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Employer-sponsored health insurance and labor market outcomes for men in same-sex couples: Evidence from the advent of pre-exposure prophylaxis☆

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ABSTRACT

In the United States, the cost of providing employer-sponsored health insurance (ESI) varies for employers based on the medical expenditures of their employees, a practice known as "experience rating". Experience rating increases the cost of employing workers who have greater medical expenditures, one example being men in same-sex couples. To study whether ESI affects labor market outcomes for men in same-sex couples, I use the 2012 advent of Pre-Exposure Prophylaxis (PrEP), a $24,000 per year drug that effectively prevents Human Immunodeficiency Virus (HIV) acquisition. Using American Community Survey data and a difference-in-difference empirical approach – comparing post-PrEP changes in earnings among men who have ESI – I find that annual earnings for men in same-sex couples decline by $2,650 (approximately 3.9%) relative to comparable men after PrEP becomes available. For those who are most likely to be taking Truvada (the brand name for PrEP), such as young men and white men, effects on earnings are considerably larger. I also observe a 3.7 percentage point (4.6%) decline in ESI prevalence and a 0.8 percentage point (10.7%) increase in part-time employment among men in same-sex couples. Event studies provide support for a causal interpretation for my findings. My estimates are also robust to placebo analyses, various specification permutations, and a range of sensitivity checks.

1. Introduction

In 2012, the United States Food and Drug Administration (FDA) approved "Truvada" as a form of Pre-Exposure Prophylaxis (PrEP) to help prevent Human Immunodeficiency Virus (HIV) acquisition. The drug is primarily used by men who have sex with men (MSM), with estimates suggesting that between 20% and 30% of MSM use PrEP (Kanny et al., 2019; Beer et al., 2020; Bosco et al., 2021). While Truvada is highly effective when taken correctly, the drug carries an annual price tag of $24,000. Adding to the medical expenditures of those who choose to take PrEP, prescription refills require quarterly clinical visits and STI screenings, including HIV testing.[1] Because the cost of providing employer-sponsored health insurance (ESI) varies for employers based on the medical expenditures of their employees, a practice known as "experience rating", employers will therefore prefer to employ workers who are less likely to use PrEP, unless wages are free to adjust for the drug's expected cost (Summers, 1989).

To estimate how the costs associated with PrEP/Truvada affect labor market outcomes, I rely on demographic information, including using same-sex marriage and cohabiting partner status to identify men in same-sex couples, along with earnings, employment status, and health insurance coverage information from the 2009 to 2019 waves of the American Community Survey (ACS). Using a difference-in-difference approach, comparing labor market outcomes for men in same-sex couples to those in different-sex couples before and after the advent of Truvada, I find that men in same-sex couples who have ESI experience a $2,650 (3.9%) decline in annual earnings after the advent of PrEP relative to men in different-sex couples. While it is not possible to determine exact rates of PrEP usage among men in same-sex couples who have ESI, a $2,650 decline in annual earnings is reasonable given the $4,800 to $8,000 expected cost implied by the 20% to 30% PrEP take-up rates reported by Kanny et al. (2019), Beer et al. (2020), and Bosco et al. (2021), especially if employers can identify men in

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[1] See https://www.hiv.gov/hiv-basics/hiv-prevention/using-hiv-medication-to-reduce-risk/pre-exposure-prophylaxis.

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same-sex couples only imperfectly, employers are able to negotiate discounts on Truvada via their Pharmacy Benefit Managers (PBMs), or if employers are unable to effectively pass along PrEP's cost in all circumstances.

Providing support for a causal interpretation for my findings, an event study analysis focusing on differences in earnings between men in same-sex couples and different-sex couples in the years prior to Truvada's approval shows no evidence of pre-trends that could explain my findings. Formally, my approach identifies the effect of PrEP on labor market outcomes for men in same-sex couples so long as there are no omitted idiosyncratic shocks that are correlated with PrEP's approval and labor market outcomes for these men.

Notably, two-thirds of PrEP users in 2016 were aged 25 to 44, with a further 11% being under 25.[2] Consistent with such usage patterns, and further supporting a causal interpretation for my findings, my estimates show that younger men in same-sex couples experience relatively larger declines in earnings after Truvada's approval. Similarly, I find that white men in same-sex couples experience greater declines in earnings, consistent with white men being more likely to be aware of and taking PrEP (Kanny et al., 2019; Starks et al., 2019). When looking at employment outcomes, I find evidence of post-PrEP declines in employment and ESI prevalence among gay men, consistent with the idea that the cost of providing PrEP reduces employer demand for the labor of men in same-sex couples. I also find a 0.8 percentage point (10.7%) increase in the proportion of men in same-sex couples working part-time, defined as fewer than 30 h per week. Finding an increase in part-time employment is important because, under Affordable Care Act (ACA) rules, those working fewer than 30 h per week do not have to be offered health coverage. Further, I find no comparable effects for females in same-sex couples, which helps to ease concerns that changes in attitudes and/or same-sex marriage laws can explain what I observe among men in same-sex couples. I also show that my estimates are robust to a variety of specification, weighting, clustering, and sample selection choices.

My findings contribute in three important ways. First, I use the change in expected costs relating to PrEP to provide novel estimates of how ESI affects workers by sexual orientation. This is particularly timely because Gavulic and Gonzales (2022), using 2014 to 2017 Medical Expenditure Panel Survey data, find that men in same-sex couples in the United States have $6,896 in annual medical expenditures while medical expenditures for men in different-sex couples were $3,994 per year. Second, my work adds a new potential explanation to the literature that studies historical differences in gay men's earnings (Badgett, 1995; Allegretto and Arthur, 2001; Carpenter, 2004, 2007). In particular, my findings provide indirect evidence to suggest that changes in medical expenditures, such as those relating to the prevalence of and treatment costs associated with HIV/AIDS over time, could help to explain some of the historical wage penalty for gay and bisexual men. Finally, I provide evidence that newly-available pharmaceuticals can lead to lower earnings for particular groups if they increase the expected cost of providing ESI for that group. Given recent FDA approvals of increasingly-expensive pharmaceuticals – such as "Aduhelm", the controversial Alzheimer's drug (see Mazer, 2021) – it is important to study how the costs of new medications are passed on to workers who are likely to use such drugs via ESI-related effects on employment and earnings. In contrast, existing work focuses on how ESI affects wages using differences in medical expenditures between groups driven by health behavior (e.g., smokers vs. non-smokers) or the effects of policies that change what ESI must cover (e.g., maternity benefits) to aid identification, with recent examples including Bhattacharya and Bundorf (2009), Lahey (2012), and Bailey (2013).

[2] See https://www.aidsmap.com/news/mar-2018/prep-use-growing-us-not-reaching-all-those-need.

One important limitation, however, is that I cannot identify MSM who are "single" in my ACS data (i.e., neither married nor cohabiting with a same-sex partner). To the extent that PrEP use is less common among men in same-sex couples, compared to "single" MSM, my estimates would represent only a lower bound on the true effect of PrEP. On the other hand, the majority of new HIV infections occur within male same-sex partnerships (Sullivan et al., 2009; Starks et al., 2019). Moreover, as Starks et al. (2019) explain, "[t]he salience of gay couples as a context for HIV transmission risk is evident in CDC's PrEP guidelines, which specifically emphasize targeting PrEP to men in sero-discordant and non-monogamous relationships".[3] Sero-discordance refers to couples where one partner is HIV-positive. Illustrating that men in same-sex couples often take PrEP, Beer et al. (2020) report that "[t]wenty-eight percent of sexually active HIV-positive MSM reported at least one HIV-discordant male partner taking PrEP". Aside from PrEP use among sero-discordant couples, Starks et al. (2019) report that more than 37% of men in same-sex male couples engaged in sexual activity with another partner in the past 90 days. I further discuss PrEP use among men in same-sex couples in Section 2.

In Section 2, I also provide background information on how Truvada/PrEP works and expand on how my findings complement existing work on labor market outcomes for gay and bisexual men. In Section 3, I explain my ACS data, empirical strategy, and approach to estimation. In Section 4, I present my main findings along with event study estimates, heterogeneity analyses, and sensitivity checks. I offer concluding remarks in Section 5.

2. Background information and existing literature

The FDA approved Truvada as a form of Pre-Exposure Prophylaxis (PrEP) in July of 2012.[4] Truvada is prophylactic in the sense that it reduces HIV risk by blocking a specific enzyme that the virus needs to reproduce itself.[5] When Truvada is taken every day at the same time, it provides a 92% to 99% reduction in HIV risk (Anderson et al., 2012). While finding "the price" of any drug in the United States can be challenging, numerous sources report that Truvada costs about $2,000 per month.[6] While health coverage typically features some cost-sharing in the form of deductibles and co-payments, ACA-compliant coverage is supposed to cover Truvada (and any associated clinical visits/testing) with zero cost-sharing, as it is a preventative medication.[7] Insurance plans, however, almost always include a Pharmacy Benefit Manager (PBM) whose function is to manage the cost of prescription drugs. Dusetzina and Bach (2019) explain that PBMs typically negotiate rebates of 26% to 30% off list prices. However, the authors caution that PBMs are generally less effective at lowering net prices for drugs that lack competitors, which is relevant here because Truvada was the only approved form of PrEP until late 2019. Illustrating the high costs associated with Truvada, a 2020 Massachusetts Health Policy Commission study found that insurers were paying an average of $1,693 per month for Truvada.[8]

[3] See https://www.cdc.gov/vitalsigns/hivprep/index.html.

