What Same-Sex Adoption Laws Can Tell Us About the Gender Wage Gap in the United States

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Abstract
Gender wage gaps are frequently explained as resulting from direct discrimination, employers’ preferences over personality traits, and differing labor force attachment. We rely on a natural quasi-experiment using exogenous changes in state-level, same-sex adoption laws to distinguish between the competing explanations of the gender wage gap. Estimates from a differences-in-differences model show the wage gap between lesbians and heterosexual women shrank or inverted in those states which legalized adoption by same-sex couples. The wage gap did not change for men. This supports the parenthood hypothesis as a viable explanation for a portion of the gender wage gap.

Keywords Discrimination · Gender · Sexual orientation · Wage differentials

JEL Classification J16 Economics of Gender · Non-labor Discrimination · J31 Wage Level and Structure · Wage Differentials · J71 Discrimination · K36 Family and Personal Law · K38 Human Rights Law · Gender Law

Introduction
There is a persistent wage gap between men and women, as well as by sexual orientation. The “stylized facts” are that men earn more than women, heterosexual men earn more than gay men, lesbians earn more than heterosexual women, married men earn more than single men, and single women earn more than married women. Several hypotheses have been proposed to explain these gaps, including direct discrimination based on gender and sexual orientation; employer preferences over personality differences that stereotypically differ by gender and sexual orientation; and expectations of, and actual differences in, labor force attachment caused by the asymmetric burden of caring for family members, particularly children. To test this “parenthood hypothesis,” we rely on a natural quasi-experiment using exogenous changes in state-level adoption laws allowing same-sex couples to adopt children in the United States. In short, we found that the various wage gaps change as would be predicted when the opportunities for, and costs of, parenthood are differentially altered across population subgroups.

The structure of our paper is as follows. First, we review the literature concerning the stylized facts of wage gaps, and then review prominent competing theories explaining these facts. We then discuss our data and the method we used to distinguish between these theories. Next, we report our results and examine the robustness of these results to alternative specifications. Finally, we conclude with a discussion of our results and directions for further research.

Literature Review
We begin by briefly summarizing two components of the vast literature on discrimination and wage gaps. First, we discuss the existence and magnitude of the wage gaps related to gender, sexual orientation, and marital status. We then summarize three sets of alternative explanations for these gaps: direct discrimination, personality differences, and the parenthood hypothesis.
Gender and Sexual Orientation Wage Gaps

Men earn more than women on average, although the precise difference, and whether it exists for all subgroups once other factors have been accounted for, is still a topic of much discussion. Blau and Kahn (2017) provide a concise summary of the current economic literature related to the gender wage gap. They found the 2010 gender wage gaps to be 20.7% (unadjusted), 17.9% (adjusting for education, experience, race/ethnicity, region, and metro area residence), and 8.4% (further adjusting for industry, occupation, government employment, and union membership). These gaps shrank quickly during the 1980s but the rate of convergence has slowed dramatically since then.

A significant portion of the decline in the gender wage gap occurred because of convergence in human capital-related characteristics. Women have closed and even reversed the educational attainment gap, earning 57% of bachelor’s degrees, 61% of master’s degrees, and 51% of doctoral degrees in 2011. Women have also narrowed the experience gap from 7 years in 1981 to 1.4 years in 2011, and the unionization gap from 13% in 1981 to −2% in 2011. The gender gap in occupations has also narrowed, particularly for college-educated women in managerial and professional occupations. There are still significant opportunities for improvement: Women are underrepresented at the highest levels of organizational leadership, earnings across occupations and industries still favor male-dominated areas, and women are still underrepresented in STEM careers and other math-intensive fields.

The literature has also established that lesbians earn a wage premium over heterosexual women and that heterosexual men earn a wage premium over gay men. Klawitter (2015) conducted a meta-analysis of 31 studies of the wage gap. She found evidence of the lesbian wage premium, with lesbians earning 9% more on average than their heterosexual counterparts. The meta-analysis also found that gay men earned 11% less on average than their heterosexual counterparts. Sabia (2015) found that the longer a man has self-identified as gay, the larger is his corresponding wage gap, supporting direct discrimination as the cause of the gap, although the sexual orientation wage gap has been decreasing over time for men (Cushing-Daniels & Yeung, 2009; Klawitter, 2015).

A wage premium related to marital status is also broadly supported by the wage gap literature. Married men earn more than single men on average, but the male marriage wage premium does not appear to extend to gay men (Zavodny, 2008). Indeed, some research finds exceptions to the general rule of a heterosexual male marriage wage premium. Mueller (2014), for example, found that gay men in long-term relationships and heterosexual men in relationships have the same wages. Aksoy et al. (2018) found partnership status to be important when conditioned on sexual orientation, with partnered lesbians earning more than partnered heterosexual women, partnered gay men earning less than partnered heterosexual men, and no difference in earnings for the unpartnered (lesbians compared to heterosexual women and gay men compared to heterosexual men). The cumulative effect of these results is consistent with the conjecture that the earnings gap is related to the perceived likelihood of becoming a parent.

Competing Theories on Wage Gaps

Direct Discrimination

Direct discrimination is a viable and common explanation for some portion of the persistent wage gaps. Becker (1957) and Arrow (1973) discussed the role and limits of taste-based discrimination. They posited that, in the long run, competition will eliminate discrimination based on employer preferences, as these raise the costs of business. On the other hand, a wage differential based on employee gender or sexual orientation may persist if competition is limited and does not drive out the inefficiency. Invoking consumers’ taste-based discrimination, however, leads to the trivial conclusion that lesbians earn a wage premium and gay men earn a wage penalty simply because the market wants it that way. This is not an explanation; it is a restatement of the stylized facts.

While testing and estimating the impact of each of these pathways is difficult, given the subtle (and often unobservable) nature of discrimination, there is empirical evidence of direct discrimination. Badgett et al. (2009) reported widespread discrimination related to sexual orientation and gender identity (or at least their perception by respondents).

Other studies that found a lesbian wage premium include Klawitter and Flatt (1998), Blandford (2003), Clain and Leppel (2001), Berg and Lien (2002), Black et al. (2003), Cushing-Daniels and Yeung (2009), La Nauze (2015), Laurent and Mihoubi (2012), and Sayers et al. (2017). On the other hand, Badgett (1995) and Sabia (2014, 2015) found no lesbian wage premium.

See, for example, Badgett (1995), Klawitter and Flatt (1998), Blandford (2003), Clain and Leppel (2001), Berg and Lien (2002), Black et al. (2003), Cushing-Daniels and Yeung (2009), Laurent and Mihoubi (2012), and La Nauze (2015). There was variation across the studies in Klawitter’s (2015) analysis, however, with some authors finding no statistical difference between gay and heterosexual men.

