and straight women and Burrows (2013) finds actual age differences in couple for heterosexual males was dramatically smaller on average than stated ranges, making the desired 8-12 year gap from the literature a clear upper bound on actual partnership ages.

⁸ The author also noted that many studies can only use “potential experience” and that this may differ by lesbian and straight women due to childbearing and childcare behavior.

⁹ The acronym PNAD stands for Pesquisa Nacional por Amostragem de Domicílios in Portuguese.

Within these various sample restrictions, we use language that refers to an adult cohabitating with another adult of the same recorded sex as gay or lesbian. Likewise, we refer to two different-sex cohabitating individuals as straight. Being considered gay or lesbian in our study therefore is defined by the recorded sex and presumed sexual orientation of the individual given their cohabitating relationship status. However, it should be noted that even if we perfectly capture couple status, this assumption is an over-simplification, as each of these groups could include bisexual individuals as well, and we have no way to identify this with our data (e.g. a bisexual man in a relationship with another man will be recorded as gay and a bisexual man in a relationship with a woman will be recorded as straight).¹³ In sum, our main sample consists of prime working age individuals, ages 30–64 years old, in presumed same-sex and different-sex cohabiting relationships in the states that experienced a policy change within our years of data, 1996–2011.

Along with our main household data, we use data on policy timing taken from [Waalwijk \(2009\)](#). [Waalwijk's \(2009\)](#) paper breaks out the years in which different states in Brazil enacted policy explicitly forbidding sexual orientation discrimination in employment. Up until the late 1980s, no state in Brazil had any legislation explicitly forbidding employment discrimination related to one's sexual orientation. From 1989 through 2006, various states across Brazil began to put such policies into place (see [Fig. 1](#)). We make use of these policy changes and data for multiple years before and after the policies take place in various states. From [Fig. 1](#), we can see that the earliest year in which legislation was passed is 1989, followed by 1993 (these four states have no data from before the policy went into effect and are excluded from the sample). The majority of states that passed ENDAs for sexual orientation did so between 2000 and 2006.

In our sample for policy analysis, we exclude states that are never observed implementing a non-discrimination policy by sexual orientation or already have such a policy in place at the start of our sample. For analysis of the policy impacts of ENDAs in Brazil, we will use a difference-in-differences estimation methodology specific to analysis with staggered implementation (see [Section 5](#)). This methodology requires the use of only those states where an observed policy change falls within our sample years. For interpretation of our results, it is important to consider that there are likely large unobserved differences between states that never implement such a policy, or adopt one very early, compared to those adopting such legislation when most policy changes were being made. We therefore present evidence for such “typical” policy-changing states in Brazil.

3.1. Sample characteristics

Individuals within same-sex relationships have many differences in observable characteristics that may contribute to employment disparities. [Table 1](#) provides summary statistics for individual demographic and labor market variables in our sample. As motivated by previous studies that have distinguished by the sex of the individual, we also summarize and estimate results separately by males and females. Our final sample summarized in [Table 1](#) is restricted to individuals aged 30–64, who have non-missing demographic information on variables such as gender, age, education and presence of children and have non-missing information on their employment status. The first column of [Table 1](#) presents summary statistics for the sample overall. The next four columns then break sexual orientation out by the sex of the individual. The final sample consists of 609,888 observations (column 1), 718 of which are defined as lesbian women and 454 of which are defined as gay

men.

Using these totals in our dataset we can do a back-of-the-envelope calculation to determine what our sample sizes for individuals in same-sex and different-sex coresident partnerships might imply about the size of the overall gay and lesbian population in Brazil. Drawing on population percentages on sexual minority status and cohabitation from other studies, we find an implied percentage of the population that would be gay or lesbian in Brazil (0.6 %) that falls within the estimated range for the OECD (0.5 %–1.7 %). While there is quite a potentially wide range of estimates to draw on, our estimates are in line with what one might reasonably expect to observe given outside literature on population ratios.¹⁴

Turning to the demographic variables in [Table 1](#), comparing individuals in same-sex couples and different-sex couples, we see that individuals in same-sex couples appear to be generally younger, more female (718 vs. 454 observations), less likely to have children in the household and are higher educated. For the most part, these patterns hold for both gay and lesbian individuals, although lesbians are more likely to have children than gay men. No notable racial differences are observed across groups.

Turning to labor market variables, we consider employment, monthly income, weekly hours worked, hourly earnings and formal sector employment. Employment is measured as an indicator for whether the individual was employed at the time of the interview. This variable considers one to be employed if the person was employed in a position outside the household, was employed but taking the week off, or is working to benefit the household, including fishing, and completing construction. Both weekly hours worked and monthly income are provided in our dataset, measured as the average of these outcomes across the year. For example, monthly income reported is the average monthly income the individual receives across months in the year. We can therefore think of this outcome, for example, as capturing what one “typically” earns in a month. We construct hourly earnings using these two variables. As with employment, formal sector employment is directly reported at the time of the interview. Minimal missing values in this variable are filled in with whether the individual contributed to social security as a proxy for formal employment.

While we observe consistent patterns in demographic variables for both gay and lesbian individuals, for labor market variables we observe some differences by sex and sexual orientation. The pattern of differences that we observe in educational attainment between lesbian women and straight women (e.g., 32 % vs. 14 % with a bachelor or higher) is remarkably similar to the pattern of differences observed between gay men and straight men (e.g., 28 % vs. 13 % with a bachelor or higher). However, while the higher education levels are quite similar for gay and lesbian individuals compared to their straight counterparts, the labor market outcomes are not. Lesbian women are much more likely to be employed (80 % vs. 58 %), work in the formal sector (48 % vs. 29

¹³ It should be noted that these questionnaires did not have any designations for transgender or gender non-conforming individuals. However, we expect this to be a small percentage of our observations and any error introduced due to miss-classification in the data unlikely to impact the interpretation of our findings.

¹⁴ [Badgett et al. \(2021\)](#) document that roughly 1.4%–1.8% of the population is gay or lesbian when using US data, while the range is 0.5%–1.7% when using data for the OECD. The same paper also documents coresident partnership status. Using their numbers on the proportion of gay and lesbian versus straight individuals who are married or living together, our sample sizes would imply 0.2% of the population in Brazil is expected to be gay or lesbian. While [Badgett et al. \(2021\)](#) report percentages of the gay population in co-residing partnerships near 50%, not all studies report as high of a percent. [Martell \(2013\)](#) reports this figure closer to 20% for the US, which in our data would imply that 0.6% of the population are gay or lesbian in Brazil, falling within the estimated range for the OECD. That we see estimates on the lower end of the estimated ranges for the United States could imply that there is a lower proportion of gay and lesbian individuals living in Brazil compared to the US (e.g. geographic sorting of where individuals live based on characteristics) or Brazil could have lower rates of gay and lesbian individuals who are living together than in the US. Unfortunately, we cannot distinguish between these two possibilities with our data.