[4] In 2019, the FDA approved Descovy as another form of PrEP with much the same price. See https://www.fda.gov/news-events/press-announcements/fda-approves-second-drug-prevent-hiv-infection-part-ongoing-efforts-end-hiv-epidemic and https://www.sfaf.org/resource-library/side-by-side-comparison-truvada-and-descovy-for-prep/.

[5] See https://prepfacts.org/prep/the-basics/.

[6] See, for example, https://www.drugs.com/price-guide/truvada and https://www.healthline.com/health-news/cost-of-hiv-prevention-drug-discouraging-people-from-doing-prep-therapy.

[7] See https://www.nbcnews.com/nbc-out/out-health-and-wellness/prep-hiv-prevention-pill-must-now-totally-free-almost-insurance-plans-rcna1470.

[8] See Exhibit 8 in https://www.mass.gov/doc/prescription-drug-coupon-study/download.

While the exact cost of Truvada will therefore vary across insurers and their PBMs (which are often a subsidiary firm) it should be clear that Truvada is an expensive medication. Experience rating in the group health insurance market therefore ensures that, for firms that offer ESI as an employment benefit, the advent of PrEP makes men in same-sex couples considerably more expensive to employ, at least in expectation. In practice, how experience rating works is that greater-than-expected medical expenditures among workers who work for "fully-insured" firms (i.e., the insurance company assumes all risk) are passed on to those firms via increases in future premiums. Summers (1989) explains that this creates incentives to hire only workers with fewer medical expenditures, unless wages are free to adjust for any differences. On the other hand, Fleitas et al. (2018) show that the pass through from changes in risk to insurance premiums in the small group market (firms with fewer than 50 employees) is no more than 70%. That said, the type of "full" insurance that Fleitas et al. study is relatively uncommon in the United States. Instead, the Kaiser Family Foundation reports that, in 2020, 67% percent of U.S. workers who receive ESI were covered by self-insured plans, ensuring that costs are passed through to employers on a dollar-for-dollar basis, after any employee cost-sharing (i.e., almost perfect experience rating).[9]

Because employers ultimately foot most of the bill for employee healthcare expenditures, studies show that medical expenditure differences among groups of workers are shifted onto those workers with greater expenditures via diminished wages and/or employment prospects (examples include Gruber, 1994; Bhattacharya and Bundorf, 2009; Cowan and Schwab, 2011, 2016; Lahey, 2012; Bailey, 2013, 2014; Lennon, 2018, 2019). These studies tend to focus on differences in medical expenditures among groups or policy changes that affect what must be covered by insurance. Notable exceptions to that approach are Baicker and Chandra (2006), who use medical malpractice claims to examine how the cost of ESI is passed on to workers, Buchmueller et al. (2011), who examine Hawaii's 1974 Health Insurance Coverage Mandate, Kolstad and Kowalski (2016), who examine the effect of health insurance reform in Massachusetts in 2006, and Lennon (2021a), who uses the ACA's employer mandate to identify how the costs of ESI are passed on to workers at the individual level. On one hand, the findings in the literature seem largely inconsequential — workers are "paying" for a benefit that they value (Summers, 1989). On the other hand, the rising cost of health care means that ESI may increasingly act as a barrier to employment for workers whose total compensation (wages plus ESI benefits) exceeds the value of their marginal revenue product. Such concerns are naturally magnified for groups that have historically experienced differential treatment in the labor market — such as racial/ethnic minorities, females, or persons who identify as lesbian, gay, bisexual, or transgender - especially if they are also expected to have greater medical expenditures. For that reason, I use the advent of Truvada/PrEP to help us learn about how ESI might matter for labor market outcomes for men who have sex with men.

Of course, whether PrEP's effects on earnings and employment outcomes for men in same-sex couples can help us learn valuable information about the effects of PrEP depends on the extent to which PrEP is used by men in same-sex couples relative to other MSM. As I mention in the introduction, the Centers for Disease Control (CDC) highly recommends PrEP for men in same-sex couples that are serodifferent or non-monogamous. While they do not provide estimates of the number of serodifferent/sero-discordant same-sex male couples, the CDC estimates that 740,400 gay and bisexual men were living with HIV in 2018.[10] Further, men in same-sex couples often take PrEP with Beer et al. (2020) reporting that 28% of sexually active HIV-positive MSM have a male partner who takes PrEP. Beyond sero-discordant couples, Starks et al. (2019) find many same-sex couples are non-monogamous with 37.3% of men in their sample reporting that they had sex with a partner other than their main partner in the last 90 days. The CDC would recommend PrEP for these men.

More broadly, even if it were the case that HIV-negative men in same-sex couples never used PrEP, couple status (and HIV status) can change over time meaning that the probability of *future* PrEP usage would still be non-zero. It is also possible that men in same-sex couples who I observe in later ACS sample years may have been single at the time of Truvada's approval. Due to the ongoing nature of employment, they may continue to experience negative labor market effects even after becoming part of a couple or ceasing PrEP usage.

Note that Truvada could reduce the expected future costs of HIV treatment. However, given only a small fraction of men in same-sex couples would otherwise contract HIV during my sample period, I do not attempt to account for potential reductions in medical expenditures due to changes in HIV prevalence. Counteracting any potential reductions in medical expenditures from reduced HIV prevalence, Eilam and Delhommer (2022) examine moral hazard effects relating to PrEP and find that rates of chlamydia, gonorrhea, and syphilis would have been between 17.9% and 25.6% lower in the absence of PrEP. Eilam and Delhommer's estimates imply that those who use PrEP are taking fewer precautions to avoid other STIs (Sexually Transmitted Infections). Their findings align well with McManus and Tello-Trillo (2022) who show that PrEP has had limited effects on the incidence of HIV in the United States, with the median county experiencing just a 4.4% PrEP-related reduction in HIV incidence between 2012 and 2018. Increases in STI infections coupled with limited changes in HIV incidence suggest that, at least among some who take the drug, PrEP is being used as a substitute for other forms of HIV and STI prevention.

Given my focus, my findings also naturally contribute to a well-established literature that studies explanations for differences in labor market outcomes among sexual minorities. As one example, Ahmed et al. (2013) use a correspondence audit to study discrimination in hiring for gay men and lesbian women. They find that heterosexual men receive 14% more positive job application responses when compared to an otherwise similar gay male. For females, heterosexuals receive 22% more positive responses. While Ahmed et al. find heterosexual females receive more job application responses, Jepsen (2007) and Klawitter (2015) show that lesbian females tend to earn more than other comparable females. Other explanations for differences in earnings among males in same-sex couples include differences in labor supply (including market work vs. household production) relative to different-sex couples (Black et al., 2003; Black et al., 2007) and occupational sorting (Antecol et al., 2008).

The literature tends to search for explanations for why gay men earn less than comparable heterosexual men because work by Badgett (1995), Allegretto and Arthur (2001), and Carpenter (2004, 2007) firmly established that gay men experienced lower earnings. More recent estimates, however, suggest that gay men now experience an earnings premium. For example, Clarke and Sevak (2013), using National Health and Nutrition Examination Survey data, find that between 1988 and 2007 males who reported same-sex sexual behavior went from experiencing a household income penalty to a significant premium. Supporting this finding, Carpenter and Eppink (2017), using 2013 to 2015 National Health Interview Survey data, find a 10% annual earnings premium for gay men. Carpenter and Eppink argue that neither reduced discrimination nor changes in household specialization are likely to be the cause of their findings. Burn and Martell (2020) provide evidence to suggest that changes in occupational sorting among gay men might be a good explanation for these patterns. Further, Sansone (2019) shows that legalization of same-sex marriage led to increases in labor force participation and employment among same-sex couples. For those interested in more work in this area, Badgett et al. (2021) provide a thorough overview of "LGBTQ Economics" in their recent review article of the same name.

[9] See https://www.kff.org/report-section/ehbs-2020-section-10-plan-funding/.

[10] See https://www.cdc.gov/hiv/group/msm/index.html.

I contribute to this literature by providing novel evidence regarding a different potential source of variation in labor market outcomes for men in same-sex couples. By showing that the advent of PrEP leads to lower earnings for these men, my work highlights that ESI could play an important role in earnings for gay and bisexual men. Future work might find it valuable to examine whether the historical earnings penalty for MSM was affected by HIV prevalence and/or treatment costs.

3. Data and estimation

I examine how the advent of Truvada affects labor market outcomes for men in same-sex couples relative to men in different-sex couples using data on men in the labor force aged 18 to 64 from the 2009 to 2019 waves of the American Community Survey (ACS). Survey respondents provide information on their demographic characteristics, location, educational attainment, employment, earnings, and health insurance coverage. While the ACS does not ask respondents about their sexual orientation, it provides same-sex marital status and a variable that identifies same-sex unmarried partners, helping me to identify more than 67,000 men in a same-sex couple and in the labor force. Note that I begin my analysis with 2009 data because, as Sansone (2019) explains, the U.S. Census Bureau implemented several changes between 2007 and 2008 to help identify same-sex couples, creating separate categories for roommates and unmarried partners. In early sample years, the ACS recoded same-sex couples who reported that they were married as unmarried partners. Helpfully, the ACS provides a data flag that allows me to reassign these couples as married.