2 Some relatively recent studies have found that gay men now earn more than heterosexual men after controlling for other factors (see Clarke & Sevak, 2013; Carpenter & Eppink, 2017).
Further, Sabia (2015) reported that “…longer-term gay identifiers earn wages that are 26.4% lower than their consistently heterosexual-identifying counterparts” (p. 33). Presumably, one’s employment-relevant personality characteristics do not change after one makes their sexual orientation publicly known, so direct discrimination is a plausible explanation of the sexual orientation wage gap.

**Personality Differences, Cultural Norms, and Expectations**

There is ample evidence, described below, that men and women exhibit different personality characteristics and behaviors, on average. Thus, a second viable and common explanation of the gender wage gap is differences by gender in characteristics such as assertiveness, competitiveness, or risk tolerance (whether actual or only perceived by employers). We do not mean to imply that personality differences are universal, immutable, or comply with the stereotypes identified above. All that is necessary for the personality hypothesis to hold is that either personality differences exist currently for whatever reason, or employers believe they exist.

One common explanation for wage gaps is a workplace bias favoring perceived masculinity (Drydakis, 2014). To the extent that people conform to gender stereotypes, or that employers believe the stereotypes, then men will be paid more than women, lesbians will be paid more than heterosexual women, and heterosexual men will be paid more than gay men. Anticipating our later analysis and discussion, we note here that these attitudes and biases would probably not change quickly as a result of adoption laws changing to allow same-sex couples to adopt a child.

Research supports the claim that men’s and women’s personalities differ in characteristics that are relevant to the labor market. For example, men are more assertive than women (Costa et al., 2001; Feingold, 1994) and tend to accept more risk than women (Byrnes et al., 1999; Costa et al., 2001; Croson & Gneezy, 2009; Dohmen et al., 2011). Since occupations with higher wages tend also to have higher wage volatilities, women may be less likely to enter these types of jobs.

Men and women also differ in how and when they negotiate, and the outcomes they experience when they do so. A meta-analysis of 21 published and unpublished studies of gender differences in negotiations finds that men’s outcomes are slightly better than those for women (Stuhlmacher & Walters, 1999).

Men also tend to be more competitive than women (Buser & Yuan, 2019; Buser et al., 2014; Flory et al., 2015; Gneezy et al., 2003; Niederle & Vesterlund, 2007, 2011). This seems to be true, even after controlling for the fact that men tend to be more overconfident than women (see Niederle & Vesterlund, 2011, and the references therein).

Weichselbaumer (2003) explores the masculinity hypothesis for wage differences. The paper tests whether lesbians earn a premium over heterosexual women because of their greater perceived masculinity, and gay men earn less than heterosexual men because they are perceived to be less masculine. To test this, the author created and submitted several different job applications where the applicants differed in their sexual and gender identities, but not in their human capital. That is, the data allowed comparison of feminine heterosexual women, masculine heterosexual women, feminine lesbians, and masculine lesbians. The data showed that lesbians have fewer employment prospects than do heterosexual women but no masculine/feminine difference. Ahmed et al. (2013) performed a similar analysis using artificial resumes. They found gay men were discriminated against when applying for typically male-dominated jobs and lesbians were discriminated against when applying for typically female-dominated jobs. Again anticipating our later analysis and discussion, we note that perceptions of personality differences would probably not change quickly when adoption laws change.

**Labor Force Attachment and the Parenthood Hypothesis**

Differences in labor force attachment may also help explain the various wage gaps. These differences can take many forms, both in daily non-labor force activities and in spans of time spent out of the labor force. The burden of caring for family members, particularly children, is not equally shared between the genders. According to the American Time Use Survey (Hofferth et al., 2018), women spend over 50% more time in household duties (2.3 h) than do men (1.4 h), and twice as much time caring for and helping household...
members (0.6 h vs. 0.3 h). Even in 2019, “On an average day, 22 percent of men reported doing housework—such as cleaning or doing laundry—compared with 50 percent of women. Forty-three percent of men did food preparation or cleanup activities versus 70 percent of women. (Data are from the 2015 survey.)” (U.S. Bureau of Labor Statistics, 2016).

Blau and Kahn (2017) identified multiple pathways which would contribute to the existence of a parenthood penalty that would specifically harm women relative to men. One pathway is that employers may invest fewer resources in training and promoting women, whom they may perceive to be more likely to quit a job to have or raise children. Some women observe the lack of opportunity and leave the firm, possibly to have and raise children, creating a self-fulfilling prophecy.

A second pathway for the parenthood penalty to affect women more than men is that women are still more likely to be the primary caregivers for children in the US, a burden which may limit their career progression. Bertrand (2018) argued that high-paying jobs increasingly demand longer hours and less flexible work schedules. These are precisely the types of jobs that working mothers are less likely to take, as they are still expected societally to be the primary caregiver for children and elderly family members. In particular, Wiswall and Zafar (2018) showed that women are more likely to take jobs with lower salaries in order to receive more job flexibility, more security, and shorter hours (see also Mas & Pallais, 2017). Cushing-Daniels and Yeung (2009) found that a differential selection into full-time work explains significant portions of the lesbian wage premium, lending further credence to the parenthood theory of the wage gap.

Similarly, whether women take more medical leave due to pregnancy, or are believed by employers to take more such leave than their male labor-force competitors, such differences in labor-force participation can lead to persistent differences between men and women, even for those women who never take maternity leave (Lai & Masters, 2005; Yip & Wong, 2014).

 Interruptions in employment dramatically decrease earnings, so the persistent differences in time spent in parenting activities and different rates of exiting the labor force to care for children by gender significantly contribute to the gender wage gap. Miller (2011) estimated that, for each year that motherhood is delayed, a woman’s annual earnings increase by 9%, her wages increase by 3%, and her hours worked increase by 6%. Lundberg and Rose (2000) estimated that, after a first child, the average mother worked 45% fewer hours, whereas the hours worked by fathers did not change. In households where a mother temporarily left the workforce completely, the father’s hours and wages rose to compensate.

 Mothers tend to earn less income than women who have never been mothers, and the gap widens over the lifetimes of the women. Neumeier et al. (2018) examined the income data for a group of women over the course of 60 years. Specifically, they compared the income histories of women who have never had children with the income histories of women who have had children. They found the size of this income difference was largely correlated with differences in labor force attachment and participation. Having children makes it more difficult for a woman to gain work experience; this difficulty also translates into a shallower future income trajectory. That is, someone directly earns less current income if they currently work less. However, they also indirectly earn less future income because of having worked less (see also Korenman & Neumark, 1992; Lundberg & Rose, 2000; Webber, 2016). Bertrand et al. (2010), for example, found that 10 years after graduating with a University of Chicago MBA, the female graduates earned less, worked 8 h fewer per week, and had left and returned to the labor force, thereby earning fewer total years of experience than the male graduates.