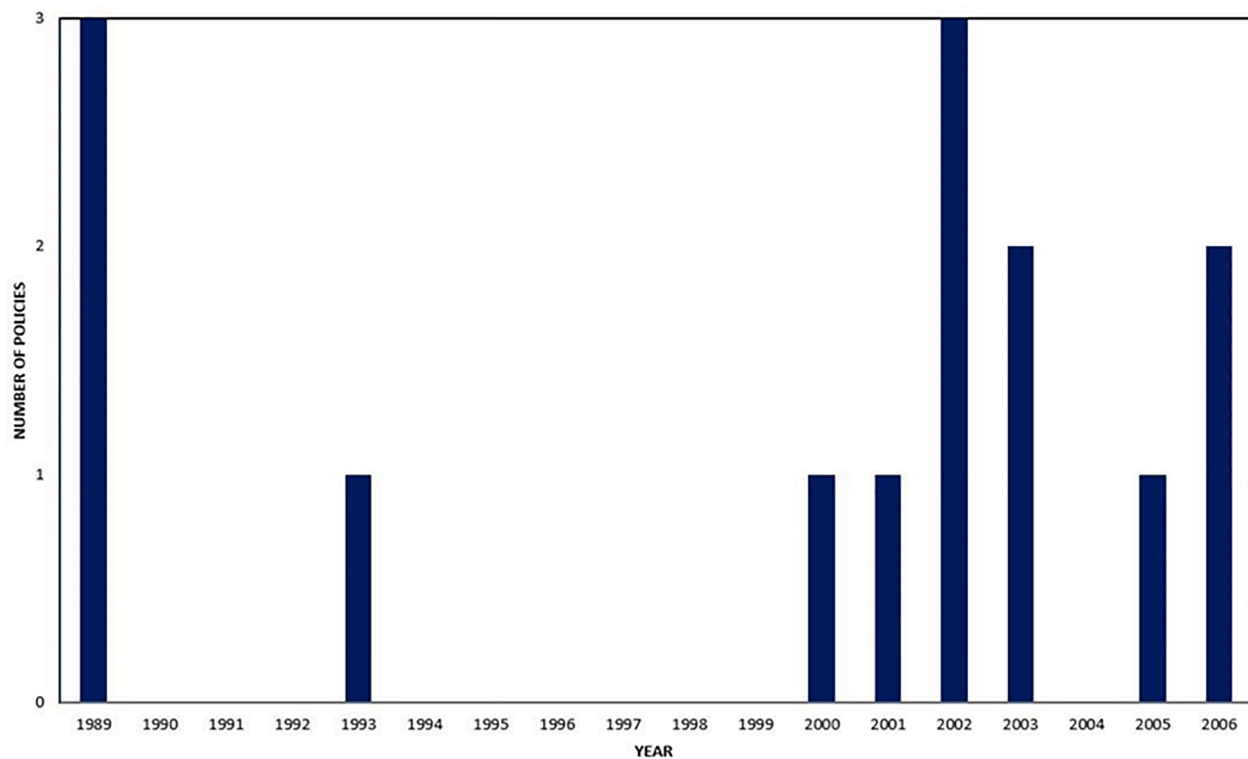


Fig. 1. Number of states to pass ENDAs for sexual orientation per year in Brazil

Notes: States by change years are as follows: 1989 Alagoas, Mato Grosso, Sergipe; 1993 Federal District; 2000 Rio de Janeiro; 2001 Sao Paulo; 2002 Minas Gerais, Santa Catarina, Rio Grande do Sul; 2003 Paraíba, Piauí; 2005 Mato Grosso do Sul; Maranhao, Pará.

%), work more hours (33 vs. 20) and earn more than twice the monthly income of straight women. These labor market outcomes are consistent with the higher education levels for lesbian women. In contrast, gay men have relatively similar probabilities of being employed and work similar hours on average compared to straight men. Gay men are also less likely to work in the formal sector (45 % vs. 54 %) and earn higher wages (22 % higher). The expected premium to their higher education levels appears to be much more muted for gay men, which is suggestive of the presence of discrimination for gay men.

3.2. Constructed variable for sexual orientation

The two main objectives of this study are: (1) examining existing labor market discrimination of lesbian and gay individuals and (2) estimating the effect of the passage of anti-discrimination legislation on labor market outcomes for lesbian and gay individuals. Both objectives necessarily involve the use of limited and inexact data due to the lack of comprehensive data including both labor market variables and sexual orientation. In our case, this means that our indicator variable for an individual in a same-sex cohabiting relationship may be measured with error, as we are proxying this with same-sex cohabiting non-relatives. This is a common potential concern in studies on sexual orientation. We therefore test that our constructed variable for cohabiting couples reliably measures what we intend to capture using Census data for Brazil. In Brazil, 2010 was the first year the census included a response in which individuals could identify the nature of their relationship to the reference person as *spouse/partner of the same sex*. This data therefore allows us to see how the directly reported measure compares to our constructed measure. In our main analysis, using the PNAD, a direct measure is not available.

A specific potential concern could be that we are picking up many cases of same-sex roommates instead of same-sex couples. This is the motivation for restricting our sample to those 30-64 in our analysis. Nonetheless, we provide descriptive statistics in Appendix Table 2

repeated for gay and lesbian individuals, where this sexual orientation variable is constructed in two distinct ways. For this analysis, we use only the 2010 Census data, applying the same sample restrictions as used for our main data (PNAD). We report summary statistics, first, using the direct measure in the Census 2010 for same-sex partnerships and, second, constructing a variable for such partnership in the Census 2010 data in the same way as we do in the PNAD data in our sample (as if the direct variable did not exist). Lastly, we provide the same summary statistics in our PNAD sample for comparison. Appendix Table 2 shows that summary statistics using our constructed variable align very closely to those for the recorded sexual orientation variable within the same dataset (Census), both for women and for men. Such close alignment mitigates (but does not eliminate) concerns that our variable construction might be picking up large groups of cohabitating same-sex individuals that are not in a relationship, such as roommates.¹⁵

While the profile of an individual in a different-sex versus same-sex relationship is remarkably similar between the stated same-sex couple status and the presumed status using our constructed method when applied to the Census data, the incidence in the population is still different.¹⁶ We cannot determine whether this difference might reflect error in our created variable or could reflect under-reporting of relationship status in the direct reporting variable in the Census data.

¹⁵ Using our PNAD data, we also show that same-sex couples in our sample show similar residing patterns (geographic distribution) as documented in Brazil's 2010 census. If this were not the case, it could indicate that our constructed sexual orientation indicator would be picking up some other individual characteristic or relationship status.

¹⁶ There are only 2729 individuals (1605 lesbian women and 1124 gay men) directly reporting their same-sex partner status in the census data (making up 0.1% of coupled individuals in the data), while there are 11,103 individuals (5485 lesbian women and 5618 gay men) presumed to be in same-sex relationships using our method (making up 0.4% of coupled individuals).

Table 1

Summary Statistics of Sample and Sub-samples (individuals 30–64 years old, in pairs with an age difference <10 years).

Sample:	Full Sample	Females in Different Sex Couples	Females in Same Sex Couples	Males in Different Sex Couples	Males in Same Sex Couples
Variables: Mean (SD)					
Male	0.523 (0.499)	0 (0)	0 (0)	1 (0)	1 (0)
SS Couple Identifier	0.00192 (0.0438)	0 (0)	1 (0)	0 (0)	1 (0)
Age	43.51 (8.907)	42.87 (8.555)	40.36 (8.766)	44.11 (9.174)	40.29 (8.697)
Household has children	0.839 (0.368)	0.839 (0.367)	0.396 (0.489)	0.840 (0.367)	0.0947 (0.293)
Number of Children	1.755 (1.284)	1.766 (1.292)	0.660 (0.994)	1.750 (1.276)	0.117 (0.395)
Education: Primary	0.564 (0.496)	0.559 (0.496)	0.292 (0.455)	0.568 (0.495)	0.383 (0.487)
Education: High School	0.308 (0.462)	0.310 (0.462)	0.393 (0.489)	0.306 (0.461)	0.339 (0.474)
Education: Bachelors	0.136 (0.343)	0.138 (0.345)	0.316 (0.465)	0.134 (0.341)	0.278 (0.448)
Race: Black	0.358 (0.480)	0.348 (0.476)	0.368 (0.483)	0.368 (0.482)	0.357 (0.480)
Race: White	0.634 (0.482)	0.646 (0.478)	0.630 (0.483)	0.624 (0.484)	0.639 (0.481)
Race: Other	0.00715 (0.0843)	0.00697 (0.0832)	0.00279 (0.0527)	0.00734 (0.0853)	0.00441 (0.0663)
Observations	609,888	290,177	718	318,539	454
Variables: Mean, (SD), Observations					
Employed	0.737 (0.440) 609,888	0.578 (0.494) 290,177	0.795 (0.404) 718	0.882 (0.323) 318,539	0.883 (0.321) 454
Monthly Income	715.0 (1,494) 602,308	368.9 (910.2) 287,884	937.9 (1,677) 700	1,032 (1,819) 313,282	1,268 (1,769) 442
Average Hours Worked	31.06 (22.51) 609,888	20.44 (21.13) 290,177	33.09 (21.26) 718	40.72 (19.11) 318,539	39.83 (19.74) 454
Hourly Earnings	4.359 (15.04) 602,308	2.644 (9.877) 287,884	5.976 (11.75) 700	5.927 (18.42) 313,282	7.430 (11.85) 442
Formal Employment	0.416 (0.493) 609,855	0.286 (0.452) 290,163	0.481 (0.500) 718	0.535 (0.499) 318,520	0.454 (0.498) 454

Notes: Means and standard deviations (in parentheses) are provided for demographic variables in the top panel, repeated for the full sample and sub-samples divided by gender and couple status. Sample and sub-sample observations are provided below. The bottom panel report the means, standard deviations (in parentheses) and observation counts for each labor market outcome. Similar summary statistics are reported for labor market variables conditional on employment in Appendix Table 1.