One limitation in ACS data is that those who have health insurance from their own employer or from their spouse's or partner's employer are denoted as being covered by ESI. It is possible therefore that my sample contains a significant number of men in same-sex couples who obtain ESI via a spouse or partner. Naturally, it would be ideal if my sample consisted of only those who have ESI from their own employer. On the other hand, the Kaiser Family Foundation (KFF) reports that 89 to 92 percent of American workers worked at a firm that offers health benefits during my sample period.[11] Also, KFF reports that coverage via one's own employer is considerably less expensive relative to dependent coverage, meaning that it is unlikely that my sample consists of a large fraction of working males who obtain coverage as a dependent rather than via their own employer.[12] To try to increase the likelihood that my respondents are those covered by ESI from their own employer, I later present estimates limiting my sample to men who work full-time, which I define as more than 30 h per week.

I present summary statistics for men in same-sex couples versus men in different-sex couples aged 18 to 64 who are in the labor force in Table 1. Because the ACS top-codes earnings in the top one half-percentile by year, I exclude respondents with earnings greater than $300,000, using a single cut-off to maintain comparability across years. That restriction leaves me with approximately 5.24 million respondents, with a little over 1% of them being men in same-sex couples. Naturally, when looking at outcomes that depend on being employed, my sample is further restricted to only respondents who report that they are working. Among the 5.24 million respondents who are aged 18 to 64 and in the labor force, 4.66 million reported that they were working at the time they were surveyed by ACS during my sample period.

Looking at the summary statistics, we can see that men in same-sex couples have greater earnings and educational attainment. They are also more likely to be white, less likely to be married, are younger, and are more likely to be a student, which is unsurprising given their greater educational attainment. Given the differences in education levels, in particular, it is perhaps not surprising that men in same-sex couples are several percentage points more likely to have ESI and have greater annual earnings. Note that given the sample size, even relatively small differences among groups are statistically significant.

Of course, it is worth repeating that many MSM are not part of a married or cohabiting same-sex couple. Unfortunately, I cannot identify those "single" MSM in my sample. As I mention earlier, the effect of that limitation is less clear than one might suspect with PrEP being highly recommended for many men in same-sex couples (Starks et al., 2019). More speculatively, it is possible MSM who are not in couples may be harder to identify for employers, limiting Truvada's effects on their outcomes. In the next subsection, I describe my approach to examining whether the advent of Trvuada was responsible for differences in labor market outcomes among men in same-sex couples after 2012.

3.1. Estimation

When studying the effect of PrEP ("Truvada") on labor market outcomes for men in same-sex couples (MSC) my estimating equation is as follows:

$$Y_{it} = \kappa + \phi_1 MSC_{it} + \phi_2 After\ 2012_t + \phi_3 After\ 2012_{it} \times MSC_{it} + X_{it}\Pi + \epsilon_{it}. \quad (1)$$

In Eq. (1), Y_{it} refers to some labor market outcome of interest for individual i at time t. Because my estimation sample is restricted to males, the MSC_{it} term is an indicator variable that equals one for men who report that they live with or are married to a same-sex partner and is zero otherwise. The $After\ 2012_t$ indicator term equals one whenever $t > 2012$ (i.e., after the approval of Truvada) and zero otherwise. The coefficient on the interaction of the indicator terms therefore represents the difference between outcomes for men in same-sex couples versus men in different-sex couples, after PrEP becomes available. Because PrEP can cost plan sponsors up to $24,000 per year (plus the cost of quarterly clinical visits and testing), and because ESI is experience rated, we would expect $\hat{\phi}_3 < 0$, all else being equal. Completing the estimating equation, I include an idiosyncratic error term, ϵ_{it}, along with controls for demographic characteristics and fixed effects, X_{it}.

In Section 4, I use my ACS data sample along with the estimating equation above to examine how ESI affects men in same-sex couples, via the cost of PrEP/Truvada. As I mention earlier, I limit my sample only to male ACS respondents age 18 to 64 who are in the labor force and part of a married or cohabiting couple. However, I present analyses using a sample including "single" men, finding similar effects, as an appendix item. Also as an appendix item, I present estimates using a difference-in-difference approach that compares earnings across groups by ESI status. Bhattacharya and Bundorf (2009) and Cowan and Schwab (2011, 2016) use this type of approach to study how ESI affects earnings for obese workers, smokers, and females. As part of that analysis I explain why the identifying assumptions required for such an approach are unlikely to hold. Indeed, I show that this approach can give wrong-signed estimates when looking at the effect of ESI on earnings.

Note that men in same-sex couples who do not have ESI are an additional potential comparison group. On the other hand, workers without ESI could be disproportionately affected by the Affordable Care Act's changes during the sample period (i.e., expansions in Medicaid eligibility, subsidized coverage available on the act's healthcare exchanges, etc.). For that reason, I present estimates focused on those who do not have ESI only as an additional appendix item. Finally, because I use a two group (men in same-sex couples versus those in different-sex couples), two-period (before versus after the advent of PrEP) difference-in-difference approach, with a single treatment date, my estimates are not subject to the heterogeneous treatment effect issues raised by the new difference-in-difference literature (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021).

[11] See Figure F at https://www.kff.org/report-section/ehbs-2020-summary-of-findings/.

[12] See Figure B at https://www.kff.org/report-section/ehbs-2020-summary-of-findings/.

Table 1
Summary statistics.
Source: 2009 to 2019 data from the American Community Survey (ACS), men age 18 to 64 in the labor force. When looking at earnings from employment, ESI status, and part-time status the sample is naturally restricted to only those respondents who report that they are working. Because the ACS top-codes earnings in the top one half-percentile by year, I exclude all respondents with earnings greater than $300,000, using a single cut-off to maintain comparability across years.

	Men in different-sex couples	Men in same-sex couples	Total
Annual earnings from Employment	$ 61,830	$ 62,885	$ 61,844
Annual earnings (conditional on ESI)	$ 65,552	$ 67,424	$ 65,577
Health Insurance from an Employer	78.4%	80.2%	78.4%
Part-time (fewer than 30 h per week)	4.0%	7.0%	4.1%
Education (Highest Level Completed)			
Less than High School	9.8%	4.3%	9.7%
High School	55.7%	45.1%	55.6%
College	21.4%	29.4%	21.5%
Graduate	13.1%	21.1%	13.2%
Race			
White	71.8%	73.7%	71.8%
Black	6.4%	5.0%	6.4%
Hispanic	7.9%	7.3%	7.9%
Other	14.0%	14.1%	14.0%
Married	88.3%	34.5%	87.6%
Age	44.5	42.9	44.5
No. of Children (at home)	1.18	0.21	1.17
Lives in a Metropolitan/Urban Area	74.6%	88.6%	74.8%
Student Status	4.3%	7.8%	4.3%
Disability Status	5.3%	5.8%	5.3%
Citizen (native born)	81.5%	85.9%	81.6%
Main Language is English	79.4%	82.9%	79.4%
Observations	5,174,433	67,696	5,242,129

4. Main findings

In Table 2, I examine how the advent of PrEP affected labor market outcomes for men in same-sex couples, using OLS to estimate the coefficients from the specification described in Eq. (1) in Section 3. I use ACS-provided weights in all specifications, presenting estimates first with only demographic controls and then with demographic controls and fixed effects for each of my outcomes. Demographic controls include race, education, age, marital status, number of children, student status, disability status, citizenship status, and main language spoken. Fixed effects include state of residence, year, PUMA, metro status (i.e., urban, suburban, rural), industry, and occupation.[13] I also include state-by-year fixed effects to account for unobserved state-level changes that could affect labor market outcomes for men in same-sex couples (MSC) relative to men in different-sex couples. For that reason, even though the estimates are mostly very similar, I consider the specifications including fixed effects to be my preferred specification.

My outcome variables in Table 2 are an indicator for being employed, an indicator for ESI prevalence among employed men, an indicator for part-time status (fewer than 30 h per week), and annual earnings. Whenever my outcome is an indicator variable, OLS estimates a linear probability model and the coefficients (×100) should be interpreted as percentage point changes. Broadly speaking, my estimates show that the advent of PrEP is associated with lower employment rates, reduced ESI prevalence, an increase in part-time employment, and a large decline in earnings for men in same-sex couples.

Looking more closely at employment, where the estimation sample consists of men in same- and different-sex couples aged 18 to 64 who are in the labor force, I find that men in same-sex couples are 1.3 percentage points less likely to be employed after the advent of PrEP in my preferred specification. Conditional on being employed, the estimates in column (4) suggest that men in same-sex couples are 3.7 percentage points less likely to have ESI after the advent of Truvada. The point estimates are statistically significant at the 1% level.