In general, in opposite-sex couples, husbands and wives have similar labor-market characteristics and wages up to childbirth. However, after the birth of their first child, wives were more likely than their husbands to exit the labor force, work fewer hours, and earn lower wages; it is also at this point that their wage trajectories begin to diverge (Angelov et al., 2016; Kleven et al., 2019).

Finally, division of labor and specialization can help explain the gender and sexual orientation wage gaps (Aksoy et al., 2018). Aksoy et al. find that, in opposite-sex relationships, men are more likely to specialize in market production, and women in non-market production, and this separation of duties becomes reinforced by the gender wage gap. Comparing lesbians to heterosexual women or gay men to heterosexual men yielded a different conclusion: women in relationships with men tend to specialize in non-market production, whereas women in relationships with other women cannot, on average, specialize in non-market production. Similarly, men in relationships with women will tend to be associated with specialization in market production, whereas men in relationships with other men cannot, on average, specialize in market production from the external perspective of an employer. Thus, specialization can explain why lesbians earn more than heterosexual women and gay men earn less than heterosexual men.

Data and Methods

We distinguished between the parenthood hypothesis and competing explanations using a differences-in-differences model, taking advantage of an exogenous change in adoption
laws across U.S. states over time. We used data combined from two sources. Worker-level data came from the general social survey (GSS) and state-level adoption data came from the movement advancement project.

The GSS is a nationally representative survey of non-institutionalized adults in the United States conducted by NORC at the University of Chicago (2021). The GSS contains data on thousands of demographic, economic, and opinion-based variables since 1972. The data consist of repeated cross-sections of individuals.

Using GSS data for 1988–2014, each respondent’s sexual orientation was categorized, using the reported sex (male or female) and the sex of the respondent’s sex partner(s) in the last year (sexsex in the GSS). We included only those respondents who had at least one sexual partner in the previous year and knew the sex of all their partners. Thus, we used a definition of sexual orientation based on reported behaviors. Very few respondents were classified as bisexual using this definition, precluding meaningful statistical analysis, so we dropped these respondents from our final data set. The available remaining categories of sexual orientation were heterosexual male, gay male, heterosexual female, and lesbian.

Additional demographic controls included the age of the respondent in years (age), and self-identified race (race: Black, White, or other). The number of children the respondent has reported as having (titled childs in the GSS data set) is an important variable for our analysis. The GSS does not distinguish between biological and adopted children, nor between living and deceased children; rather, the GSS merely asks, “How many children have you had?” We also included controls for the region of the country, educational attainment (degree), whether the respondent was self-employed (works for self), work status (full-time versus part-time, omitting respondents who were retired, students, unemployed, or not in the labor force), and the prestige of their current occupation (prestige). We used the respondent’s state of residence (restricted use data provided by NORC) to determine whether adoption by same-sex couples was legal in the year in which the respondent participated in the survey. All observations were weighted by the inverse of their sampling probabilities (using the GSS variable wtsall).

The mean differences across the four subgroups of the sample are provided in Table 1. Both measures of earnings, income and hourly wage, fit the general result in the literature that heterosexual men earn more than gay men, and lesbians earn more than heterosexual women. A larger fraction of

### Table 1 Summary statistics

|                | Males      | Females    |
|----------------|------------|------------|
|                | Heterosexual | Gay       | Heterosexual | Lesbian |
| Income         | 41,125     | 37,953     | 26,046       | 32,897   |
| (29,749)       | (28,999)   | (20,632)   | (23,874)     |
| Hourly Wage    | 18.37      | 16.87      | 13.82        | 16.22    |
| (13.13)        | (11.85)    | (10.79)    | (12.23)      |
| Age            | 40.24      | 39.88      | 39.33        | 38.33    |
| (12.36)        | (11.56)    | (11.54)    | (10.74)      |
| Year           | 2000.1     | 2000.8     | 2001.1       | 2002.1   |
| (7.82)         | (7.06)     | (7.79)     | (6.95)       |
| Children       | 1.66       | 0.73       | 1.72         | 0.92     |
| (1.52)         | (1.36)     | (1.43)     | (1.28)       |
| Work for self  | 0.14       | 0.16       | 0.09         | 0.08     |
| (0.35)         | (0.37)     | (0.29)     | (0.27)       |
| Work status    |            |            |              |          |
| Full time      | 0.91       | 0.91       | 0.78         | 0.82     |
| (0.29)         | (0.28)     | (0.42)     | (0.39)       |
| Part time      | 0.09       | 0.09       | 0.22         | 0.18     |
| (0.29)         | (0.28)     | (0.42)     | (0.39)       |
| Prestige       | 47.57      | 48.73      | 47.77        | 47.45    |
| (13.22)        | (13.67)    | (13.01)    | (13.95)      |
| Race           |            |            |              |          |
| White          | 0.82       | 0.79       | 0.80         | 0.85     |
| (0.38)         | (0.41)     | (0.40)     | (0.35)       |
| Black          | 0.10       | 0.13       | 0.13         | 0.09     |
| (0.30)         | (0.34)     | (0.34)     | (0.28)       |
| Other          | 0.08       | 0.08       | 0.07         | 0.06     |
| (0.27)         | (0.27)     | (0.25)     | (0.24)       |
| Education      |            |            |              |          |
| High School    | 0.53       | 0.42       | 0.55         | 0.48     |
| (0.50)         | (0.50)     | (0.50)     | (0.50)       |
| Junior College | 0.08       | 0.08       | 0.10         | 0.10     |
| (0.27)         | (0.27)     | (0.30)     | (0.30)       |
| Bachelors      | 0.19       | 0.26       | 0.20         | 0.20     |
| (0.40)         | (0.44)     | (0.40)     | (0.40)       |
| Graduate       | 0.10       | 0.16       | 0.09         | 0.14     |
| (0.30)         | (0.37)     | (0.29)     | (0.35)       |
| After          | 0.11       | 0.08       | 0.11         | 0.13     |
| (0.31)         | (0.28)     | (0.31)     | (0.34)       |
| Observations   | 6974       | 228        | 6491         | 176      |

Data are from the 1988–2014 waves of the restricted-use GSS data. Sample is restricted to respondents with full data appearing in the full regression. Observations are weighted using the GSS weights provided in variable wtsall. Standard deviations are listed in parentheses below the means.