However, there are reasons to believe that our approximation may be more accurate than the direct reporting of same-sex cohabitating partnership. For example, [Ham et al. \(2024\)](#) use direct reporting of sexual orientation from household surveys compared to that from a list experiment (which allows respondent anonymity) for the case of Bogota, Colombia. Their results demonstrate that direct reporting of sexual orientation results in dramatic under-reporting (in their study: 1 %–4 % instead of actual 12–22 %). This is not unique to a single country context, as substantial underreporting in directly reported sexual orientation has also been found for the United States (e.g. [Coffman et al. 2017](#), [Black et al. 2000](#)).

As an additional test for possible miss-classification, we run our main specification for conditional labor market gaps that we present in the next section (see [Eq. \(1\)](#) in [Section 4](#)) also on the Census 2010 sample. For this test, we compare estimated conditional labor market gaps in the Census 2010 sample to the same estimation using our PNAD sample limited to only the years 2009 and 2011, drawing the most similar time comparison possible with our data. If our constructed same-sex couple indicators are miss-classified (e.g. by inclusion of heterosexual

individuals who are roommates), then the corresponding same sex couple estimates would be expected to be closer to zero than when the directly reported measure is used. This estimation is reported in [Table 2](#) and shows estimates that are quite similar. For example, estimates for income are nearly identical. Overall, estimates appear to align particularly well for the female sample. In the male sample, estimates are insignificant in the PNAD sample, while many are significant in the much larger Census sample. However, estimates are not systematically closer to zero in either the male PNAD or Census sample. While we cannot eliminate concerns of miss-classification of our variable of interest, since it is innately an approximation, we are reassured by this evidence, as well as by how closely aligned individuals defined by our constructed measure are in general compared to those defined by stated status.

4. Documenting discrimination

We still know remarkably little regarding the potential discrimination that sexual minorities face across contexts. This study is an effort to

Table 2
Comparable premium/penalties in Census and PNAD.

Panel A	Female sample				
	1 Employed	2 Ln Income (Cond.)	3 Formal Employment (Cond.) PNAD 2009 & 2011 (constructed)	4 Hours Worked (Cond.)	5 Ln Hourly Earnings (Cond.)
sscouple	0.127*** (0.0330)	0.246*** (0.0721)	0.00973 (0.0405)	4.196*** (1.024)	0.197** (0.0840)
Observations	44,032	23,169	26,849	26,849	26,169
R-squared	0.077	0.374	0.153	0.058	0.341
Census 2010 (direct report)					
sscouple	0.142*** (0.0108)	0.259*** (0.0230)	-0.00564 (0.0135)	2.335*** (0.478)	0.206*** (0.0258)
Observations	1,233,275	644,563	692,734	692,734	644,563
R-squared	0.090	0.334	0.147	0.026	0.298
Panel B	Male sample				
	1 Employed	2 Ln Income (Cond.)	3 Formal Employment (Cond.) PNAD 2009 & 2011 (constructed)	4 Hours Worked (Cond.)	5 Ln Hourly Earnings (Cond.)
sscouple	-0.00509 (0.0331)	0.0494 (0.103)	-0.0919 (0.0569)	2.503 (1.528)	0.0331 (0.101)
Observations	47,673	40,333	42,325	42,325	41,002
R-squared	0.111	0.352	0.116	0.027	0.320
Census 2010 (direct report)					
sscouple	-0.0461*** (0.0112)	0.0489* (0.0297)	-0.0190 (0.0155)	-2.405*** (0.526)	0.126*** (0.0339)
Observations	1,392,384	1,139,282	1,190,958	1,190,958	1,139,282
R-squared	0.098	0.331	0.128	0.022	0.289

Notes: This table reports the same specification for conditional labor market gaps (see Eq. (1) in Section 4) for both the constructed measure of same-sex couple status in the PNAD sample (limited to 2009 and 2011 - the most similar years possible) and using the directly reported variable in the Census 2010 sample. Robust standard errors in parentheses.

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$.

fill this gap. As far as the authors are aware this paper is one of the first studies to document labor market gaps by sexual orientation for a Latin American country, adding to the relatively few studies that do so for different countries (e.g. Ahmed et al. 2013, Plug and Berkhout 2004, Drydakis 2009, Bridges and Mann 2019, Brown et al. 2019).

As we already discussed in Table 1, descriptively, gay men and lesbian women differ from their straight counterparts in ways that have already been observed in the literature. In Brazil, we also see that gay and lesbian individuals are higher educated, as is regularly documented in the literature. Given that we observe education differences resulting in labor market differences for females, but not the same labor market rewards for higher education for gay men, this is highly suggestive of potential discrimination against gay men in the labor market in Brazil. We explore this further in Table 3, where we present estimation from labor market regressions including a control for same-sex couple status. We estimate the following specification separately for the male and female sub-samples using only data on individuals observed in pre-ENDA policy years, therefore providing baseline pre-policy conditional labor market gaps:

$$Y_i = \alpha + \beta_1 sscouple_i + \phi X_i + \lambda_t + \delta_s + \varepsilon_{it} \quad (1)$$

The dependent variables Y_i is the labor market outcome of interest. We use employment, formal sector employment, weekly hours worked, monthly income and hourly earnings of individual i in separate

specifications. Labor market variables are defined the same as discussed in Section 3 (in relation to Table 1).¹⁷ For regression analysis we follow the literature and express income and earnings in logs.¹⁸ In Eq. (1), an indicator variable for being in a same sex couple is included. The coefficient β_1 therefore gives the labor market differences between lesbian women relative to straight women in the female subsample and for gay men relative to straight men in the male subsample. X_i includes standard individual-level controls often included in labor estimation, such as education achievement dummies, race dummies, the presence and

¹⁷ Employment at the time of the interview considers employment in a position outside the household, being employed but taking the week off, or working to benefit the household, including fishing, and completing construction. Both weekly hours worked and monthly income are measured as the average of these variables across the year. Hourly earnings is constructed from these two variables. Formal sector employment is directly reported at the time of the interview. Minimal missing values in this variable are filled in with whether the individual contributed to social security as a proxy for formal employment.

¹⁸ Specifically, we add 1 to income and earnings to avoid taking logs of zeros. Chen and Roth (2024) warn that corresponding estimates based on these log-like transformations may not be interpretable as percentage effects. If we instead run Poisson regressions on unconditional earnings and income (e.g. columns 4 and 6 in Table 3), our unconditional income effects for females are smaller in magnitude, with no other notable changes to our results.

Table 3

Baseline pre-policy labor market gaps, unconditional and conditional on controls.