Looking at earnings, I find a $2,650 relative decline after 2012 for men in same-sex couples in my preferred specification in column (8). The estimate is also statistically significant at the 1% level. Given mean annual earnings of just over $67,000 for men in same-sex couples, the effect amounts to a 3.9% relative decline in earnings for men in same-sex couples compared to men in different-sex couples after the advent of PrEP. If Truvada costs about $24,000 per year, a $2,650 decline in annual earnings would be the expected wage offset if 11% of men in same-sex couples in my sample were PrEP users. If a larger proportion use PrEP, my estimates would be reasonable if PBMs were able to negotiate some discount on the cost of the drug for plan sponsors, employers were able to identify men in same-sex couples only imperfectly, or if employers were otherwise unable to pass along the cost of PrEP to all workers (e.g., perhaps it is easier to pass along the cost to new employees via lower wage offers but harder to do so for existing workers). In Fig. 1, I provide event study plots that show a change in earnings that occurs after PrEP is approved along with no evidence of problematic pre-trends. I discuss my event study analyses further in the next subsection.

Note that under Affordable Care Act (ACA) rules, those working fewer than 30 h per week do not have to be offered health coverage. Therefore, employing men in same-sex couples on a part-time basis could also help employers avoid the costs associated with Truvada. Defining part-time as working fewer than 30 h per week, I find gay men are 0.8 percentage points more likely to work part-time after

[13] The Census Bureau explains that "Public Use Microdata Areas (PUMAs) are non-overlapping, statistical geographic areas that partition each state or equivalent entity into geographic areas containing no fewer than 100,000 people each". See https://www.census.gov/programs-surveys/geography/guidance/geo-areas/pumas.html for more information.

Table 2
OLS estimates focusing on the advent of PrEP.

	(1) Employed	(2) Employed	(3) Has ESI	(4) Has ESI	(5) Part-time	(6) Part-time	(7) Annual earnings	(8) Annual earnings
After 2012	0.035***		0.001***		−0.003***		6,961***	
	(0.000)		(0.000)		(0.000)		(44)	
MSC	0.021***	0.015***	0.086***	0.080***	0.001	−0.006***	4,404***	2,920***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(356)	(310)
MSC × After 2012	−0.015***	−0.013***	−0.045***	−0.037***	0.011***	0.008***	−2,800***	−2,650***
	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)	(414)	(360)
Observations	5,242,129	5,242,129	4,659,439	4,659,439	4,659,439	4,659,439	3,652,016	3,652,016
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes

Data: 2009 to 2019 ACS restricted to men age 18 to 64 in a same- or different-sex couple and who are in the labor force. In columns (3) to (6), the sample is further restricted to males who are employed. In the final two columns, the sample is restricted only to men who are covered by ESI. Standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. Demographic Controls include race, education, age, marital status, number of children, student status, disability status, citizenship status, and main language spoken. Fixed effects include state of residence, year, state-by-year, PUMA, metro status (i.e., urban, suburban, rural), industry, and occupation. Naturally, when including year fixed effects, we no longer observe an "After 2012" coefficient. MSC = Male in a same-sex couple.

2012 in my preferred specification in column (6), amounting to a 10.7% increase in part time employment relative to rates of part-time employment among men in same-sex couples prior to 2012.

4.1. Event study analyses

To study whether there are differential trends that would threaten identification, I estimate an event-study specification that is a time-disaggregated version of the difference-in-difference estimating equation that I specify in Eq. (1) in Section 3:

$$Y_{it} = MSC_{it} \times \sum_{k=-l}^{m} \delta_k 1[t - T_i = k] + \rho \times MSC_{it} + \gamma_t + X_{it}\Pi + \epsilon_{it}. \quad (2)$$

In Eq. (2), the key difference versus Eq. (1) is that I replace the indicator for "PrEP" with a set of time period indicators $1(t - T_i = k)$ interacted with the indicator for men in same-sex couples (MSC_{it}). The time period indicator term equals 1 only for respondents in year t when it is k years away from T_i, the first full year PrEP is available (i.e., 2013). The coefficients on each time period indicator represent the difference in outcome Y_{it} between men in same-sex couples and men in different-sex couples relative to the same difference in 2012, which is the "omitted" year (i.e., $k = -1$, the year of PrEP approval). Because the focus here is on examining pre-trends, I collapse all observations beyond 2016 into a single time period.[14] Note that I also include a year fixed effect γ_t in place of only an indicator for the period after PrEP/Truvada is announced, along with demographic controls and fixed effects. Please note that the description and notation in this section borrows from Miller and Wherry (2019), Teltser et al. (2021), and Lennon (2021b).

I present event study plots for my outcomes of interest in Fig. 1, with the timing of Truvada's approval indicated by the vertical line centered between the years 2012 and 2013 in each subfigure. The event studies provide relatively strong support for the parallel trends assumption when looking at my annual earnings, ESI prevalence, and part-time employment outcomes. The event studies also allow us to examine how rapidly any labor market changes occurred. Looking at earnings specifically, it appears that there is a relatively small negative effect in 2013 and the 95% confidence interval, represented by vertical bars around the point estimates, includes zero. After 2013, the effect on earnings becomes larger and statistically significantly different from zero. Given Truvada usage took a few years to ramp up (McManus and Tello-Trillo, 2022), it would make sense for its effects to take some time to become apparent in labor market outcomes. On the other hand, for employers in 2012, it would not have been clear that take up of this breakthrough drug would be slow. Moreover, to the extent that employment is an ongoing relationship, then the advent of PrEP changes the future stream of expected medical expenditures for MSM, even if relatively few used PrEP as early as 2013.

Looking at employment, however, it is difficult to rule out that there is a pre-trend that continues after the advent of PrEP. Therefore, while PrEP may have had some effect, it is not possible to make strong claims regarding PrEP's causal effect on employment outcomes. Speculating somewhat, it is possible that the advent of PrEP had effects on both labor demand and labor supply for MSM, particularly labor supply toward firms that offer ESI, because ESI is more valuable for those men after 2012. However, opposing changes in both labor supply and demand mean that we might not expect to see a significant break from trend in employment even when we see a clear effect on earnings.

4.2. Heterogeneity, sensitivity, placebo analyses

4.2.1. Heterogeneity

In Panel A of Table 3, I present estimates that examine how outcomes for younger men in same-sex couples change relative to older men in same-sex couples after the advent of PrEP. Because evidence suggests younger males are more likely to be taking PrEP (Beer et al., 2020; McManus and Tello-Trillo, 2022), effects on labor market outcomes are likely to be highly concentrated among those under 40 years of age, all else equal. To produce the estimates in Panel A of Table 3, I limit my sample to men in same-sex couples and use an estimating equation similar to Eq. (1) in Section 3:

$$Y_{it} = \omega + \rho_1 Young_{it} + \rho_2 After\ PrEP_{it} + \rho_3 After\ PrEP_{it} \times Young_{it} + X_{it}\Pi + \epsilon_{it}. \quad (3)$$

Relative to Eq. (1), in Eq. (3) the MSC_{it} indicator term is replaced with a $Young_{it}$ term that equals one whenever $Age_{it} < 40$ and zero otherwise. All else is the same as in Eq. (1). Note that in Table 3 and all further estimates in the paper, the estimates reflect a specification including demographic controls and fixed effects, which is my preferred specification as I explain when discussing Table 2.

[14] Sun and Abraham (2021) show that the key parameters of interest, δ_k, remain identified when collapsing observations where $t > m$ into period $k = m$ (and those where $t < -l$ into period $k = -l$, although I do not do that in this case).