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6 These income statistics were calculated using the GSS variable conrinc and hours worked. We further restricted our data set by removing respondents reporting the top and bottom 1% of incomes.
of the men than the women had full-time jobs, with women twice as likely to be employed part time. This difference can potentially explain much of the gap in annual income; less so with hourly wage. Heterosexual people have more children, on average, than gay male or lesbian respondents, with lesbians tending to have more children than gay men. The women in the sample are slightly younger than the men, with lesbians slightly younger than heterosexual women. Gay male and lesbian respondents were more educated than their heterosexual counterparts, on average. Finally, all groups have about the same average levels of prestige in their occupations.

Our second source of data contains state-specific regulations governing adoption laws. These data were provided by research staff at the movement advancement project (MAP; Naomi Goldberg, personal communication). MAP is an advocacy group “that provides rigorous research, insight and analysis that help speed equality for lesbian, gay, bisexual and transgender (LGBT) people” (Movement Advancement Project, 2017). The data identify the year in which adoption by same-sex couples became legal in each state (Table 2). This is not to say that lesbians and gay men could not have children, or could not adopt as individuals in some states. That said, single parents faced higher barriers to adopt in some states, a barrier (intentional or unintentional) making it more difficult for lesbians and gay men to adopt because they may not have been allowed to marry in those states. We created a dummy variable equal to zero for respondents in state-year combinations where same-sex couples could not legally adopt, and equal to one in the state-year combinations where they could (after). The variable also interacted with the gender-specific sexual orientation variables (notated as orientation below, equal to one for gay males and lesbians and zero for heterosexuals) to permit estimation of changes in the sexual orientation wage gap for men and women separately.

Table 2 Year of legalized joint adoption by same-sex couples

| State        | Year | State        | Year | State        | Year |
|--------------|------|--------------|------|--------------|------|
| Alabama      | 2015 | Kentucky     | 2015 | North Dakota | 2015 |
| Alaska       | 2014 | Louisiana    | 2015 | Ohio         | 2015 |
| Arizona      | 2014 | Maine        | 2007 | Oklahoma     | 2014 |
| Arkansas     | 2015 | Maryland     | 2012 | Oregon       | 2007 |
| California   | 2013 | Massachusetts| 1993 | Pennsylvania | 2014 |
| Colorado     | 2013 | Michigan     | 2015 | Rhode Island | 2011 |
| Connecticut  | 2005 | Minnesota    | 2013 | South Carolina| 2014 |
| Delaware     | 2011 | Mississippi  | 2016 | South Dakota | 2015 |
| District of Columbia | 2010 | Missouri    | 2014 | Tennessee   | 2015 |
| Florida      | 2015 | Montana      | 2014 | Texas       | 2015 |
| Georgia      | 2015 | Nebraska     | 2015 | Utah        | 2014 |
| Hawaii       | 2011 | Nevada       | 2009 | Vermont     | 1993 |
| Idaho        | 2014 | New Hampshire| 2008 | Virginia    | 2014 |
| Illinois     | 2011 | New Jersey   | 1997 | Washington  | 2012 |
| Indiana      | 2006 | New Mexico   | 2013 | West Virginia| 2014 |
| Iowa         | 2009 | New York     | 2011 | Wisconsin   | 2014 |
| Kansas       | 2014 | North Carolina| 2014 | Wyoming     | 2014 |

Data were provided by Naomi Goldberg (personal communication, 2017), staff member of the movement advancement project.

There is a reasonable concern about comparing the people who choose to work pre-legalization to those who choose to work post-legalization. By excluding respondents with zero hours of work, we are biasing our analysis against finding our results. Namely, legalization of same-sex adoption would impact lesbians and gay men more than heterosexual women and men, primarily motivating lesbians and gay men to exit the labor force. This disproportionate imposition of zeros (exiting the labor force) would disproportionately lower the average income for lesbians and gay men, without affecting the average income for heterosexual women and men. In other words, including the zeros would bias the results in a direction that favors our hypothesis: Having children lowers earnings (not just total household income). Because we did not want to bias the results in our own favor, we did not include zero incomes.
legalization variable which was at the state-year level. Here, earnings were measured as the natural log of the respondent’s inflation-adjusted personal income, the natural log of their inflation-adjusted hourly wage, or the log of hours worked. Coefficient measures the overall change in incomes after the various dates of legalization of joint adoption for same-sex partners, measures the wage gap between heterosexual respondents and either gay male or lesbian respondents in each sub-sample, and measures the change in the wage gap after legalization. Our differences-in-differences approach allowed us to directly test (via) how the pay gap between heterosexual women and lesbians changed after the legalization of same-sex adoption. We included as regressors a vector of dummy variables for year, to control for country-wide changes in the economy.

Additionally, we added a vector of individual-level controls, $X$, which included a third-degree polynomial of each respondent’s age, number of children, self-employment and full-time versus part-time work status, urban residency, geographic region (to control for time-invariant, unobserved heterogeneity based on region of the country), educational attainment indicators, race, and occupational prestige. Given any association between labor market outcomes and social acceptability of diversity in sexual orientation, our results would have been biased if we were to ignore the aggregate time trend. Therefore, we also included a linear time trend to control for any aggregate, unobserved, time-based relationships. Unfortunately, our data set was not large enough to control for interaction effects between these individual-level variables and each state, which would require substantially more coefficients (50 state variables times the number of education categories). In other words, we had to assume the effect of education is the same in each state, with observed differences in education levels explaining a portion of the income variation.

The parenthood hypothesis predicted an increasing sexual orientation wage gap for men, and a decreasing gap for women. With improved access to adoption and thus the burden of parenthood, wages for gay men and lesbians would drop. Given that earnings for gay men are currently lower than those for heterosexual men, parenthood would increase this pay gap. However, given that lesbians earn more than heterosexual women on average, this would result in a decreased, or even inverted, wage premium for lesbians. Parenthood would have a similar burden on gay men and lesbians, but this would manifest itself in different changes in the respective wage gaps. This is illustrated in Fig. 1.

