Employer-sponsored Health Insurance and Labor Market Outcomes for Gay Men: 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." All else equal, experience rating increases the cost of employing workers who have greater medical expenditures, such as gay men. To study whether ESI affects labor market outcomes for gay men, 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 $4,665 (7.6%) relative to otherwise comparable men after PrEP becomes available. Event study analyses support a causal interpretation. I also observe statistically significant post-PrEP declines in hours worked and in the probability of being employed for gay men. My findings are robust to placebo analyses, various specification permutations, and a range of sensitivity checks. 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.

Keywords: ESI, PrEP, Truvada, Gay Men, HIV, HIV Prevention, Earnings, Employment

JEL: J31, J32, J33, I13, J71

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1 Introduction

In 2012, the United States Food and Drug Administration approved "Truvada" as a form of Pre-Exposure Prophylaxis (PrEP) to help prevent Human Immunodeficiency Virus (HIV) acquisition. Despite a $24,000 per year price tag, Truvada's manufacturer reported that, as of May 2019, "approximately 200,000 of the estimated 1.1 million Americans who are at risk for HIV currently receive Truvada."[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 hire workers who are less likely to use PrEP, unless wages are free to adjust for its expected cost (Summers, 1989).

To estimate how the costs associated with PrEP/Truvada affect gay men's labor market outcomes, I rely on demographic information, including using same-sex marriage and cohabiting partner status to identify gay 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, I find that annual earnings for men in same-sex couples with ESI decline by $4,665 after the advent of PrEP, relative to other men. While it is unclear how having health insurance affects the likelihood of PrEP usage, these estimates appear to align well with the expected cost of providing PrEP for employers given that about 18.2% of at-risk men use the drug at an annual cost of $24,000.[2] Providing immediate support for a causal interpretation, an event study analysis focusing on differences in earnings shows no evidence of problematic pre-trends. Formally, my approach identifies the effect of PrEP on labor market outcomes for gay men so long as there are no omitted idiosyncratic shocks that are correlated with PrEP's approval and labor market outcomes for gay men.

Further supporting a causal interpretation, I find that gay men aged 18 to 40 experience relatively larger declines in earnings compared to older gay men. Such a pattern is consistent with the fact that PrEP is prescribed mainly to those aged 40 and younger.[3] Similarly, I find that white gay

[1] See https://www.nbcnews.com/feature/nbc-out/prep-use-jumps-35-percent-among-gay-bi-men-risk-n980516 and https://www.gilead.com/news-and-press/press-room/press-releases/2019/5/gilead-sciences-to-provide-free-truvada-for-prep-to-support-us-initiative-to-end-the-hiv-epidemic.

[2] The proportion of men who take Truvada among those with ESI could be more than 18.2%. On the other hand, workers tend to pay for some of their own care via employee contributions to premiums and cost-sharing.

[3] In 2016, two-thirds of PrEP users were age 25 to 44, with a further 11% being under 25. See https://www.aidsmap.com/news/mar-2018/prep-use-growing-us-not-reaching-all-those-need.

men experience larger declines in earnings, consistent with white gay men being more likely to be aware of and taking PrEP (Kanny et al., 2019). When looking at employment outcomes, I find suggestive evidence of post-PrEP declines in the likelihood of being employed and hours worked, consistent with the idea that the cost of providing PrEP reduces employer demand for the labor of gay men.[4] Further, when focusing on females as a placebo analysis, I find no comparable effects for females in same-sex couples, which helps to ease concerns that changes in attitudes and same-sex marriage laws can explain the effects I observe among gay men. My findings are also robust to different approaches to weighting, clustering, and sample selection.

One significant limitation, however, is that I cannot identify gay men who are not part of a couple (i.e., those who are gay but not married or cohabiting with a same-sex partner) in my ACS data. To the extent that PrEP use is less common among men in same-sex couples, my estimates would represent only a lower bound on the true effect of PrEP on gay men's labor market outcomes. On the other hand, many couples are "serodifferent," meaning one partner is HIV positive and the other is negative. PrEP is highly recommended in such cases.[5] Further, to the extent that employers can determine sexual orientation only imperfectly, gay men who are not in a same-sex couple may be harder to identify for employers and therefore might experience only limited effects on labor market outcomes after PrEP becomes available.

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. 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; and Carpenter, 2004, 2007). In particular, my findings suggest that changes in the prevalence of and treatment costs associated with HIV/AIDS over time could help to explain differences in the earnings of gay men relative to other men.[6] Finally, I provide evidence that newly-available pharmaceuticals can lead to lower earnings

[4]Finding a negative effect on hours worked is reasonable in this setting because, under Affordable Care Act (ACA) rules, those working fewer than 30 hours per week do not have to be offered health coverage. Without being able to avoid to cost of ESI in this way, an increase in the cost of an employment benefit would usually favor having fewer workers combined with an increase in hours worked per worker.