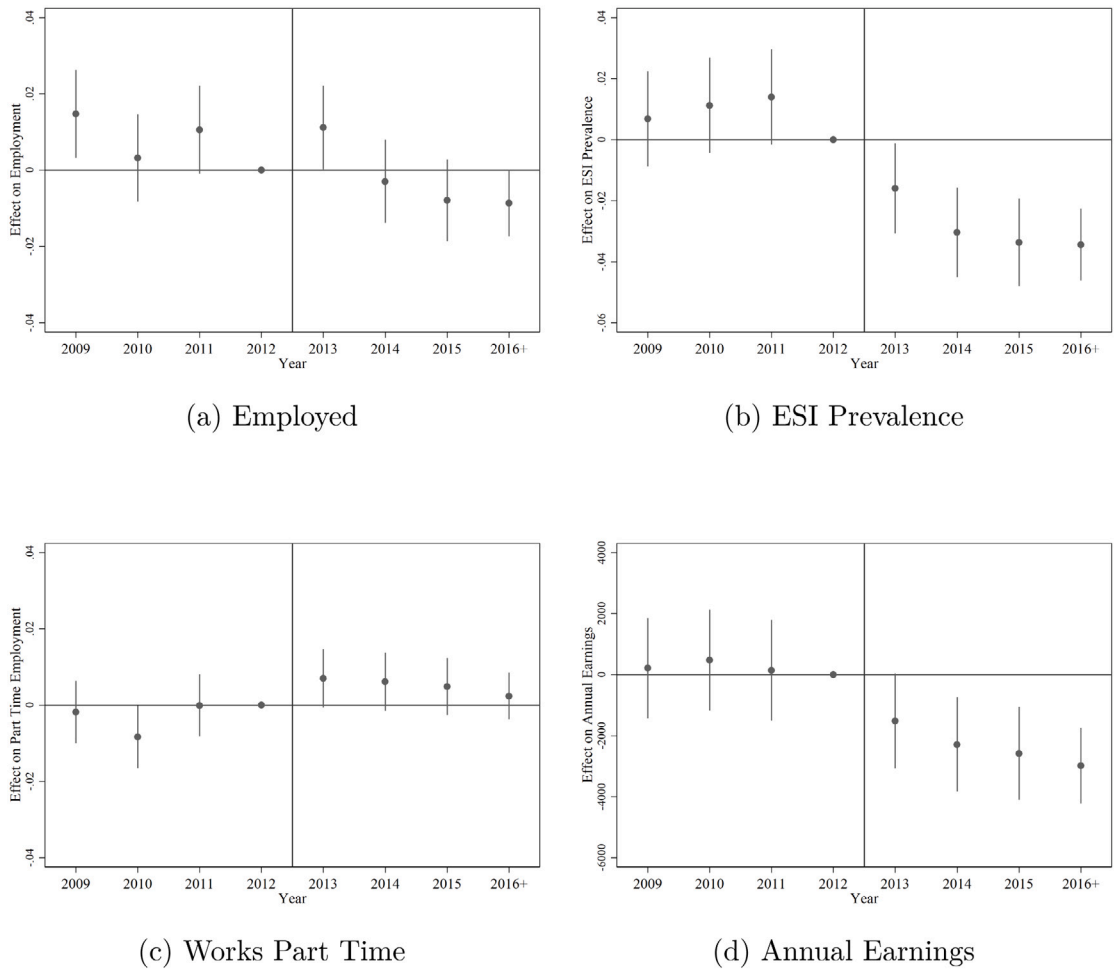

Fig. 1. Event Studies for Main Outcomes.
Notes: Event studies use ACS respondents age 18 to 64 who are in the labor force and are in either a same-sex or different-sex couple. The dependent variable is noted below the related subfigure. In subfigures (b) and (c) the sample is further restricted to those are currently employed and in subfigure (d) the sample consists only of workers with ESI. In all analyses, the year 2012 is the "omitted" time period ensuring that the point estimates refer to differences relative to that year. Vertical bars around point estimates represent 95% confidence intervals and the vertical line between 2012 and 2013 denotes the timing of Truvada's approval.

Looking at earnings in column (4) of Table 3, the estimates show that relative to older men in same-sex couples, younger men in same-sex couples experience a $2,214 decline in earnings after the advent of PrEP. That effect is statistically significant at the 1% level. For context, my estimates in Table 2 suggested that men in same-sex couples experience a $2,650 overall decline in earnings relative to men in different-sex couples. Because the sample here is already restricted to a group that experiences a relative decline in earnings after 2012, the estimates in Table 3 imply that younger men in same-sex couples experience more than a $2,650 relative decline in earnings. Naturally, that means that older men in same-sex couples experience a smaller than $2,650 relative decline. Such a pattern is consistent with younger males being significantly more likely to take PrEP. In contrast, I find younger men in same-sex couples experience smaller declines in employment and ESI prevalence, relative to older men, perhaps suggesting heterogeneous changes in labor supply in response to the advent of PrEP.

In Panels B and C of Table 3, I examine outcomes for white and then non-white men (consisting mostly of black and hispanic males). While the 95% confidence intervals overlap, the pattern of estimates, with white men in same-sex couples experiencing larger declines in annual earnings relative to non-white men aligns well with evidence that white men are more likely to be aware of and taking PrEP. For example, Kanny et al. (2019), using 2017 National HIV Behavioral Surveillance data, report that 42% of white, 30% of Hispanic, and 26% of black men in urban areas report taking PrEP. The pattern of findings in Table 3, given PrEP use is greatest among younger men and white men, strongly suggests that the advent of PrEP explains a significant portion of the changes in labor market outcomes after 2012 for men who may take the drug.

4.2.2. *Placebo analysis using female ACS respondents*

In Table 4, I present estimates where I compare outcomes for females in same-sex couples to females in different-sex couples after the advent of PrEP. Because females are unlikely to take PrEP (McManus and Tello-Trillo, 2022), this exercise serves as a falsification/placebo test. If I were to find that there were similar effects on earnings and employment outcomes for females in same-sex couples, then it is likely some other change, that affects individuals in same-sex couples more broadly, is driving my findings.

I find, however, no comparable effects for females in same-sex couples. For example, while there is a decline in employment among men in same-sex couples after the advent of Truvada, females in same sex couples are 0.6 percentage points more likely to be employed relative to females in different-sex couples. Further, I find a statistically insignificant $374 decline in earnings for females in same-sex couples. This compares to a $2,650 decline in annual earnings for men

Table 3
OLS estimates — Age and race heterogeneity.

	(1) Employed	(2) Has ESI	(3) Part-time	(4) Annual earnings
Panel A — Young MSC				
Young (Aged 18 to 40)	−0.005	−0.030***	−0.008	−3,986***
	(0.004)	(0.007)	(0.005)	(858)
Young × After 2012	0.011***	0.029***	−0.006	−2,214***
	(0.004)	(0.007)	(0.005)	(803)
Observations	67,661	60,342	60,342	48,386
Panel B — White Men				
MSC	0.016***	0.081***	−0.006***	3,083***
	(0.002)	(0.003)	(0.002)	(353)
MSC × After 2012	−0.012***	−0.038***	0.005***	−3,042***
	(0.002)	(0.004)	(0.002)	(414)
Observations	3,762,667	3,355,627	3,355,627	2,776,783
Panel C — Non-White Men				
MSC	0.009**	0.076***	−0.006*	3,312***
	(0.004)	(0.007)	(0.004)	(645)
MSC × After 2012	−0.006	−0.034***	0.010**	−2,441***
	(0.004)	(0.008)	(0.004)	(728)
Observations	1,479,462	1,303,811	1,303,811	875,233
Demographic Controls	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes

Data: 2009 to 2019 ACS restricted to men in a same- or different-sex couple aged 18 to 64 and who are in the labor force. In Panel A, the sample is further restricted to only men in same-sex couples. In Panel B and C, the sample is restricted to white and non-white men as indicated. In columns (2) and (3), the sample includes only males who are employed. In the final column, the sample is restricted only to men who are covered by ESI. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Demographic Controls include race, education, age, marital status, number of children, student status, disability status, citizenship status, and main language spoken. However, for estimates stratified by self-reported race I do not separately control for race. Fixed effects include state of residence, year, state-by-year, PUMA, metro status (i.e., urban, suburban, rural), industry, and occupation. MSC = Male in a same-sex couple.

in same-sex couples, statistically significant at the 1% level, in the corresponding specification in Table 2.

If my findings for men were somehow caused by changing attitudes towards the LGBTQ population or by changes in same-sex marriage laws, we would expect to find similar effects when focusing on females. My event study analyses in Fig. 1, showing large changes occurring after 2012, also suggest it is unlikely that changes in attitudes (which we might expect to change only gradually) or same-sex marriage laws, because same-sex marriage was only legalized at the federal level in the U.S. on June 26, 2015, can explain my findings. For these reasons, my estimates when looking at females further support the idea that PrEP affected labor market outcomes for men in same-sex couples. For completeness, I present event study plots for females in Fig. A.1, showing no evidence of pre-trends or post-PrEP effects on labor market outcomes for females in same-sex couples.

As an appendix item, to further examine whether same sex marriage legalization could be driving my findings, I present estimates where I control for whether same-sex marriage is legal in a state in a given year using an indicator variable that equals 1 if same sex marriage is legal in year t and is zero otherwise. When controlling for the effect of same-sex marriage legalization in this manner, the point estimates are very similar to my main estimates (in Table 2) and the 95% confidence intervals overlap considerably for each outcome, reinforcing the idea that the legalization of same-sex marriage is not driving my findings.

4.2.3. Sensitivity

In Table 5, I examine the sensitivity of my findings to choices regarding clustering, weighting, and the analysis sample period. I also provide estimates limiting the sample to private sector workers only, full-time workers only, and when I include industry-by-location-by-year fixed effects. Overall, while the point estimates differ, the 95% confidence intervals overlap for each outcome across specifications and also overlap with those of my main estimates (see Table 2). Note that, to avoid the table becoming cluttered, I only present the coefficient on the interaction terms from each sensitivity analysis.

Specifically, Panel A of the table repeats my main estimates but where I cluster the standard errors at the state level. While this is an overly conservative approach, given treatment (the advent of PrEP) does not vary at the state level, the point estimates remain statistically significant at the 1% level. Panel B provides estimates without using ACS-provided person weights (as I do in all other estimates). There I find similar effects for employment and ESI outcomes but a larger $3,178 wage offset for men in same-sex couples relative to men in different-sex couples after the advent of PrEP. Panel C examines what happens if I trim my sample and focus only on the years 2010 to 2016, rather than 2009 to 2019 as in my main estimates. Consistent with the increasingly negative effects I observe over time in my event studies, I find slightly smaller effects of PrEP when limiting the sample. AIDSvu, an "interactive online mapping tool that visualizes the impact of the HIV epidemic on communities across the United States", reports that PrEP use increased by 73% per year in the years leading up to 2016.[15] With PrEP use rising rapidly, it makes sense that I would find even larger effects when using a longer sample period.

In Panel D, I eliminate self-employed workers and public sector workers from my estimates and find slightly larger negative effects on earnings. Given 82% of workers in my sample are in the private sector, this demonstrates that any effects for those in the public sector and those who are self-employed are significantly smaller. In Panel E, I limit the sample to workers who work more than 30 h per week.

[15] See https://aidsvu.org/prep/.

Table 4
OLS estimates focusing on females.