Limitations

Before we proceed, we must acknowledge several weaknesses with our data and analysis that are common in research in this area. First, defining and identifying sexual orientation through a survey is challenging. Issues of identity, orientation, behavior, and relationships are complicated, and all imply different measures and definitions for researchers who have a limited ability to observe those being researched (Sabia, 2014). For example, when measuring the sexual orientation wage gap for men, it is not obvious whether a man should be classified as gay if he is attracted to men but has never had a sexual relationship with a man. Similarly, it is unclear whose beliefs about
a worker’s sexual orientation should be included in the statistical analysis. For example, how should a researcher elicit and control for a respondent’s sexual orientation if the respondent has not revealed their orientation to employers? When analyzing employer data, how should a researcher elicit and control for an employer’s beliefs about a worker’s sexual orientation if the employer does not know the worker’s orientation with certainty? Similarly, it is unclear whether a researcher should differently categorize a woman who is attracted to women and has a sexual relationship with another woman, but whose employer is not aware of her history or sexual orientation. In each case, these imprecise or insufficient survey questions will tend to bias our estimated coefficients toward zero, and thus against finding a significant relationship, should one exist. Moreover, sexual orientation may change over time, both in status and in visibility to employers (Sabia, 2014).

Some researchers have explored different definitions of sexual orientation. Aksoy et al. (2018) examined both individual and partnership orientation data, looking at the impact of being partnered versus single. Sabia (2014) examined multiple measures of orientation, separating attraction from behavior. He summarized the percentages of respondents who identified as gay male, lesbian, or bisexual across multiple surveys, and found that between 2 and 7% of men identified as gay or bisexual, while the results are more variable for women, with some studies finding between 2 and 4% of women identified as lesbian or bisexual, with the latter number as large as 20% in some surveys, depending on the wording of the question.

Endogenous non-response may also introduce bias into our estimates. While the research community has worked to make survey instruments and human subjects training more sensitive to issues related to identity (for an example see the CITI training module “Gender and Sexuality Diversity (GSD) in Human Research,” 2018), respondents may choose to not disclose their personal information to researchers (Badgett, 1995). This hesitance or refusal to disclose may occur for a variety of reasons, including a desire for privacy, concerns about the confidentiality of the data, or uncertainty on how to respond to imprecise or insufficient questions. Thus, those respondents who choose to disclose their private information may not be representative of their demographic subgroup.

For there to be labor force discrimination based on a worker’s sexual orientation, the employer and/or coworkers must be aware of it. To the extent that employees are able (and prefer) to keep their sexual preferences hidden from their employer, our estimates of wage differentials will be further biased toward zero. For example, a lesbian with an employer who holds a bias against lesbians may successfully hide her orientation from her employer, thus potentially avoiding the existing bias by that employer and (inadvertently) causing researchers to underestimate the level of labor force discrimination.

Plug et al. (2014) noted that lesbian, gay, and bisexual workers may choose occupations and employers which are more accepting of diverse sexual orientations, which would again bias the measured amount of discrimination in the labor market toward zero. Similarly, these workers may migrate to more tolerant urban environments (Bryson, 2017). If these workers prefer industries, occupations, employers, or geographic regions that are more progressive, the increased supply of workers in those areas will tend to hold down wages. Thus, endogenous sorting is also a potential source of bias in coefficient estimates.

Finally, as described by Blau and Kahn (2017), the gender wage gap may overstate or underestimate the amount of discrimination in the labor market. If men have systematically higher levels of productivity due to unmeasured characteristics such as physical strength, or typically work in more dangerous working conditions, then the wage gap between men and women would overstate the amount of discrimination in the labor market. On the other hand, the gap may underestimate the level of discrimination. For example, this may occur if women have systematically higher levels of productivity than men, due to unmeasured characteristics such as interpersonal communication skills. Women may negotiate differently for raises and promotions than men and/or have their efforts perceived differently than those performed by men, and thus get different results when they do negotiate. Similarly, if women face discrimination that encourages them to enter particular occupations or industries with lower wages, then the gap would also underestimate the amount of discrimination against women.

One common challenge for analyses of public policy is the possibility that some other set of policies caused the observed changes in the variables of interest. We control for U.S.-wide policy changes by including fixed effects for years. State-level policies are more difficult to detect, analyze, and measure, but the staggered legalization of same-sex adoption over states and years makes it unlikely any were sufficiently aligned to affect our analysis. It is certainly possible that at least one state changed wage-related policies in the same year they legalized same-sex adoption, and that this policy also impacted the relative wage gaps by sexual orientation and gender in the directions that we predict. However, state-level wage and marriage policies are not always aligned, even within a state. Since each state legalized same-sex adoption at a different time, we believe any such effects would be minimal.

Such deficiencies in the data affect all research in this field, including this paper. We cannot escape them, but we do acknowledge them as affecting our work and as an important area for additional research and continued improvement in the literature and in survey construction.
Results

We estimated the impact of the legalization of joint adoption by same-sex couples for each gender separately and allowing for different effects by sexual orientation. We estimated the regression equation given in the previous section, with results provided in Tables 3 and 4. These regressions also used the previously listed controls. Research in this area (Carpenter & Eppink, 2017; Clarke & Sevak, 2013; Cushing-Daniels & Yeung, 2009; Klawitter, 2015) has indicated that the sexual orientation wage gap may be shrinking over time, so we included the results of adding data over the time of the data set, including the data from 1988 through 2008 in columns 1 and 3 and the data from 1998 through 2014 in columns 2 and 4.

Table 3 reports the estimates for the women-only subsample, which shows that, before legalization, lesbians earned a 19–24% wage premium over heterosexual women. Legalizing same-sex adoption increases access for lesbians (and lesbian couples) to adopt children. Some employers may anticipate that lesbian employees (or potential employees) will thus require time off from work to care for children at increased rates, while legalization would not change the demands placed on heterosexual women. Thus, the parenthood hypothesis suggests that the (perceived) increased costs of (potential) parenthood faced by lesbians would narrow the pay gap between lesbians and heterosexual women, possibly even inverting the gap. In the context of Table 3, this is equivalent to testing whether the difference-in-difference in earnings decreases, that is, whether the coefficient on orientation*after < 0. Table 3 reports the evidence for this hypothesis for both yearly income (Panel 1) and hourly wages (Panel 2). When looking at yearly salaries, this was found to be the case. This result is robust to the time periods estimated, and whether the actual number of children is included as a regressor. Moreover, this shrinking of the gap was of such magnitude that the wage gap was inverted (summing the coefficients on orientation and orientation*after) so that lesbians earned marginally less than heterosexual women after legalization.

The magnitude of the coefficient estimates in personal income (Table 3, Panel 1) were surprisingly large, which indicates there may be multiple effects being captured, such as changes in both hourly wages and hours worked. For hourly wages, the lesbian wage premium was lower, and even inverted, for the earlier years (1988–2008), but was not statistically significantly affected when adding data after 2008. To summarize, the parenthood hypothesis predicts that the increased costs of parenthood faced by lesbians will result in a narrowing of the pay gap between lesbians and heterosexual women. Using a one-tailed test, we found statistical evidence in support of a narrowing earnings gap, consistent with the parenthood hypothesis for women. The magnitudes of the coefficients on hourly wages were more moderate than those for income, but were still larger than expected.