[5]The Centers for Disease Control estimates that 740,400 gay and bisexual men were living with HIV in 2018. See https://www.cdc.gov/hiv/group/msm/index.html.

[6]Gavulic and Gonzales (2021), 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 just $3,994 per year. The prevalence of and cost of treatment for Human Immunodeficiency Virus (HIV) potentially explains some of these medical expenditure differences (Hess et al., 2017; McCann et al., 2020). Notably, Gavulic and Gonzales (2021) find no similar difference among females in same-sex couples. Note

for particular groups if they increase the expected cost of providing ESI for that group. Given recent Food and Drug Administration (FDA) approvals of increasingly-expensive pharmaceuticals (such as the controversial recent approval of "Aduhelm" for early stage Alzheimer's disease), 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 opportunities and wages.[7] 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).

In Section 2, I provide background information on Truvada/PrEP. I also expand on how my findings complement existing work on labor market outcomes for gay men. In Section 3, I explain my ACS data, empirical strategy, and approach to estimation. In Section 4, I present my 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.[8] The drug is prophylactic in the sense that it reduces HIV risk by blocking a specific enzyme that the virus needs to reproduce itself.[9] Users are supposed to take Truvada every day at the same time and it provides a 92% to 99% reduction in HIV risk for those who use the drug as directed (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.[10] Of course, insurance plans help

that to the extent that Truvada reduced the expected cost of HIV care among gay men, it would bias my empirical estimates towards zero. Given only a small fraction of men who take PrEP would likely otherwise contract HIV in my sample period, I can ignore any potential reductions in medical expenditures due to changes in HIV prevalence.

[7] See https://www.theatlantic.com/health/archive/2021/07/americas-drug-approval-system-unsustainable/619422/.

[8] 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/. In 2020, the FDA approved a generic form of PrEP that appears to cost about 20 percent less than Truvada. See https://www.poz.com/article/first-generic-truvada-now-available-united-states.

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

[10] 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.

individuals pay that cost.[11] However, because of experience rating, for firms that offer ESI as an employment benefit, the advent of PrEP makes gay men more expensive to employ, on average.

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. However, 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).[12]

Because employers ultimately foot the bill for employee healthcare expenditures, studies show that medical expenditure differences among groups of workers are then 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).[13] On one hand, this seems 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

[11] Gilead, Truvada's manufacturer offers varying levels of assistance towards cost-sharing, but that only makes it more likely that individuals will take PrEP, increasing costs for employers.

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

[13] As I mention in the introductory section, 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 these kinds of approaches 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 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.

expenditures. For that reason, I use the advent of Truvada/PrEP to help us learn about how ESI might matter for gay men's labor market outcomes.

Given such a focus, my findings also naturally contribute to a well-established literature that studies explanations for differences in labor market outcomes for gay men and lesbian women. 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. Other explanations 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).[14]

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.[15] 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.

I contribute to this literature by providing novel evidence regarding a different potential source of variation in labor market outcomes for gay men. By showing that the advent of PrEP leads to lower earnings for gay men, my work highlights that ESI can play an important role in gay men's earnings. Future work might find it valuable to examine how the earnings penalty for gay men over time changed in response to changes in HIV prevalence and treatment costs.

[14] Badgett et al. (2021) provide a broad overview of "LGBTQ Economics" in their recent article of the same name.

[15] Jepsen (2007) and Klawitter (2015), however, show that lesbian females earn more than comparable non-lesbian females.

3 Data and Estimation

I examine how ESI affects gay men's labor market outcomes using data 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 categorical variable that identifies same-sex unmarried partners, helping me to identify close to 65,000 men in a same-sex couple and in the labor force. Naturally, that means that, whenever I refer to the effect of PrEP on "gay" men's labor market outcomes, I am referring only to an observed effect for men in same-sex couples. 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.