Reports estimate for same-sex couple indicator, using only data from before the policy change						
	1	2	3	4	5	6
	Employed	Formal Employment	Hours Worked (Cond.)	Ln Income	Ln Income (Cond.)	Ln Hourly Earnings
Female subsample, 30-64 years old						
SS couple indicator in specification with:						
(No controls)	0.238*** (0.0301)	0.195*** (0.0374)	7.641*** (1.245)	2.361*** (0.222)	0.370*** (0.0857)	0.627*** (0.0794)
(State/Year Effects)	0.243*** (0.0316)	0.209*** (0.0366)	7.433*** (1.310)	2.354*** (0.224)	0.362*** (0.0806)	0.624*** (0.0790)
(Controls + State/Year Effects)	0.169*** (0.0311)	0.102*** (0.0338)	6.432*** (1.333)	1.561*** (0.210)	0.128** (0.0584)	0.340*** (0.0655)
Observations	99,283	99,272	54,399	98,857	43,481	98,857
Male subsample, 30-64 years old						
SS couple indicator in specification with:						
(No controls)	-0.0114 (0.0253)	-0.0770* (0.0458)	-0.884 (1.536)	0.0349 (0.207)	0.0631 (0.0961)	0.0772 (0.0974)
(State/Year Effects)	-0.00667 (0.0255)	-0.0886* (0.0455)	-0.944 (1.536)	0.00181 (0.209)	0.00763 (0.0961)	0.0439 (0.0972)
(Controls + State/Year Effects)	-0.0342 (0.0251)	-0.143*** (0.0443)	-0.00949 (1.498)	-0.332* (0.194)	-0.148* (0.0818)	-0.140* (0.0801)
Observations	110,041	110,022	97,453	108,772	94,581	108,772

Notes: The table shows estimation of Eq. (1), using different outcomes reported in each column. The coefficient for the indicator for an individual in a same-sex couple is reported in separate rows, repeated for estimation done, first without any controls included, then adding just state and year dummies and lastly including relevant controls and state and year dummies. Each entry in the table therefore presents the coefficient of interest from a separate regression. Controls include age, age-squared, racial dummies, education dummies, an indicator for children are present in the household and the number of children present. See Appendix Table 3 for full estimation results. Standard errors in parenthesis.

*** $p < 0.01$,** $p < 0.05$,* $p < 0.1$.

number of children, age and age-squared. As the data is a repeated cross-section, individuals are only observed once (they have no variation over time t). Dummy variables are included for both year λ_t and state δ_s .

Each column of Table 3 reports the estimated coefficient of interest, β_1 , when a different labor market outcome is used. The outcome variable in Column 1 is whether the individual is employed; in Column 2 an indicator for whether the individual is employed in the formal sector; in Column 3 weekly hours worked conditional on being employed; in Column 4 natural log of income (transformed as defined above); in Column 5 the natural log of income conditional on being employed; and in Column 6, the natural log of the hourly earnings (see footnote 18). The coefficient for the indicator for an individual in a same-sex couple is reported in separate rows, repeated for estimation done, first without any controls included, then adding just state and year dummies and lastly including relevant controls and state and year dummies. Each entry in the table therefore presents the coefficient of interest from a separate regression.

From this estimation, we see that lesbian women are more likely to be employed, are more likely to work in the formal sector, work more hours and are paid more than straight women. These differences are smaller conditional on observable characteristics. However, even conditional on controls like age and education, lesbian women experience greater integration in the labor market (employment and hours). They also receive a higher income and earnings premium relative to straight women conditional on education and experience. Turning to results for men, one can see that without including controls, no statistically significant difference is observed between gay men and straight men in terms of income or hourly earnings, yet when a full set of controls is included (including education), gay men are paid less. This is consistent with the pattern observed and discussed in Table 1. Gay men are not rewarded equally for their higher education levels as straight men in the labor market. We also observe that gay men are less likely to work in the formal sector. This estimate is even larger when controls are included, possibly indicating that this is especially the case given their higher educational levels.

It is notable that in such a distinct context to Europe and the US,

where the majority of similar estimates originate, we still find very similar patterns in labor market gaps. We observe wage penalties for gay men and wage premiums for lesbian women conditional on individual characteristics. We also see lesbian women working more, and more hours, than straight women that cannot be explained simply by education and experience. Perhaps this should not be surprising, as the typically proposed mechanisms for such labor market effects would still apply to the Brazil context as well. That results remain consistent across context may lend support to mechanisms behind such gaps that are typically proposed in the academic literature, such as the role of gendered household specialization and expected partner income. For example, that lesbian women do not expect to partner with the typically higher male earnings, nor do they expect to take on the typical disproportionately-high gendered household workload, therefore investing more in labor market success even given similar education levels or presence of children.

Labor market premiums found for lesbian women could reflect either these types of responses to the expected spousal gender roles in domestic and professional work environments – or – could reflect a direct preference for lesbian workers. Likewise, the wage penalty found for gay men could reflect that some partnered gay men rely on typically higher male earnings of their partner and therefore choose to specialize less in the labor market and more in home production – or – could reflect a direct preference-based discrimination against gay men in the workplace. For lesbian women, we view the mechanisms related to behavior in response to household incentives as more likely than a direct preference for lesbian workers, as research using resume studies (where behavioral factors cannot influence results) have found direct discrimination effects for lesbian women (e.g., Drydakis 2015, Weichselbaumer 2003). Regarding gay men, while we cannot rule out similar behavioral responses to household incentives, the fact that we do not find statistical differences in employment or hours, where one would expect such choices to be reflected, the wage penalty found for gay men is likely to at least partly reflect discrimination. As with lesbian women, direct discrimination toward gay men has been documented in resume study research as well (e.g., Tilcsik 2011, Drydakis 2015).

The results provided in Table 3 establish new evidence on pre-policy-intervention gaps in the labor market for sexual minorities in the Latin American context – for Brazil. It is also useful to see whether and how such gaps have evolved over time. We therefore show the evolution of such penalties and premiums in Fig. 2. As with Table 3, we use the same sample of states that are used in estimation of ENDA policy effects, but unlike Table 3, we do not limit observations to pre-policy data. Due to the potentially small number of observations for partnered and cohabitating gay and lesbian individuals per year, we plot the penalty/premium estimates for gay and lesbian individuals taken from regressions on samples consisting of 3 waves, where we gradually drop and add waves. The first estimate presented in each figure uses the data from 1996 to 1998, followed by 1997–1999, and so on.

The clearest pattern seen in Fig. 2 is for positive employment gaps for lesbian females. The higher employment that lesbian women experience compared to straight women rises until roughly the year 2000 (1999–2001) where it begins to fall but remains positive over the whole range of years in our sample. We can also see a pattern for lesbian women in terms of income premiums, which despite variation also remain positive across all sample years. Here we see the income premium for lesbian women compared to straight women fall until 2000–2002, where it begins to rise. For gay men, we do not observe any notable evolution in the employment or income estimates over time. For income, the point estimates are regularly below the zero line, indicating a potential penalty, but only become significant in a few years. However, we do see a similar pattern to that for lesbian women when looking at the income penalties for gay men conditional on employment. Here estimates also appear to dip by 2000–2002 and then rise.

While ENDA policy analysis requires the use of a sample limited to those states that experience a policy change within our sample period, establishing labor market penalties and premiums by sexual orientation does not. For comparison-sake Table 3 and Fig. 2 are limited to the same sample of states as used in subsequent estimation throughout this paper. Nonetheless, we provide additional analysis using the full sample of Brazilian states in Appendix Table 4 (comparable to Table 3). Despite small variations, results are generally very similar.¹⁹

Overall, the evidence presented in Table 3 shows pre-policy-intervention penalties for gay men and premiums for lesbian women compared to their straight counterparts. This provides a baseline for the potential impacts of the ENDA policy to impact such pre-existing labor market gaps. We have also seen changes in employment and income premiums (for lesbian women) and penalties (for gay men) that evolve over our sample period. Notably, changes in such patterns appear to begin around the year 2000, which in the context of Brazil is also when ENDA policies begin to be implemented within our sample, starting with Rio de Janeiro in 2000, with subsequent policy changes in other states nearly every year after that (see Fig. 1). Fig. 2 is therefore suggestive of potential policy impacts and emphasizes the need for such analysis. We now turn to estimating the impacts of ENDA legislation aimed at closing such gaps in Brazil.