	(1) Employed	(2) Has ESI	(3) Part-time	(4) Annual earnings
FSC	−0.003**	−0.020***	−0.039***	4,666***
	(0.001)	(0.003)	(0.003)	(224)
FSC × After 2012	0.006***	0.004	0.004	−374
	(0.002)	(0.003)	(0.003)	(265)
Observations	4,587,667	4,134,677	4,134,677	3,319,759
Demographic Controls	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes

Data: 2009 to 2019 ACS restricted to females age 18 to 64 in a same- or different-sex couple and who are in the labor force. In columns (2) and (3), the sample is further restricted to females who are employed. In the final column, the sample is restricted only to those who are covered by ESI. Standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. Demographic Controls include race, education, age, marital status, number of children, student status, disability status, citizenship status, and main language spoken. Fixed effects include state of residence, year, state-by-year, PUMA, metro status (i.e., urban, suburban, rural), industry, and occupation. FSC = Females in Same-sex Couples.

One value of this specification is to limit the potential for a large fraction of my sample to be covered by ESI via a spouse/partner. Generally, few part-time workers receive health coverage from their employer. There, I find a larger $3,013 effect on earnings. In Panel F, I add industry-by-location-by-year fixed effects to the specification, where location refers to Public Use Microdata Area or PUMA. The goal with this specification is to account for areas and industries that have concentrations of workers who are men in same-sex couples. If my estimates change significantly after including such fixed effects, it would suggest that my findings could be explained by broader changes in remuneration to all employees, regardless of sexual orientation, at firms heavily impacted by the cost of Truvada. Instead, the coefficient estimates are quite similar to my other estimates, although the point estimate for the effect on earnings is several hundred dollars smaller. Again, however, the 95% confidence interval overlaps with my main estimates in Table 2 and the other specifications in Table 3, limiting any strong conclusions.

5. Discussion and conclusion

The advent of Truvada in 2012, a Pre-Exposure Prophylaxis (PrEP) drug that effectively prevents HIV acquisition, significantly increased the expected cost of employing men who have sex with men. Because ESI is experience rated, either via changes in premiums or via the direct costs of self-insurance, economic theory predicts that employers will prefer workers who are not likely to use PrEP, unless wages are free to adjust to compensate the employer for its expected cost (approximately $24,000 per year times the probability of use). This remains true even if PrEP eliminates HIV transmission among users because (1) the cost of PrEP is similar to the annual cost of HIV treatment (McCann et al., 2020) and (2) only a small fraction of men who take PrEP would otherwise contract HIV. In any case, McManus and Tello-Trillo (2022) find that PrEP has had limited effects on the incidence of HIV, which suggests that PrEP is often substituting for other effective HIV prevention strategies.

Using data from the American Community Survey my findings show that after the advent of PrEP (in 2012), annual earnings for men in same-sex couples decline by at least 3.9% ($2,650) relative to comparable men. Notably, when I limit the sample to those working full-time, I find a larger $3,013 relative decline in earnings. Event study analyses, placebo analyses focusing on females in same-sex couples, and a range of heterogeneity and sensitivity analyses provide support for a causal interpretation. I also find evidence of lower levels of employment, reduced ESI prevalence, and increases in part-time work among men in same-sex couples consistent with employers reducing their demand for the labor of men in same-sex couples and/or employing them on a part-time basis to avoid providing health benefits under ACA rules.

To the extent that PrEP provides a convenient and effective way to reduce HIV risk, the advent of Truvada means that ESI becomes increasingly valuable for men who have sex with men after 2012. As ESI becomes more valuable, it is likely that my estimates reflect changes in both labor supply and demand. Indeed, the effects I observe could be entirely due to changes in labor supply towards firms that offer ESI. In such a case, men in same-sex couples would still be "paying" for the cost of PrEP via lower earnings. Although, consistent with other types of insurance, it is quite likely that many who do not use Truvada are paying for its cost via negative labor market effects. However, given PrEP is potentially so valuable to those at risk of HIV infection, it is difficult to make any overall welfare claims.

There are some important caveats and limitations to my findings. One significant limitation is that my data only allows me to identify men in same-sex couples. I cannot identify or examine outcomes for "single" men who have sex with men. We might expect these non-partnered gay and bisexual men to be more likely to take PrEP, even though PrEP is recommended and used by many men in same-sex couples (Starks et al., 2019; Beer et al., 2020). To the extent that single MSM are more likely to use PrEP, however, my difference-in-difference estimates are likely a lower bound on the drug's true effect.

A second potential limitation is that my sample period begins around the time of the Great Recession. It is possible, although not obviously the case, that the Great Recession affected men differently by sexual orientation. In addition, PrEP was approved for use just over a year before many provisions of the Affordable Care Act (ACA) came into effect. The ACA may have led to expansions in health insurance availability that differed by sexual orientation and may also have led to changes in labor supply and demand for MSM that could affect earnings but are unrelated to PrEP. I cannot directly refute such alternate explanations for my findings. I can, however, point to the fact that men in same-sex couples under 40 in my sample experience larger reductions in earnings compared to older men in same-sex couples. I also find larger effects on labor market outcomes for white men, who are much more likely to be aware of and taking PrEP. It would be surprising if the ACA and/or the Great Recession disproportionately affected exactly those subgroups of gay men who are also most likely to be taking PrEP. Further, I find no evidence of a similar effect on earnings or employment for females in same-sex couples, which again eases concerns that the ACA and/or the Great Recession explains my findings. It also suggests that changing attitudes regarding sexual orientation over the sample period, including changes in the legal status of marriage for same-sex couples (Sansone, 2019), are unlikely to be driving my estimates.

Like other similar work on the effect of changes in the cost of providing ESI for various groups (Gruber, 1994; Lahey, 2012), it is challenging to identify the mechanisms underlying the observed empirical effects. For example, it is unlikely that men in same-sex couples experienced nominal reductions in earnings after the advent of PrEP. The effects are, instead, likely to be the outcome of several complementary changes

Table 5
OLS sensitivity analyses.

	(1) Employed	(2) Has ESI	(3) Part-time	(4) Annual earnings
Panel A — State Level Clustering				
MSC × After 2012	−0.013***	−0.037***	0.008**	−2,650***
	(0.003)	(0.005)	(0.003)	(706)
Observations	5,242,129	4,659,439	4,659,439	3,652,016
Panel B — No Weights				
MSC × After 2012	−0.016***	−0.040***	0.006***	−3,178***
	(0.002)	(0.003)	(0.002)	(355)
Observations	5,242,129	4,659,439	4,659,439	3,652,016
Panel C — 2010 to 2016 Sample				
MSC × After 2012	−0.010***	−0.032***	0.009***	−2,071***
	(0.002)	(0.004)	(0.002)	(431)
Observations	3,314,988	2,926,353	2,926,353	2,278,444
Panel D — Private Sector Workers Only				
MSC × After 2012	−0.012***	−0.040***	0.006***	−2,899***
	(0.002)	(0.004)	(0.002)	(393)
Observations	4,023,098	3,815,315	3,815,315	2,999,362
Panel E — Full-time Workers Only				
MSC × After 2012				−3,013***
				(366)
Observations				3,552,853
Panel F — Industry-by-Location-by-Year FEs				
MSC × After 2012	−0.010***	−0.038***	0.004**	−2,306***
	(0.002)	(0.004)	(0.002)	(426)
Observations	5,242,129	4,659,439	4,659,439	3,652,016
Demographic Controls	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes

Data: 2009 to 2019 ACS restricted to men age 18 to 64 in a same- or different-sex couple and who are in the labor force. In columns (2) and (3), the sample is further restricted to males who are employed. In the final column, the sample is restricted only to men who are covered by ESI. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Demographic Controls include race, education, age, marital status, number of children, student status, disability status, citizenship status, and main language spoken. Fixed effects include state of residence, year, state-by-year, PUMA, metro status (i.e., urban, suburban, rural), industry, and occupation. MSC = Male in a same-sex couple.

including (1) relative reductions in earnings (i.e., nominal increases for some workers but not others), (2) men in same-sex couples not being offered jobs they would otherwise have been selected for, (3) having to accept lower wage offers than they would otherwise be offered when they do switch jobs, and (4) perhaps longer periods of unemployment leading to lower reservation wages. Further, to the extent that PrEP is valued by gay men, that would also reduce their reservation wage for jobs that offer ESI as an employment benefit. While I do not have the data necessary to shed much light on the relative importance of each of the mechanisms that could be at work here, by showing that ESI has potentially large effects on labor market outcomes for men in same-sex couples, my work provides a novel (and complementary) explanation for historical differences in earnings for MSM. In future work, it may be possible to relate changes in HIV prevalence, and in the cost and effectiveness of HIV *treatment*, to changes in earnings for MSM over time.

A further significant contribution of my work is that I show how the cost of new pharmaceuticals targeted towards specific populations can have negative effects on labor market outcomes (via ESI) for those most likely to take the drug. When those negative effects happen to be concentrated among workers who have also historically experienced differential treatment in the labor market, it naturally leads to concerns regarding the welfare effects and the equitable nature of providing health insurance as an employment benefit. More work is needed to help us understand the empirical regularities associated with expensive new pharmaceuticals.