I present summary statistics, stratified by sexual orientation (again, to be specific, men in same-sex couples versus all other men), for my main estimation sample in Table 1. Because my estimates focus on comparing labor market outcomes among men by sexual orientation, the estimation sample is restricted to men aged 18 to 64 who are in the labor force. That restriction leaves me with approximately 7.2 million respondents. Of men age 18 to 64 who are in the labor force, 6.9 million reported that they were working at the time they were surveyed. When looking at outcomes that depend on being employed (ESI status, for example), the sample is naturally restricted to only respondents who report that they are working. I also exclude respondents who claim they are currently working but report earnings of less than $1 in the past twelve months. All dollar values are in 2017 dollars, adjusted using annual CPI index values from the U.S. Federal Reserve website. Because the ACS top-codes earnings in the top one half-percentile by year, I also exclude respondents with earnings greater than $300,000 in 2017 dollars, using a single cut-off to maintain comparability across years. A little less than 1% of the remaining respondents are men in same-sex couples. Of course, it must be noted that many gay men are not in a cohabiting same-sex couple. Unfortunately, I cannot identify those "single" gay men in my sample and they are necessarily grouped with "All Other Men." As I mention in the introductory section, the effect of that limitation is less clear than one might suspect. In particular, gay men who are not in couples may be harder to identify

for employers. Moreover, many gay couples are serodifferent, ensuring that PrEP use is relatively common among couples.

Looking at the summary statistics, we can see that men in same-sex couples have greater earnings and educational attainment than other men. They are also more likely to be white, less likely to be married, are younger, and are less likely to be a student. 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.

Later, I examine earnings for men who have ESI before and after the advent of PrEP in 2012. For that reason, I also provide earnings and ESI information for the 2009 to 2012 and 2013 to 2019 periods separately. Broadly speaking, earnings increase for all men after 2012. However, the increase in earnings for men in same-sex couples is considerably smaller than for other men, which suggests that gay men did not fare quite as well in the labor market after 2012. Note that given the sample size, even relatively small differences among groups are statistically significant. In the next subsection, I describe my approach to examining whether the advent of Trvuada was responsible for differences in gay men's labor market outcomes after 2012.

Table 1: Summary Statistics

	All Other Men	Men in Same-Sex Couples	Total
Annual Earnings from Employment	$ 53,015	$ 61,058	$ 53,088
Earnings 2009 - 2012	$ 51,446	$ 60,309	$ 51,526
Earnings 2013 - 2019	$ 53,887	$ 61,338	$ 53,955
Has Health Insurance (ESI) from Employer	71.7%	77.3%	71.8%
ESI 2009 - 2012	70.6%	76.9%	70.7%
ESI 2013 - 2019	72.3%	77.5%	72.4%
Usual hours worked per week	41.6	41.3	41.6
Education (Highest Level Completed)			
Less than High School	9.6%	4.4%	9.6%
High School	58.8%	45.7%	58.7%
College	20.5%	29.0%	20.6%
Graduate	11.1%	20.9%	11.2%
Race			
White	79.0%	82.9%	79.0%
Black	8.2%	5.4%	8.2%
Other	12.8%	11.7%	12.8%
Married	58.7%	33.7%	58.5%
Age	42.6	41.6	42.6
Student Status	9.2%	8.8%	9.1%
Observations	7,124,236	64,958	7,189,194

Source: 2009 to 2019 data from the American Community Survey, men age 18 to 64 in the labor force. Of men age 18 to 64 who are in the labor force, 6,886,619 reported that they were working at the time they were surveyed. When looking at earnings from employment, ESI status, and usual hours worked the sample is naturally restricted to only the 6,886,619 respondents who report that they are working. All dollar values are in 2017 dollars, adjusted using annual CPI data from the U.S. Federal Reserve. Because the ACS top-codes earnings in the top one half-percentile by year, I exclude all respondents with earnings greater than $300,000 in 2017 dollars, using a single cut-off to maintain comparability across years.

3.1 Estimation

When studying the effect of PrEP ("Truvada") on gay men's labor market outcomes my estimating equation is as follows:

$$Y_{it} = \kappa + \phi_1 Gay_{it} + \phi_2 After\ PrEP_t + \phi_3 After\ PrEP_{it} \times Gay_{it} + X_{it}\Pi + \epsilon_{it}. \quad (1)$$

In Equation (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 Gay_{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 zero otherwise. The $After\ PrEP_t$ indicator term equals one whenever $t > 2012$ and zero otherwise. The coefficient on the interaction of the indicator terms therefore represents the difference between outcomes for gay men versus other men, after PrEP becomes available. Because PrEP costs $24,000, 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 equations above to examine how ESI affects gay men. When looking at the effect of PrEP on outcomes for gay men, I limit my sample only to male ACS respondents who have ESI. Further, I include "single" men, by which I mean those not part of a married or cohabiting couple, in the comparison group in my main estimates. I present analyses using a sample limited to men in cohabiting couples, 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. Further, I show that such an 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, that group might 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 where I

leverage those who do not have ESI as a comparison group only as an additional appendix item.[16] Finally, because I use a two group (men in same-sex couples versus other men), two-period (before versus after PrEP) difference-in-difference approach, with a single treatment date, my estimates are not subject to the issues raised by the new difference-in-difference literature (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2020; Goodman-Bacon, 2021).