5. Empirical strategy: policy impacts

In this study we make use of a difference-in-differences (D-D) approach to identify the causal impact of employment protections for sexual minorities. We compare the labor market outcomes for gay and lesbian individuals before and after the policy implementation in their state of residence to the difference in labor market outcomes for similar straight individuals. As we use male and female sub-samples for our

¹⁹ Using the full sample of Brazilian states, we do not find statistically significant income and hours penalties for gay men, which were significant in the reduced sample. Comparing the evolution of penalties and premiums over time (see Appendix Fig. 2), the observed inflections in the year 2000 in Fig. 2 are muted by the addition of these non-policy states.

analysis, this means that we use heterosexual women with similar characteristics as a counterfactual group for lesbian women, and heterosexual men with similar characteristics as a counterfactual group for gay men (proxied by individuals in different-sex versus same-sex partnerships). Both groups experience the same state-specific characteristics, such as the local labor market, but only lesbian and gay individuals experience a legal change in their labor market protections when the policy change occurs in their state.

In our setting, policy implementation varies in timing across states. Until recently, economists regularly estimated D-D with this type of staggered implementation using a two-way fixed-effects approach.²⁰ Recent developments in D-D methodology however have shown that for this estimation to produce unbiased average policy estimates requires not only a parallel trends assumption but also an assumption of no policy impact heterogeneity over time within state (i.e. group) (Goodman-Bacon, 2021).²¹ It is quite likely that a policy change within any given state may have heterogeneous effects over time. For example, it may take a few years for policy impacts to appear or effects that start large may diminish over time. Since the standard ways of estimating D-D in this setting has been shown to exhibit potentially substantial bias, (even resulting in estimates of flipped sign), new methods have developed in recent years to address this concern.²² We use the method proposed by Callaway and Sant'Anna (2021) that circumvents the potential bias introduced by the comparison between early and late adopters in estimation by instead calculating the group-year effects separately for each policy change. This can be written as:

$$ATT(g, t) = E[Y_t - Y_{g-1} | G_g = 1] - E[Y_t - Y_{g-1} | C = 1]$$

Where the time period when an individual first becomes treated is denoted by G . Distinct treatment groups (g) are therefore the same for all individuals experiencing a policy change in the same year (t).²³ Looking at Fig. 1, for example, we can see that San Paulo would be in a group of its own, being the only state to change in the year 2001, while three states would enter into a group together for experiencing a policy change in 2002 (Minas Gerais, Santa Catarina, Rio Grande do Sul). The control group (C), is comprised of heterosexual individuals in our study, as they are never treated (i.e. the policy does not apply to them even when it turns on in their state). In this case, just as in a traditional two-way D-D (i.e. with controls “after”, “treatment” and their interaction), comparisons are always made before and after the same policy change year (i.e. same group), between individuals in same-sex (treated) versus different-sex (not treated) couples. These group-year treatment effects can then be represented visually (as with event study analysis) or aggregated to an average treatment on the treated (ATT) policy effect. In the next section, we present evidence on ATT estimates, event study graphs and various checks for the robustness of our findings.

6. Results

Main policy estimates are presented in Table 4 for males and females separately. The corresponding event study estimation is visually presented in Fig. 3 below. The columns of Table 4 are in the same order as

²⁰ This refers to a regression including both state and individual fixed effects, as well as a time-varying treatment variable, which in this case would be a dummy that turns to 1 for a treated individual when the policy is implemented in their state.

²¹ Specifically, the bias in the two-way fixed-effects estimation arises because, with differential timing in implementation, post-policy years from some states serve as part of the “control” group for other states’ policy impacts. Newly developed estimators correct for this by making use of only those “never treated” or “not-yet-treated” units as a control.

²² See, for example, De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021) and Callaway and Sant'Anna (2021).

²³ $G_g = 1$ if a unit is first treated in period (g).

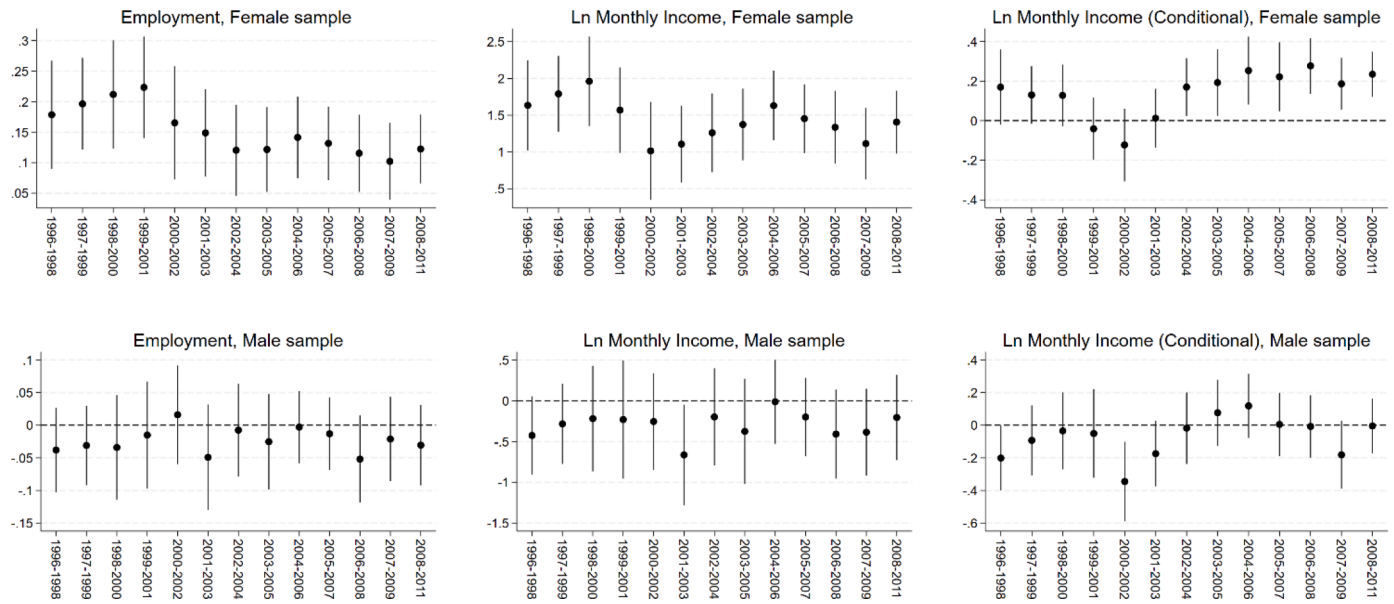


Fig. 2. Evolution of the penalty/premium for same-sex couples over sample waves, full sample years

Note: Estimation of Eq. (1) is run separately for combinations of 3 waves at a time (e.g. 1996–1998, 1997–1999, 1998–2000,...). The final wave includes 2008, 2009 and 2011 as 2010 is missing. These estimates are then plotted over time. While the estimation equation is the same as presented in Table 3, in these figures, all observations are used in estimation, including both pre- and post-policy observations for each state.

Table 4

Estimated policy impacts, male and female gender sub-samples, 30–64 years old.

Panel A	Female sub-sample					
	1 Employed	2 Formal Employment	3 Hours Worked (Cond.)	4 Ln Income	5 Ln Income (Cond.)	6 Ln Hourly Earnings
ATT	-0.0994** (0.0470)	0.0254 (0.0842)	-2.514 (2.483)	0.0223 (0.417)	0.333*** (0.100)	0.324** (0.127)
Observations	290,864	290,850	168,156	288,553	140,287	288,553
Panel B	Male sub-sample					
	1 Employed	2 Formal Employment	3 Hours Worked (Cond.)	4 Ln Income	5 Ln Income (Cond.)	6 Ln Hourly Earnings
ATT	0.0314 (0.0565)	0.0967 (0.0822)	0.132 (2.766)	0.265 (0.420)	0.372** (0.161)	0.249 (0.189)
Observations	318,978	318,959	281,280	313,709	271,446	313,709

Notes: ATT policy effects using the Calloway and Sant-Ana (2021) method described in Section 5 are reported here for different labor market outcomes in each column. Controls include age, age-squared, racial dummies, education dummies, an indicator for children are present in the household and the number of children present. Standard errors in parentheses.

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$

presented in Table 3: Column 1 reports ATT estimates for the outcome of being employed; Column 2 for being employed in the formal sector; Column 3 for hours worked conditional on employment; Column 4 for income; Column 5 for income conditional on employment; and Column 6 for hourly earnings. Variables are the same as previously defined (see Sections 3 and 4).