Panel 3 of Table 3 contains estimates for the log of hours worked, and provides some insight into the large coefficient values in earnings gap estimates. The data show that lesbians reported working 10% more hours (about 4 h more) per week than heterosexual women before legalization. After legalization, however, lesbians worked approximately 10% less than heterosexual women (4 h less) per week on average (summing the coefficients on orientation and orientation*after: 0.10–0.20).

Table 4 displays the results for the male sub-sample. As with the analysis for women, legalizing same-sex adoption increases the probability that gay men (and gay male couples) will adopt. Again, some employers may anticipate that gay male employees (or potential employees) will thus require additional time from work to care for children at increased rates, while not changing the demands placed on heterosexual men. Thus, the parenthood hypothesis suggests that the pay gap between gay and heterosexual men will increase after legalization of same-sex adoption (see Fig. 1).

In the context of Table 4, this is equivalent to testing whether the coefficient on orientation*after > 0. Table 4 shows no statistically significant changes in the difference-in-difference for gay men versus heterosexual men in any time frame or controls for the presence of children in the household. Gay men still earned approximately 15% less than their heterosexual male counterparts (whether measured as income or hourly wages), but this gap did not change when adoption laws changed. That is, we found no statistical evidence in support of the parenthood hypothesis for men. The reason for this, we suspect, is revealed in Tables 5 and 6. These tables show that gay men did not respond to the legal change by adopting more children. In fact, rates of adoption have slowed for gay men, moderating the impact of discrimination related to the parenthood hypothesis.

Panel 3 of Table 4 contains estimates for the log of hours worked for men, which show an unexpected result. Gay men had lower income and hourly wages than heterosexual men before legalization, and this did not substantially change after legalization. Hours worked, however, went from not statistically different between the groups to a gap of 4.4–5.9 h per week less for gay men. This is true in the raw averages reported in Table 5 as well as in the regression results of Table 4.

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8 As mentioned in the discussion of the data, the survey data available from the GSS contained few bisexual respondents. Coefficient estimates for that group were not robust to small changes in the model used, and were thus omitted from our analyses. The coefficient estimates for the remaining groups did not change qualitatively based on including or omitting bisexual respondents from the analysis.
Table 3  OLS results, female sub-sample

Panel 1: Log of personal income

|                  | (1) 1988–2008 | (2) 1988–2014 | (3) 1988–2008 | (4) 1988–2014 |
|------------------|----------------|---------------|---------------|---------------|
| Lesbian          | 0.193**        | 0.202***      | 0.235***      | 0.244***      |
|                  | (3.071)        | (3.557)       | (3.769)       | (4.363)       |
| After            | 0.118*         | −0.034        | 0.124*        | −0.036        |
|                  | (2.196)        | (−0.938)      | (2.218)       | (−0.970)      |
| Lesbian × after  | −0.643***      | −0.240        | −0.665**      | −0.252†       |
|                  | (−3.071)       | (−1.598)      | (−3.227)      | (−1.726)      |
| Control: children| −0.065***      | −0.060***     | −0.065***     | −0.060***     |
|                  | (−6.603)       | (−6.992)      | (−6.992)      | (−6.992)      |
| Controls: X      | Yes            | Yes           | Yes           | Yes           |
| Observations     | 5493           | 6667          | 5493          | 6667          |
| Adj. R²          | 0.418          | 0.426         | 0.411         | 0.420         |

Panel 2: Log of hourly wage

|                  | (1) 1988–2008 | (2) 1988–2014 | (3) 1988–2008 | (4) 1988–2014 |
|------------------|----------------|---------------|---------------|---------------|
| Lesbian          | 0.116*         | 0.121*        | 0.154**       | 0.156**       |
|                  | (2.063)        | (2.344)       | (2.710)       | (3.030)       |
| After            | 0.086          | −0.037        | 0.091         | −0.039        |
|                  | (1.552)        | (−1.016)      | (1.626)       | (−1.045)      |
| Lesbian × after  | −0.478**       | −0.058        | −0.498**      | −0.068        |
|                  | (−2.852)       | (−0.405)      | (−3.044)      | (−0.484)      |
| Control: children| −0.059***      | −0.051***     | −0.059***     | −0.051***     |
|                  | (−5.944)       | (−5.875)      | (−5.875)      | (−5.875)      |
| Controls: X      | Yes            | Yes           | Yes           | Yes           |
| Observations     | 5493           | 6667          | 5493          | 6667          |
| Adj. R²          | 0.261          | 0.275         | 0.254         | 0.269         |

Panel 3: Log of hours worked

|                  | (1) 1988–2008 | (2) 1988–2014 | (3) 1988–2008 | (4) 1988–2014 |
|------------------|----------------|---------------|---------------|---------------|
| Lesbian          | 0.098**        | 0.097**       | 0.101**       | 0.102***      |
|                  | (2.923)        | (3.270)       | (3.033)       | (3.447)       |
| After            | 0.029          | 0.004         | 0.029         | 0.004         |
|                  | (1.149)        | (0.301)       | (1.167)       | (0.294)       |
| Lesbian × after  | −0.194*        | −0.200**      | −0.196*       | −0.202***     |
|                  | (−2.005)       | (−3.257)      | (−2.020)      | (−3.294)      |
| Control: Children| −0.006†        | −0.007*       | −0.006†       | −0.007*       |
|                  | (−1.651)       | (−2.430)      | (−1.651)      | (−2.430)      |
| Controls: X      | Yes            | Yes           | Yes           | Yes           |
| Observations     | 5461           | 6626          | 5461          | 6626          |
| Adj. R²          | 0.520          | 0.514         | 0.519         | 0.513         |

All regressions use the control vector X, which includes the year of survey, age (polynomial), self-employment and work status, urban residency, geographic region, educational attainment, race, and occupational prestige.

Observations are weighted using the GSS weights provided in variable wtsall.

t-stats are reported in parentheses below their associated coefficients. They are calculated using robust standard errors.