Finally, it is worth mentioning that the FDA approved a "generic" form of PrEP that initially cost about 20 percent less than Truvada in 2020.[16] However, further generic competition has reduced the price of generic PrEP to about $1 per day, although quarterly clinic visits and STI testing remain additional costs. To the extent that individuals switch from the "brand name" version to generic brands, this should reduce the cost of PrEP for employers. On the other hand, at the end of 2021, the FDA approved "Apretude", which is an injectible form of PrEP administered bi-monthly by a doctor. There is no generic injectible version of PrEP and Apretude costs $3,700 per dose.[17] Combined with the fact that PrEP must be covered with no cost-sharing given its preventative nature, it is perhaps more likely that Truvada users would switch to bi-monthly Apretude injections, rather than generic daily-pill formulations. If so, there will be little to no change in the cost of providing PrEP via ESI for the foreseeable future.

[16] See https://www.poz.com/article/first-generic-truvada-now-available-united-states.

[17] See https://www.nbcnews.com/nbc-out/out-health-and-wellness/fda-approves-first-injectable-hiv-prevention-drug-rcna9426.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix. Additional estimates

A.1. Estimates of effect of ESI on men in same-sex couples

To examine whether ESI leads to diminished labor market outcomes for men in same-sex couples, I use the advent of an expensive HIV-prevention drug (Truvada) to aid identification. In contrast, earlier work on the effect of ESI on outcomes for certain groups often employs a difference-in-difference approach that compares earnings across groups by ESI status. Bhattacharya and Bundorf (2009) and Cowan and Schwab (2011) use this type of approach to study how ESI affects earnings for obese workers and smokers. In this appendix section, I use a similar approach comparing earnings for men in same-sex couples to those of men in different-sex couples with and without ESI. When using this approach to look at how ESI affects gay men, the estimating equation is;

$$Y_{it} = \alpha + \beta_1 MSC_{it} + \beta_2 ESI_{it} + \beta_3 ESI_{it} \times MSC_{it} + X_{it}\Pi + \epsilon_{it}. \quad \text{(A.1)}$$

In Eq. (A.1), Y_{it} refers to an outcome of interest for individual i in time period t. Again, the MSC_{it} term is an indicator variable that equals one for men who report that they live with or are married to a same-sex partner and is zero otherwise. The ESI_{it} indicator term equals one if individual i reports that they have ESI at time t and is zero otherwise. These indicator variables control for differences in outcomes for men in same-sex couples that persist regardless of ESI status and for differences in outcomes for those who have ESI versus those who do not. The coefficient on the interaction of the indicator terms therefore represents the difference between outcomes for men in same-sex couples versus men in different-sex couples whenever they are offered ESI. Because men in same-sex couples have significantly greater medical expenditures, and because ESI is experience rated, we would expect $\hat{\beta}_3 < 0$, all else being equal. Completing the estimating equation, I include an idiosyncratic error term, ϵ_{it}, along with controls for demographic characteristics and fixed effects, X_{it}.

In Table A.1, I present estimates where I use Eq. (A.1) to examine how ESI and earnings are related for men in same-sex couples, relative to men in different-sex couples. In columns (1) and (2) of the table, before examining whether men in same-sex couples who have ESI experience lower earnings because of their greater medical expenditures, I present estimates of the overall difference in earnings for men in same-sex couples, regardless of ESI status. In particular, in the first column, I include demographic controls only (age, education, race, etc.). I then add state, metro location (urban, suburban, rural), industry, and occupation fixed effects to produce the estimates in the second column. In all specifications, I use OLS estimation and ACS-provided weights. In a specification that includes demographic controls and fixed effects, I find that men in same-sex couples earn $923 *more* than other men on an annual basis, statistically significant at the 1% level. We can think of this as an estimate of the conditional difference in average earnings. Looking at the likelihood of having ESI (via a linear probability model) in columns (3) and (4), I find that men in same-sex couples are about 5.3 percentage points more likely to have ESI.

In columns (5) and (6), I provide estimates of the effect of ESI on earnings for men in same-sex couples. As with the other outcomes in the table, I first include only demographic controls and then add fixed effects. The estimates suggest that men in same-sex couples earn $1,268 less per year than other men and that all men with ESI earn considerably more ($10,690) than those without ESI. The coefficient on the interaction term then represents the difference in earnings for men in same-sex couples relative to other men when they have ESI relative to the same difference when they do not have ESI. This approach identifies the effect of ESI on differences in earnings under an assumption that the only way ESI affects earnings is via the cost wedge it introduces between workers with different medical expenditures. Illustrating that such an identifying assumption is unlikely to be valid, I find a large *positive* effect of ESI on earnings for men in same-sex couples, despite that group having several thousand dollars greater annual medical expenditures (Gavulic and Gonzales, 2022). In a specification with controls and fixed effects, my estimates suggest that – relative to the gap in earnings between men in same-sex and different-sex couples when they do not have ESI – men in same-sex couples experience a $2,056 wage premium relative to men in different-sex couples, and that effect is statistically significant at the 1% level. These estimates highlight that this empirical approach, one that is relatively common in related literature, is unlikely to identify the effect of ESI on earnings among groups of workers with different medical expenditures.

One plausible explanation for such a counter-intuitive finding is that there are positive correlations among ESI, wages, and firm size. The correlation between wages and firm size (see Oi and Idson, 1999) is particularly problematic in this setting. Essentially, larger firms can allow for greater specialization, potentially increasing the earnings gap between any two workers with different levels of productivity. To see the issue this creates, note that larger firms are more likely to offer ESI (Buchmueller and Monheit, 2009; Lennon, 2021b) and my estimates suggest comparable workers earn $10,690 more per year when they work somewhere that offers ESI. There is no reason to think that ESI causes those greater earnings. Further, in Table 1, I show that men in same-sex couples are much more likely to have a college education or greater. Even though education is not a perfect measure of productivity, we would, therefore, expect men in same-sex couples to earn more at larger firms, on average. However, because larger firms are also much more likely to offer ESI, this pattern leads to a positive correlation between the earnings of men in same-sex couples and ESI, regardless of differences in medical expenditures.

Whatever the explanation, avoiding this issue requires a source of exogenous variation that changes the cost of providing ESI for a particular group of workers (as in Gruber, 1994, for example). When looking at how ESI affects men in same-sex couples, the advent of PrEP provides the necessary variation. While workers may pay some of its cost via cost-sharing, most of the cost is borne by employers via experience-rated insurance plans (i.e., providing ESI is more expensive for the firm if their workers are costlier to cover), which is why I focus on estimating the effect of ESI on men in same-sex couples using the advent of Truvada.

A.2. Event studies for sample restricted to females

In Fig. A.1, I present event study plots (using the same estimating equation that I present in Section 4.1 in the body of the paper) where I limit my sample only to females in couples. I examine the same earnings and employment outcomes as in Fig. 1 in Section 4 of the paper, but now the estimates compare outcomes for females in same-sex couples to other females.

In these event studies, I find no evidence of any post-PrEP effects on earnings and employment. These event study analyses therefore illustrate that, when looking at outcomes for men in same-sex couples, it is not likely that changes in attitudes regarding sexual orientation or changes relating to same-sex marriage laws could explain my findings. If those kinds of changes were driving my findings, they ought to have qualitatively similar effects for women in same-sex couples. Instead, I only observe effects for men in same-sex couples, and only after 2012, strongly supporting the idea that the advent of PrEP is causing the effects that I report in Section 4.

Table A.1
ESI and MSC men's labor market outcomes using standard approach.

	(1) Emp. Income	(2) Emp. Income	(3) ESI	(4) ESI	(5) Emp. Income	(6) Emp. Income
MSC	2,443***	923***	0.053***	0.053***	−1,184***	−1,268***
	(165)	(145)	(0.002)	(0.002)	(351)	(307)
ESI					16,031***	10,690***
					(44)	(41)
MSC × ESI					3,517***	2,056***
					(393)	(344)
Observations	4,659,439	4,659,439	4,659,439	4,659,439	4,659,439	4,659,439
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes

Data: 2009 to 2019 ACS restricted to men age 18 to 64 in a same- or different-sex couple and who are employed. Standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. Demographic Controls include race, education, age, marital status, number of children, student status, disability status, citizenship status, and language spoken. Fixed effects include state of residence, year, state-by-year, PUMA, metro status (i.e., urban, suburban, rural), industry, and occupation. MSC = Male in a same-sex couple.