From Table 4, we see that the ENDAs in Brazil resulted in some separation from the labor market for lesbian women, with reduced employment. However, conditional on employment, lesbian women experience higher hourly earnings and higher income post-policy. For gay men, we see no changes to employment but, conditional on employment, they also experience higher income post-policy. Fig. 3 plots corresponding event study estimates. Likely due to the smaller

sample sizes for gay and lesbian women and how demanding of the data this estimation method is, these estimates jump around a bit and some pre-policy estimates are significant. While we would caution against trying to interpret any one year's estimate directly, we look to these figures to potentially better understand the average treatment effects presented in Table 4.

For example, for lesbian women (Fig. 3, Panel A) one can see the effects found in Table 4. The negative employment effect, as well as the positive conditional income and earnings effects are all evident in the overall patterns in the figures. For gay men, only a conditional income effect was observed in estimation. Though less pronounced than in the female sample, this pattern is observable for gay men in Fig. 3. Overall, estimation results presented in Fig. 3 are generally supportive of findings

from the main estimation.²⁴

To test for the robustness of our findings, we conduct a number of tests. Our main findings are quite robust to how controls are treated in our estimation. We therefore test all specifications with the full set of controls (Table 4), with a limited set of controls (age, age-squared and racial dummies) and no controls (see Appendix Tables 6 and 7). Additionally, we run specifications limiting the sample to observations within a limited band of years from the policy change year. Variation in implementation dates naturally results in estimates further from the policy change year being identified on few observations, as more states automatically fall outside of the range of data years in our sample. We cap this at 5 or fewer years before the policy was enacted and 8 or fewer years after the policy was enacted (see Appendix Fig. 3). Estimation using the capped sample is shown in Appendix Table 8 and is supportive of our main findings.

Each specification presented thus far has been run on our sample of individuals aged 30–64 with a difference in partner ages of 10 years or less. We also test for robustness to changes in these two sample assumptions. First, considering a trade-off in terms of increased observations included in the sample versus greater room for misclassification (e.g. roommates), we re-run our main estimation using the more permissive age range of 26–64. While some estimates are weakened by including these younger individuals in the sample, estimation using this alternative age range supports our main findings (see Appendix Table 9). The second sample restriction we use in this paper is limiting the age difference between presumed couples to 10 years, which is based on research on preferred age differences in couples. We test the robustness of our results to alternative thresholds and find results are largely robust to how this is defined (see Appendix Table 10).²⁵

Lastly, we provide tests regarding our sample composition (see Appendix Table 11). A concern when using state-level policy changes for identification is that people can move across states and changes in labor market differentials could reflect changes in the types of workers living in states, rather than changes to the individuals who had been living in those states when the policy was introduced. We therefore test for policy impacts on two variables measuring internal migration: a dummy variable for whether the individual had moved recently (in the past 5 years) and a continuous variable for how long the individual had lived in their current state of residence. In both cases, we can see that the policy had no impact on movement across states in Brazil. A second concern is that the policy may directly change the population recorded as gay or lesbian. We therefore check whether the passage of the ENDA policy impacts the proportion of coupled individuals identified as being in a same-sex couple. We alter our policy impacts specification so that the outcome variable is the dummy for being in a same-sex couple, where treatment happens to all individuals in a state when that state implements the policy change and identification comes from comparison of individuals in states that adopt the ENDA policy in a given year to

individuals in “not-yet-treated” states. This estimation shows no evidence of a change in the proportion of people entering same-sex couple cohabitating relationships because of the policy (see Appendix Table 11).²⁶

7. Discussion

We run multiple variations to our main specification presented in Table 4 that are robust to (i) the control variables included and (ii) how we define our sample restrictions based on years around the policy change, ages and couple age differences. We also test various potential threats to identification of the causal estimation of policy impacts that could arise if the sample were to change over time. Considering this evidence together we can draw some overall conclusions.

For lesbian women, we estimate robust evidence of post-policy lower employment. The implementation of ENDAs protecting lesbian women from employment discrimination in Brazil resulted in labor market engagement for lesbian women becoming more similar to straight women (roughly half of the positive employment gap for lesbian women is eliminated). This same convergence of labor market outcomes between gay and straight men and lesbian and straight women has been recently documented in other studies as well (Burn, 2018; Delhomme and Vamossy, 2024). Delhomme and Vamossy (2024) finds that this post-policy labor market convergence coincides with lesbian women having more children, resulting in one woman within the pair accommodating this change by taking on fewer working hours. We therefore test whether the policy in Brazil impacted having children or the number of children for lesbian versus straight women, finding no statistically significant impacts.

To understand why we might observe lower employment, it is worth considering mechanisms behind the initial pre-policy gaps. Lesbian women experienced wage premiums and a greater incorporation in the labor market even conditional on their individual and work-related characteristics. One proposed mechanism for this initial lesbian premium is that lesbian women work and earn more than straight women because lesbian women make their labor-market decisions with the assumption that they will not be in a relationship with a higher-earning male partner or be in a household with traditional roles, in which the woman specializes in non-market production (e.g., Black et al. 2003). In this case, if there is an underlying desire for children, with job security one might expect to see increased parenthood post-policy, which we do not observe. An alternative explanation is that lesbians’ higher engagement in the labor market (even conditional on education) reflected compensating behavior in the face of discrimination (both their own, as well as lower expected partner earnings). Regardless of preferences for children, one would expect increased job security to potentially reduce such behavior.

It is, however, still unclear whether the post-policy reduction in labor market integration should be interpreted as a beneficial or detrimental outcome for lesbian women. To the extent that the policy enables lesbian women to feel sufficiently secure in their jobs to be able to voluntarily pull back from the labor market to some degree (e.g., reduction in compensating behavior and/or choosing to specialize in home versus labor market work), this could be interpreted positively from an individual perspective. Nonetheless, it represents a reduction of female involvement in the labor market in Brazil. On the other hand, we also find that those lesbian women who remain engaged in the labor market earn more, which is a clear positive impact of the policy.

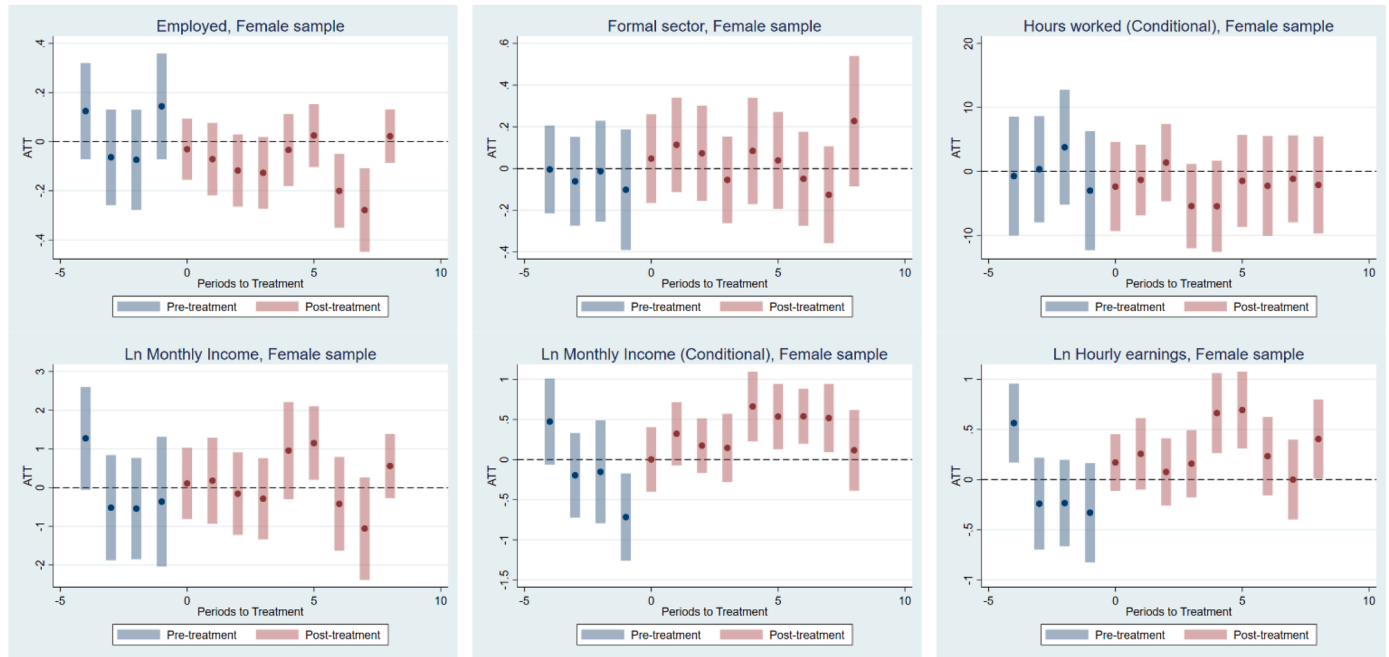
Turning to results for males, we can conclude that the ENDA policy