†Indicates p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001 using a two-sided hypothesis test.
### Table 4: OLS results, male sub-sample

#### Panel 1: Log of personal income

|          | (1) 1988–2008 | (2) 1988–2014 | (3) 1988–2008 | (4) 1988–2014 |
|----------|---------------|---------------|---------------|---------------|
| Gay      | −0.150**      | −0.131**      | −0.165***     | −0.146**      |
|          | (−3.255)      | (−2.938)      | (−3.565)      | (−3.263)      |
| After    | −0.019        | −0.026        | −0.017        | −0.025        |
|          | (−0.354)      | (−0.732)      | (−0.312)      | (−0.716)      |
| Gay × after | −0.080      | 0.024         | −0.101        | 0.014         |
|          | (−0.487)      | (0.150)       | (−0.616)      | (0.085)       |
| Control: children | 0.021**      | 0.019**       |               |               |
|          | (2.826)       | (2.876)       |               |               |
| Controls: X | Yes         | Yes           | Yes           | Yes           |
| Observations | 5970        | 7202          | 5970          | 7202          |
| Adj. R²  | 0.427         | 0.432         | 0.426         | 0.431         |

#### Panel 2: Log of hourly wage

|          | (1) 1988–2008 | (2) 1988–2014 | (3) 1988–2008 | (4) 1988–2014 |
|----------|---------------|---------------|---------------|---------------|
| Gay      | −0.160**      | −0.153**      | −0.167**      | −0.161**      |
|          | (−3.119)      | (−3.083)      | (−3.262)      | (−3.253)      |
| After    | −0.033        | −0.017        | −0.032        | −0.017        |
|          | (−0.597)      | (−0.484)      | (−0.577)      | (−0.475)      |
| Gay × after | 0.009        | 0.127         | −0.001        | 0.122         |
|          | (0.071)       | (0.865)       | (−0.009)      | (0.821)       |
| Control: children | 0.010        | 0.011         |               |               |
|          | (1.356)       | (1.604)       |               |               |
| Controls: X | Yes         | Yes           | Yes           | Yes           |
| Observations | 5970        | 7202          | 5970          | 7202          |
| Adj. R²  | 0.293         | 0.304         | 0.293         | 0.304         |

#### Panel 3: Log of hours worked

|          | (1) 1988–2008 | (2) 1988–2014 | (3) 1988–2008 | (4) 1988–2014 |
|----------|---------------|---------------|---------------|---------------|
| Gay      | 0.004         | 0.015         | −0.004        | 0.008         |
|          | (0.167)       | (0.690)       | (−0.184)      | (0.386)       |
| After    | 0.013         | −0.007        | 0.014         | −0.007        |
|          | (0.654)       | (−0.531)      | (0.711)       | (−0.513)      |
| Gay × after | −0.077      | −0.103*       | −0.089        | −0.107*       |
|          | (−1.316)      | (−2.001)      | (−1.509)      | (−2.086)      |
| Control: children | 0.011***  | 0.009**       |               |               |
|          | (3.710)       | (3.088)       |               |               |
| Controls: X | Yes         | Yes           | Yes           | Yes           |
| Observations | 5954        | 7184          | 5954          | 7184          |
| Adj. R²  | 0.394         | 0.392         | 0.392         | 0.391         |

All regressions use the control vector X, which includes the year of survey, age (polynomial), self-employment and work status, urban residency, geographic region, educational attainment, race, and occupational prestige.

Observations are weighted using the GSS weights provided in variable wtssall.

$t$-stats are reported in parentheses below their associated coefficients. They are calculated using robust standard errors.

*Indicates p < 0.05; **p < 0.01; ***p < 0.001 using a two-sided hypothesis test.
The results in Tables 3 and 4 also align with the well-documented finding from the literature that women’s earnings tend to be lower when they have children, while men’s earnings tend to be higher (see, for example, Budig & England, 2001 and Gough & Noonan, 2013). As can be seen in Table 3, women who have children had lower income, both from lower hourly wages and from working fewer hours. Table 4 shows that men did not have higher hourly wages on average, but did work statistically significantly more hours. In summary, lesbians (but not gay men) have responded to the legal change in adoption laws by adopting 10% more children, and concurrently working 5 fewer hours per week on average. We note that the micro-level data available did not allow us to investigate these phenomena further, and, as such, we only note the correlational nature of the relationship. Thus, the parenthood penalty that previously applied primarily to heterosexual women now increasingly also applies to lesbians.

### Robustness

Regression results are often fragile to seemingly small differences in model specification. To illustrate the robustness of our results, we re-estimated our regressions using four new frames of reference: leading the date of legalization forward by 1 year and by 2 years, and lagging it backwards by 1 year and by 2 years. This “placebo analysis” allows us to verify that our results are not dependent on one particular year’s observations. Thus, we re-estimated four different versions of Tables 3 and 4. The coefficient estimates differed mostly in the second or third decimals, and statistical significance was almost exactly identical as well. Therefore, we feel confident that our estimates do not depend on random fluctuations in the particular year of legalization.  

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**Table 5** Average hours of work per week

| Panel 1: Female sub-sample | Before legal change | After legal change |
|---------------------------|---------------------|--------------------|
| Heterosexual              | 38.50               | 38.86              |
| Lesbian                   | 43.10               | 36.50              |

| Panel 2: Male sub-sample  | Before legal change | After legal change |
|---------------------------|---------------------|--------------------|
| Heterosexual              | 45.89               | 45.95              |
| Gay                       | 46.20               | 41.69              |

Numbers are simple averages, weighted by GSS variable wtssall

**Table 6** Average number of children

| Panel 1: Female sub-sample | Before legal change | After legal change |
|---------------------------|---------------------|--------------------|
| Heterosexual              | 1.72                | 1.65               |
| Lesbian                   | 0.91                | 1.04               |

| Panel 2: Male sub-sample  | Before legal change | After legal change |
|---------------------------|---------------------|--------------------|
| Heterosexual              | 1.66                | 1.62               |
| Gay                       | 0.77                | 0.37               |

Numbers are simple averages, weighted by GSS variable wtssall

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9 These results were provided to the referees and are available from the authors. Please contact the corresponding author for copies of these additional tables.
It may be argued that legalizations are anticipated, so that the economic behaviors change before actual implementation. These ex-ante responses are entirely possible, but they only serve to strengthen our conclusions. If changes in income and hours worked predate the legal change, then it will be only more difficult to identify any differences pre- vs post-legalization. Incomes would be smoothed over the date of legal change. In light of this, the fact that incomes and hours still differ is all the more impressive. Moreover, the “placebo analysis” mentioned in the previous paragraph accounts for the possibility that behavior changes up to 2 years before formal legal changes, and we report no appreciable differences in coefficient estimates or statistical significance in these placebo results.