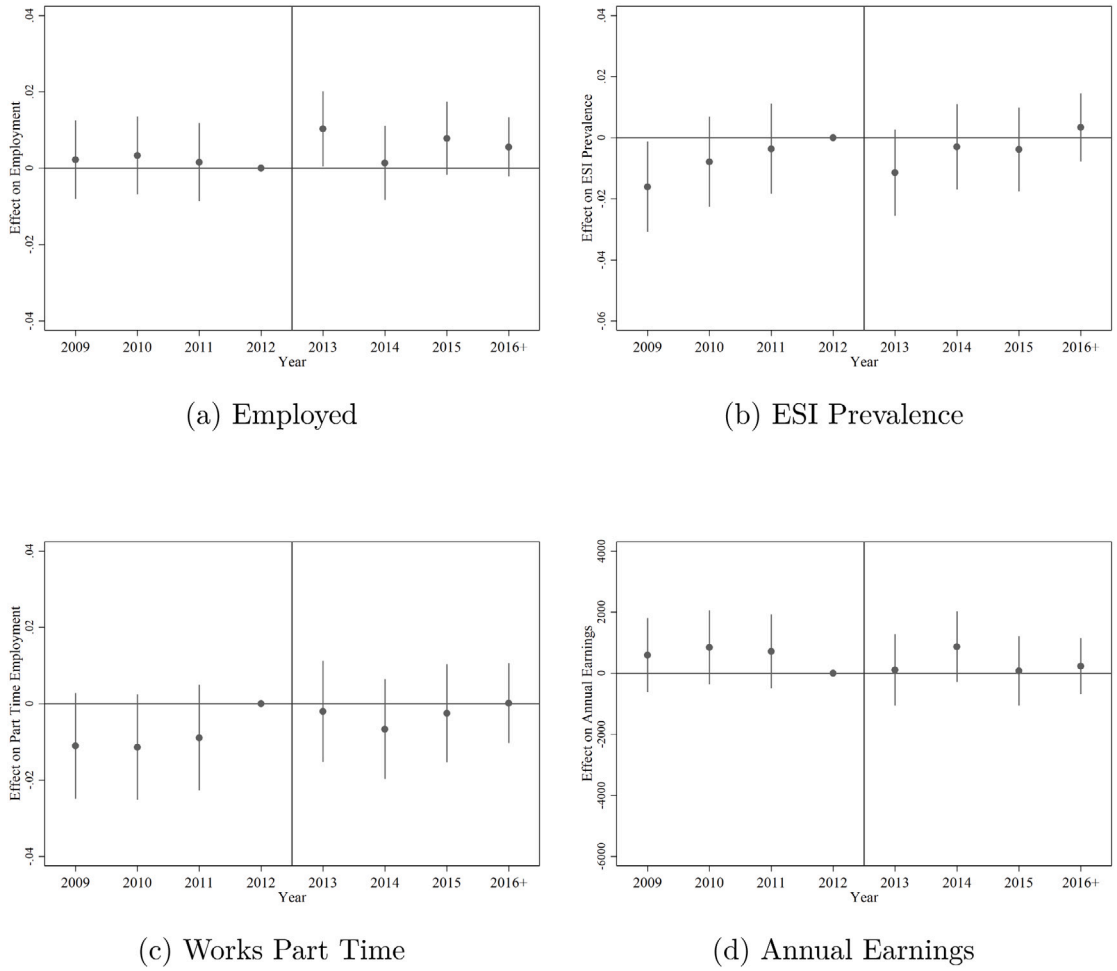

Fig. A.1. Event Studies for *Female* Labor Market Outcomes.
Notes: Each plot represents an event study where the sample is restricted to female ACS respondents age 18 to 64 in the labor force and in a same- or different-sex couple. In subfigures (b) and (c) the sample is further restricted to those are currently employed and in subfigure (d) the sample consists only of workers with ESI. The year 2012 is the "omitted" category. The dependent variable is noted below the related figure. Vertical bars represent 95% confidence intervals. See Section 4 for more details on these event study specifications.

A.3. Estimates using men in couples without ESI

I explain in the main text that men in same-sex couples who do not have ESI from an employer could be another valid comparison/placebo group. I also explain, however, that this group may be disproportionately affected by the ACA's changes to Medicaid eligibility and the establishment of the healthcare exchanges, where individuals can purchase a non-group health insurance plan. For example, average earnings for men in same-sex couples without ESI are just $30,079 in my sample whereas they are $67,424 among those with ESI. Further, there is significant potential for a SUTVA (Stable Unit Treatment Values Assumption) violation, with potential spillovers from the effect of

Table A.2
OLS estimates focusing on employed men in same-sex couples without ESI.

	(1) Employed	(2) Has ESI	(3) Part-time	(4) Annual earnings
MSC			0.031***	−3,216***
			(0.005)	(518)
MSC × After 2012			0.007	−850
			(0.006)	(610)
Observations	–	–	993,545	993,545

Data: 2009 to 2019 ACS restricted to men age 18 to 64 in a same-sex couple and who are in the labor force who do not have ESI. *** p < 0.01, ** p < 0.05, * p < 0.1. Both specifications include demographic controls (race, education, age, number of children, student status, disability status, citizenship status, and main language spoken) and fixed effects (state of residence, year, state-by-year, PUMA, metro status, industry, and occupation). MSC = Male in a same-sex couple.

Table A.3
OLS estimates focusing on the advent of PrEP using all males in the sample.

	(1) Employed	(2) Has ESI	(3) Part-time	(4) Annual earnings
MSC	0.026***	0.074***	−0.031***	4,412***
	(0.002)	(0.003)	(0.002)	(293)
MSC × After 2012	−0.024***	−0.043***	0.015***	−2,491***
	(0.002)	(0.004)	(0.002)	(342)
Observations	7,839,206	6,820,977	6,820,977	5,076,197
Demographic Controls	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes

Data: 2009 to 2019 ACS restricted to men age 18 to 64 who are in the labor force. In columns (2) to (3), the sample is further restricted to males who are employed. In the final column, the sample is restricted only to men who are covered by ESI. Standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. Demographic Controls include race, education, age, marital status, number of children, student status, disability status, citizenship status, and main language spoken. Fixed effects include state of residence, year, state-by-year, PUMA, metro status (i.e., urban, suburban, rural), industry, and occupation. MSC = Male in a same-sex couple.

Table A.4
OLS estimates adding controls for same-sex marriage legalization by state.

	(1) Employed	(2) Has ESI	(3) Part-time	(4) Annual earnings
MSC	0.017***	0.081***	−0.006***	3,202***
	(0.001)	(0.003)	(0.001)	(275)
MSC × After 2012	−0.015***	−0.037***	0.008***	−2,937***
	(0.002)	(0.003)	(0.002)	(330)
Observations	5,242,129	4,659,439	4,659,439	3,652,016
Demographic Controls	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes

Data: 2009 to 2019 ACS restricted to men age 18 to 64 who are in the labor force and in a same-sex couple. In columns (2) and (3), the sample is further restricted to males who are employed. In the final column, the sample is restricted only to men who are covered by ESI. Standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. Demographic Controls include race, education, age, marital status, number of children, student status, disability status, citizenship status, and main language spoken. Fixed effects include state of residence, year, state-by-year, PUMA, metro status (i.e., urban, suburban, rural), industry, and occupation. In each specification in this table, I also control for whether same-sex marriage was legal at the time. MSC = Male in a same-sex couple.

PrEP/Truvada wherever ESI is available. For example, if employers who offer ESI reduce demand for men in same-sex couples after the advent of PrEP, then men in same-sex couples have fewer labor market options overall, potentially limiting career progression, on-the-job learning, and altering their reservation wage, leading to diminished outcomes among workers at firms without ESI.

For completeness, in Table A.2 I present estimates for changes in part-time work and annual earnings for men in same-sex couples, relative to men in different-sex couples but limiting the sample to workers without ESI, using the same estimating equation as I use for Table 2 in Section 4 of the paper. With the sample limited to working men without ESI, there are no estimates in columns (1) and (2), as those focus on employment status conditional on being in the labor force and ESI status conditional on working. In columns (3) and (4), I find a statistically insignificant 0.7 percentage point increase in part-time employment and a statistically insignificant $850 decrease in earnings among men in same-sex couples who are working and do not have ESI. This pattern could be related to mild "spillover" effects or to the effects of the ACA (perhaps via changes in labor supply decisions), but it is beyond the scope of this paper to investigate further.

A.4. Estimates including men who are "single"

In the main text, I present estimates limited to samples including only those part of a married or cohabiting couple. In Table A.3, I present estimates corresponding to my preferred specification in Table 2 but where I re-introduce "single" men (i.e., men who are unmarried or non-cohabiting). Naturally, as they are not part of a same-sex couple, they are considered as part of the comparison group along with men in different-sex couples. This is problematic because I

cannot identify unmarried and non-cohabiting MSM. Instead of being part of the treatment group, these MSM are therefore included in the comparison group. Helpfully, estimates are quite similar despite these data limitations.

A.5. Estimates controlling for legalization of same-sex marriage

In Table 4 (in the main text), I provide estimates that examine labor market outcomes for females in same-sex couples versus females in different-sex couples after the advent of Truvada. Those estimates indicate that there are no statistically significant negative effects on labor market outcomes for females in same-sex couples after the advent of Truvada. This is as expected given that females are unlikely to use Truvada. Furthermore, if it were the case that my main findings (Table 2) regarding labor market outcomes for men in same-sex couples were driven by changes in same-sex marriage laws, then we might expect to see similar effects for females in same-sex couples. Instead, those estimates point towards my findings being related to the change in cost of employing MSM after the advent of Truvada.

To further illustrate that my findings are not related to changes in same-sex marriage laws, in Table A.4 I present estimates where I control for whether same-sex marriage is legal in a state in a given year using an indicator variable that equals 1 if same sex marriage is legal in a given state for all of year t and then set the indicator variable to zero otherwise.[18] If anything, my estimates now suggest a slightly larger negative effect on earnings after the advent of Truvada. However, it is difficult to draw any firm conclusions as the 95% confidence intervals overlap considerably.

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[18] My data on same sex marriage legalization dates comes from https://gaymarriage.procon.org/state-by-state-history-of-banning-and-legalizing-gay-marriage/.