²⁴ An alternative approach to using newly developed methods designed for addressing heterogeneity in results over time is to center the data at the change year, effectively removing the differential timing in implementation in the data, and then treat the estimation as a simple two-way D-D (i.e. with controls “after”, “treatment” and their interaction). Doing so, however, cannot include any controls or consideration for year effects, as this would reintroduce the same bias problem as with the two-way fixed effects model. We present the results from this alternative specification in Appendix Table 5, observing fewer significant estimates. Only conditional hourly earnings for lesbians and conditional income for gays are positive and significant – consistent with our main results – but smaller in magnitude. As with results from Table 3, these results are also robust to instead using Poisson regressions for unconditional income and hourly earnings, as suggested by Chen and Roth (2024).

²⁵ Thresholds for age differences used in Appendix Table 12 include: 8 and 12 (alternative nearby maximums observed in the literature) as well as 14 and 22 year differences (corresponding to the 95th and 99th percentile in the distribution of age differences between non-relatives co-residing in the sample).

²⁶ This is perhaps unsurprising. The law prohibiting firing of workers based on sexual orientation could directly impact the proportion of individuals who openly identify as gay or lesbian, but may not impact the proportion of gay or lesbian individuals who have a cohabitating couple status, which is what we are able to measure in our data.

Panel A: Female sample



Panel B: Male sample

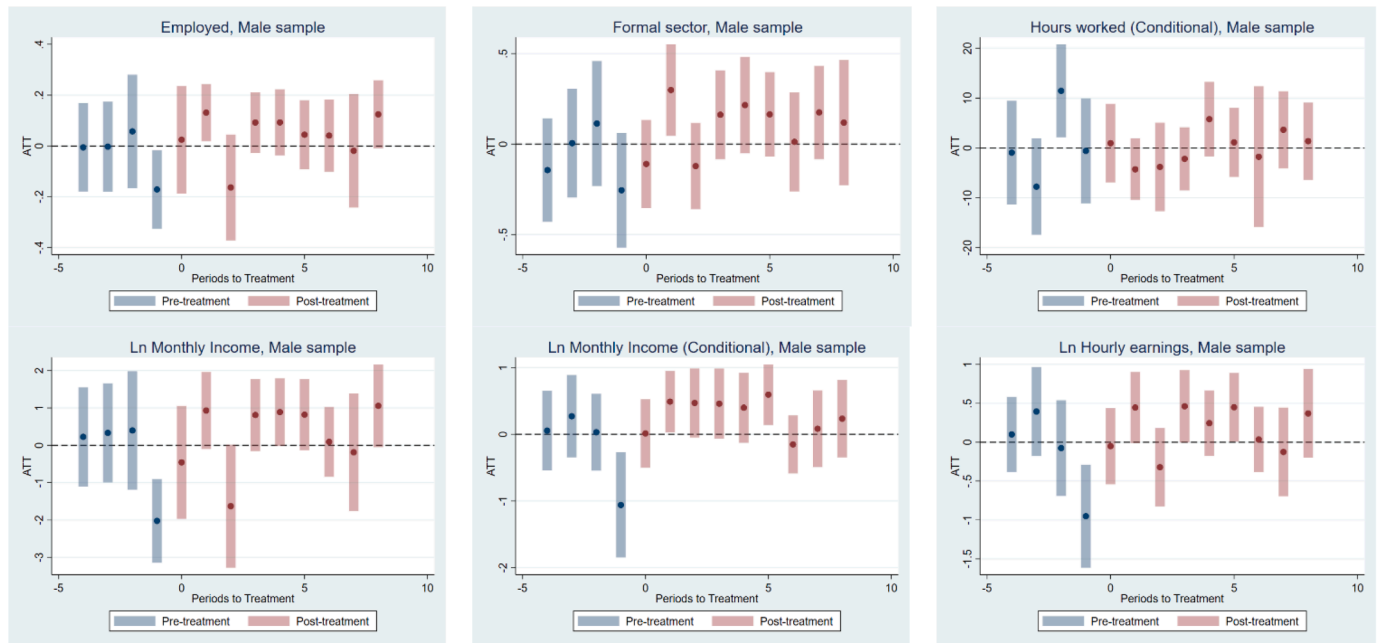


Fig. 3. Event study graphs by gender, observations capped at 5 years before the policy change to 8 years after the policy change.

Panel A: Female sample

Panel B: Male sample

Notes: Figures report event study style graphs corresponding to the estimation reported in Table 4.

implementation resulted in increased income conditional on employment for gay men. This is robust across all specifications. Overall, our combined estimation shows positive policy impacts for gay men. As we previously documented, gay men faced an income penalty pre-policy. While our estimates for the policy impacts vary in magnitude across specifications, they are large enough to imply that this gap is nearly or fully closed in the years following the policy. The positive income effects

are exclusively concentrated on those who are employed (conditional sample), as opposed to income gains through employment gains. This is a quite notable effect. We find that the policy increased wages for gay men relative to straight men by around 30 %. The literature is mixed in terms of magnitudes of ENDA policy impacts for gays and lesbians, with some studies finding quite small effects of 2-3 % (e.g. Burn 2018, Gates 2009), others around 8 % (e.g. Klawitter 2011, Baumle and Poston

2011) and others finding impacts closer to 10–20 % (e.g. [Delhommer and Vamossy, 2024](#), [Martell 2013](#)). Our estimates are clearly on the larger side of the existing research for the United States, yet we have no comparison for other countries than the US context. While the ENDA in Brazil did have clear positive impacts for gay men, we should also note that it did not close all negative gaps, having no impact on the (14 %) lower rate of formal employment experienced by gay men.

It is also worth noting that some of the income effect sizes mentioned above come from estimation that fully controls for occupations (e.g. [Burn 2018](#), [Gates 2009](#)), while other studies do not include occupation dummies in their main estimation (e.g. [Martell 2013](#), [Delhommer and Vamossy, 2024](#)). Even when available in the data, occupational dummies may be excluded from estimation due to the potential endogeneity of this control, as the policy could impact occupational decisions. While the PNAD data used in this study does have detailed occupational codes, we have relatively small sample sizes of gay and lesbian individuals and do not have enough power to make use of occupational dummies in our estimation. It is possible that our larger income effects for gay males reflects a mixture of both income increases for those who remain in the same occupations, as well as income increases through individuals making different occupational choices due to the law change. We know from literature on gender gaps that occupations play a large role in remaining gender gaps in the developed world (e.g. [Blau and Kahn, 2017](#)). Research is limited on the importance of occupations in wage gaps for sexual minorities. On one hand, [Antecol et al. \(2008\)](#) finds no impact of occupational sorting in explaining wage premiums and penalties. On the other hand, there is evidence that gays and lesbians make career choices in response to prejudice in both their occupations ([Plug et al., 2014](#)) and fields of study ([Burn and Martell, 2020](#)). It is therefore reasonable that an ENDA protecting sexual minorities could alter occupational choices and that such changes could explain a potentially important part of the positive impacts we find for the ENDA in the case of Brazil.