Modeling Assumptions

The difference-in-difference approach relies on a parallel trends assumption. In the present case, this assumption would be interpreted as parallel slopes in the trend of the ratio of earnings (lesbians versus heterosexual women, or gay men versus heterosexual men) between states without legalized same-sex adoption and states with legalized same-sex adoption. Given the multiple treatment times, the appropriate test for a full panel of data would be that provided in Goodman-Bacon (2018, 2019). The GSS, however, does not create a full panel of data. For example, there are some years where there are no respondents with data for all our variables of interest from low-population states such as Alaska. Similarly, in some years there were observations in a state but all of the respondents were classified as heterosexual. As a result of these gaps in the data, we do not have a full panel and thus are unable to implement the Goodman-Bacon test.

Instead, we provide a graphical representation of the pre- and post-treatment trends in Fig. 2, with the year of legalization normalized to year zero for all states. This figure contains the ratio of average earnings for lesbians versus heterosexual women for both states that legalized same-sex adoption 2014 or earlier and those that did not legalize same-sex adoption until the United States legalized same-sex adoption at the federal level in 2015 through the Supreme Court ruling Obergefell v. Hodges (2015).

As shown in Fig. 2, the data suggest trends consistent with parallel trends. Note the figure represents only one set of comparisons. As articulated succinctly in Goodman-Bacon (2018, 2019), there is neither a single pre-treatment period nor a fixed-size control group in the full analysis. For example, when Iowa made the policy change in 2007, Maine and Louisiana had not. When Maine then made the policy change in 2009, Louisiana was still in the “untreated” group. This staggering of policy changes prevents a single simple analysis of the parallel trends assumption. Figure 2 suggests that the lesbian to heterosexual women’s wage ratio exhibited the same (i.e., parallel) trend in states that legalized previous to Obergefell v. Hodges as the ones that legalized as a result of Obergefell v. Hodges. This wage ratio dips significantly for several years after legalization, implying that lesbians suffered a wage penalty relative to their heterosexual female counterparts as a result of increased likelihood of
motherhood. This wage penalty eventually disappears within a decade of legalization.

**Discussion and Conclusion**

Several different explanations have been offered by the literature to explain gaps in income and wages based on gender and sexual orientation, such as direct discrimination, personality differences, and the parenthood hypothesis. We leveraged a natural quasi-experiment using exogenous state-level changes in laws allowing same-sex couples to adopt children to evaluate the relevance of the parenthood hypothesis. Allowing same-sex couples to legally adopt children in a state does not change personality differences or employers’ discriminatory predilections in a time frame relevant to the current research. Thus, these explanatory factors are held constant throughout the natural quasi-experiment.

Using a differences-in-differences approach, the data show that policy changes aimed at securing access to adoption rights for same-sex couples has had the unintended effect of decreasing or inverting the wage gap between lesbians and heterosexual women. This policy change did not affect the gap between gay men and heterosexual men. That is, allowing same-sex couples to adopt may increase the perceived likelihood that one of the partners will leave or be less attached to the labor force for women but not for men.

These results led to two observations. First, a smaller effect on male pay differentials than on female pay differentials may be the result of an “exposure” consideration, namely, lesbians are more likely to have children than are gay men, and so employers may perceive a larger impact on women than on men. As reported in Table 1, lesbians have, on average, about 26% (0.73 vs. 0.92) more children than do gay men. Gay men may have decreased their rates of adoption post legalization (Table 6). Therefore, it should not be surprising that legalizing adoption has had a larger effect on women, and a smaller, even statistically insignificant, effect on male pay differentials.

These results partially support the parenthood hypothesis, and suggest that it may more appropriately be called the “motherhood hypothesis;” an increased likelihood of parenthood has an adverse impact on the earnings of women but not of men. This change for women can be seen in a decrease in income (both hourly pay and hours worked) for lesbians that did not affect heterosexual women in the same way, and was concurrent with the legalization of same-sex adoption. The change in hours worked occurred similarly for gay men, but without any net effect on their income or hourly pay differentials.

One potential objection to these results is that the legalization of same-sex adoption may have been the result of a pre-existing or concurrent change in attitudes toward sexual orientation. If that were the case, the wage gap would close, not widen, between gay men and heterosexual men and increase (or not decrease) in favor of lesbians over heterosexual women; we do not see such a change. Instead, we see the gap decrease between lesbian and heterosexual women.

A second potential objection is in the change in results based on time (comparing the period before 2008 against the entire time period). Such a change could be the result of employers rationally anticipating the eventual legalization of same-sex adoption and incorporating this into their hiring and advancement policies. Any long-term social trends regarding equality and tolerance can cause changes in both wages and child-rearing. The same could be said for advances in fertility treatments. However, long-term trends such as these do not exhibit the type of discontinuity which our difference-in-difference estimator finds. For such long-term factors to confound our findings, these long-term trends would have to follow the same geographic and temporal sequence as did the legalization of same-sex marriage. We find this hypothesis unlikely, but we leave this for future research.

The gender wage gap has been the subject of scores of studies, with many competing explanations for this persistent phenomenon. Until now, we have been unable to distinguish empirically between hypotheses which emphasize gender-based personality differences, direct discrimination, and the effects of parenthood. By pivoting to a related problem—the sexual orientation wage gap—we are able to cast new light on an old problem. The legal changes occurring largely in the 2000s have allowed same-sex couples to adopt children. It does nothing to existing or perceived personality differences, or to employers’ discriminatory beliefs. It does, however, change the likelihood that gay males or lesbians will become parents through adoption, with all the pleasures and burdens children entail. We find that, consistent with the parenthood hypothesis, lesbians’ wage premium over heterosexual women has shrunk and may even have become inverted. This singles out one hypothesis as the dominant explanation: parenthood. Thus, coming back to the issue of pay differentials across genders, we find that the lingering wage differential is significantly explained by the higher burden placed on women in caring for children.

The results in this paper have even more relevance, considering the differing effects of the COVID-19 pandemic. There is now ample evidence that women have been more affected than men, and that this may have long-term detrimental consequences on women’s labor outcomes via the mechanisms identified in this study. For example, Alon et al. (2020) outline how COVID-related school closures affect working mothers to a greater extent than working fathers. Women have borne the brunt of added childcare, home schooling, and housework associated with the pandemic (Del Boca et al., 2020; OECD, 2020). This gender-based
asymmetry may have long-term effects, as it is more costly to re-enter the labor force than it is to remain in the labor force. Pointing again to the importance of children (and thus parenthood), Heggeness (2020) finds that both men and women who had no school-age children suffered no changes, on average, in their employment status or hours worked, in contrast to those who had school-age children. Thus, the COVID-19 pandemic reinforces the hypothesis that parenthood and the differing gender-based roles of parenting, are significant drivers of the wage gap.

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Code Availability Available on request.

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