Lastly, we consider the most direct mechanism through which the ENDA protecting sexual minorities in Brazil could have impacted labor market outcomes for such groups - the strict enforcement and use of such laws. There is evidence that utilization of US state-level ENDAs protecting sexual minorities is consistent across states despite heterogeneity in the laws and that utilization of laws is similar to that of discrimination protection laws based on race or sex ([Martell, 2013](#); [Ramos et al., 2008](#)).²⁷ Unfortunately, we do not have any direct evidence regarding enforcement of the ENDAs protecting sexual minorities in Brazil.

However, there are reasons to believe that enforcement would have occurred. Almeida and Carneiro (2007) and [Cardoso and Lage \(2006\)](#) both study enforcement of labor regulations in Brazil and provide comprehensive reviews of the labor market context. They both note that labor inspections did not play an important role in the labor market until the second half of the 1990s, after regulations became quite strict with the implementation of the 1988 Constitution, and when governments started using fines from labor inspectors to gain revenues to counter government deficits. In the current context (past 2-3 decades), inspections often originate from anonymous reports that can be initiated by workers, unions, the public prosecutor's office or by police. In addition to the ease of reporting, labor inspectors are likely to take their jobs seriously. They are highly qualified and relatively well paid and are incentivized to enforce regulations with fines. Almeida and Carneiro (2007) also note that labor inspectors have little incentive and therefore a low incidence of collecting bribes. Both studies also emphasize that unions have significant power in the labor market and help with enforcement of labor regulations as well. Additionally, [Cardoso and](#)

[Lage \(2006\)](#) note that “in legal and union circles” the fine amounts are “considered reasonable and high enough to curtail breaches of the law” (p.6). While there appears to be a relatively well-functioning role of inspections and fines in enforcing regulations regarding issues such as severance pay and health and safety reports ([Almeida and Carneiro, 2007](#)), the system is not without critiques. [Cardoso and Lage \(2006\)](#) report that around 80 % of formal workers are covered by labor inspections, with 16 %–30 % of visited companies being cited each year, yet they also note that this high coverage of formal workers is achieved by targeting larger companies more heavily. They emphasize that “while the system has improved, it does not fully satisfy what is perhaps its greatest goal: to curtail the rate of illegal labor relations in Brazil” (p.3). Enforcement of labor regulations does not reach unregistered companies as they have no records and there is very low fining of official companies for employment of non-registered workers. In the context of our findings, it is therefore not surprising that we see no movement in the pre-policy gaps in formal sector employment. Improved conditions in the formal sector through more frequent inspection could lead to movements across sectors due to expanding (contracting) labor supply in the formal sector (informal sector), paired with contracting labor demand in the formal sector (e.g. [Almeida and Carneiro 2007](#)). However, [Cardoso and Lage \(2006\)](#) argue that this is specifically the margin for which labor regulations have largely failed to impact.

While these studies would indicate that it is quite likely that the ENDA was enforced, even without enforcement of laws, we could see changes in societal attitudes, which could then directly impact labor market outcomes for affected groups. Research has highlighted this additional important channel through which anti-discrimination policy can have impacts. For example, [Aksoy et al. \(2020\)](#) show that legal relationship recognition laws in Europe improved attitudes toward sexual minorities, while [Ofosu et al. \(2019\)](#) showed that same-sex marriage legalization in the US was followed by a reduction in implicit and explicit antigay bias. There are various channels through which the ENDA in Brazil may have impacted sexual minorities, including direct impacts through enforcement of the law, by inducing changes in occupational choices on the part of gay and lesbian individuals or through changes in societal attitudes leading to less labor market discrimination.

8. Conclusions

In this study, we tackle two main objectives. First, we document labor market gaps, both unconditional and conditional on individual and work-related characteristics for gay and lesbian individuals in Brazil. We find pre-policy labor market premiums for lesbian women and labor market penalties for gay men. The evidence we provide regarding baseline labor market differentials between sexual minorities and their straight counterparts is consistent with previous research that has also found differential wage premiums or penalties conditional on individual characteristics for gay and lesbian individuals (e.g., [Black et al. 2003](#), [Antecol et al 2008](#)). As mentioned, evidence on labor market gaps by sexual orientation for diverse geographies is particularly scarce, as most existing literature has focused on Europe and the United States. We therefore provide the first evidence that this general pattern in differential labor market gaps by sex still holds in the notably different context of a Latin American country - Brazil. This evidence adds to the growing literature that documents labor market gaps for sexual minorities.

As our second objective in this paper, we analyze the effects of Brazil's legislation forbidding discrimination in the labor market based on sexual orientation. As far as we are aware, we present the first analysis to study the impact of ENDAs based on sexual orientation outside of the US context, adding to the still relatively small overall literature on such policies. Consistent with the existing literature, we estimate different impacts of ENDAs on gay men versus lesbian women. We estimate robust positive income effects of the policy for gay men that

²⁷ [Martell \(2013\)](#) notes that while utilization is not the same as enforcement, higher utilization may be indicative of effective enforcement mechanisms since the marginal benefits of filing of a claim depends on believing something could change by doing so.

counter most of the prior levels of labor market discrimination. This relatively large policy impact likely reflects a combination of policy impacts through enforcement of the laws, through changes in societal views and through possible occupational changes on the part of gay men. While we cannot disentangle these mechanisms in this paper, we can state that legislation providing employment protections for sexual minorities produced clear positive impacts on gay men in Brazil. However, we also document that the ENDA policy does not successfully alter the pre-policy lower incorporation in the formal sector experienced by gay men relative to straight men. For lesbian women, we find that the policy resulted in disengagement from the labor market, along with higher hourly earnings and income conditional on employment. There are various proposed mechanisms behind the initial pre-policy labor market premiums that could explain the mixed policy findings for lesbian women in Brazil. As the larger literature on labor market outcomes from sexual minorities is still emerging, hopefully future research can further directly test some of these mechanisms.

In interpretation of our findings, we must acknowledge that the data used in this research has some limitations. Our sample is limited only to individuals within a cohabitating relationship. Because of this, our analysis is missing information for every gay man or lesbian woman who was either single or living separately from their partner at the time of the survey.²⁸ While census questionnaires in many countries are beginning to allow more inclusive responses regarding one's relationship to the head of household, making sample selection easier, they still fall short of including sexual minorities not in a cohabitating relationship. Ideally, future data collection efforts will adapt to include questions related to sexual orientation in a standard way, such as is done for race or gender. This will allow for better understanding of a wider range of LGBTQ⁺ experiences and can help with often limited sample sizes in the research on sexual minorities.

That we find positive impacts of this ENDA policy in Brazil is promising, showing that some of these policies that can be thought of as "first steps" toward equality can do something to move society forward. However, there may be further welfare impacts of such policies beyond the direct labor market outcomes used in the current literature. *Sears and Mallory (2011)* note that LGBTQ⁺ workers who feel they are not accepted at their workplace will look for new jobs or stay home and that LGBTQ⁺ employees of an LGBTQ⁺-accepting workplace have higher rates of satisfaction. *Barron (2009)* and *Delhommer and Vamossy (2024)* find employment nondiscrimination legislation for sexual orientation to be related to decreased prejudice toward gay men. Thus, it could be the case that post-policy implementation, members of the LGBTQ⁺ community may find their employer more accepting and find work more enjoyable, resulting in a better quality of life, irrespective of any effect on their income. These "secondary" impacts of the policy are not explored in this paper and are areas for further research.

CRediT authorship contribution statement

Jennifer Graves: Investigation. **Christopher Trond:** Investigation.

Declaration of competing interest

None

²⁸ *Carpenter and Gates (2008)* document key differences between partnered and non-partnered gays and lesbians, finding partnered lesbian and gay individuals to be more likely to be white and higher-educated. They note how differences in selection into partnership by sexual orientation is an important consideration, as many datasets can only identify sexual orientation within partnership status.

Data availability

Data will be made available on request.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at [doi:10.1016/j.labeco.2024.102548](https://doi.org/10.1016/j.labeco.2024.102548).

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