4 Main Findings

In Table 2, I examine how the advent of PrEP affected men in same-sex couples, using OLS to estimate the coefficients from the specification described in Equation (1) in Section 3. Because the effects of PrEP could manifest as lower earnings or reduced employment (either on the intensive or extensive margin), I examine annual earnings, usual hours worked, and the probability of being employed. I present estimates with just demographic controls and then with demographic controls and fixed effects for each of my three outcomes. I use ACS-provided weights and cluster standard errors at the Public-use Microdata Area (PUMA) level, even though it is perhaps overly-conservative.[17] I present estimates both without clustering and with state-level clustering in later sensitivity checks. Because the outcome is an indicator, the coefficients in columns (5) and (6) are from a linear probability model and should be interpreted as percentage point changes.[18]

I find that the advent of PrEP is associated with lower earnings and reduced employment for men in same-sex couples. Looking at earnings, I find a $4,665 relative decline after 2012 for men in same-sex couples in my preferred specification (one that includes demographic controls and location, occupation, and industry fixed effects). The estimate is statistically significant at the 1% level. Given mean annual earnings of just over $61,000 for men in same-sex couples, the effect amounts to a 7.6% relative decline for men in same-sex couples compared to other men. If Truvada costs

[16] In particular, because of the ACA, firms without ESI but with more than 50 employees had to begin offering ESI in 2015 (100+ employees) or 2016 (50+ employees) under the employer mandate's provisions. It is possible that firms without ESI prior to 2015 therefore also respond to the high cost of providing Truvada (and gay men's medical expenditures, more generally) by demanding relatively fewer gay men as workers. Without being able to control for firm size, I cannot explore this potential issue in greater detail. However, it should be clear that men without ESI are likely not a valuable comparison group during this time period.

[17] 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. I cluster at the PUMA level to try to account for PrEP use/awareness potentially being (serially) correlated *within* particular communities.

[18] Technically, $\hat{\phi} \times 100$ converts the estimates to percentage point terms.

employers about $24,000 per year, a $4,665 decline in annual earnings would be be the expected wage offset if about 19% of men in same-sex couples in my sample were PrEP users. In Figure 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.[19] I discuss my event study analyses further in the next subsection.

When looking at intensive and extensive margin employment measures, I find a relative decline of just under one hour per week in hours worked and a 1.6 percentage point decline in the probability of being employed in a specification with demographic controls and fixed effects. The point estimates are statistically significant at the 1% level. While an increase in the cost of providing some employment benefit might favor having workers work more hours, finding a negative effect on hours worked is reasonable in this setting because, under Affordable Care Act (ACA) rules, those working fewer than 30 hours per week do not have to be offered health coverage. However, event study analyses looking at these outcomes provide only limited support for the parallel trends assumption that is inherent in my difference-in-difference approach.

[19] Given we know, in 2021, that Truvada usage took a couple of years to ramp up, we might expect the effect to take time to show up in labor market data. However, for employers in 2012, it would not have been clear that take up of this breakthrough drug would be relatively slow. Moreover, to the extent that employment is an ongoing relationship, then the advent of PrEP significantly and immediately changes the future stream of expected medical expenditures for gay employees, even if relatively few gay men actually used PrEP as early as 2013.

Table 2: Estimates Focusing on the Advent of PrEP

	(1) Emp. Income	(2) Emp. Income	(3) Usual Hours Worked	(4) Usual Hours Worked	(5) Employed	(6) Employed
Gay	8,205.84***	5,456.43***	0.97***	0.95***	0.022***	0.016***
	(580.45)	(463.77)	(0.11)	(0.11)	(0.002)	(0.002)
After PrEP	1,085.70***	862.46***	0.18***	0.20***	0.026***	0.024***
	(179.50)	(105.97)	(0.02)	(0.02)	(0.000)	(0.000)
Gay × After PrEP	-6,585.12***	-4,665.49***	-1.40***	-0.96***	-0.018***	-0.016***
	(611.24)	(531.17)	(0.14)	(0.13)	(0.002)	(0.002)
Observations	5,347,733	5,347,733	5,347,733	5,347,733	7,189,194	7,189,194
R-squared	0.29	0.46	0.11	0.22	0.020	0.037
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes

Data: 2009 to 2019 data from ACS restricted to men in the labor force and, in the first four columns, to men with ESI. Standard errors, clustered at the statistical area level (Public Use Microdata Area or PUMA), in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic Controls include race, education, age, marital status, and student status. Fixed effects include state of residence, PUMA, industry, and occupation.

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 Equation (1) in Section 3:

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

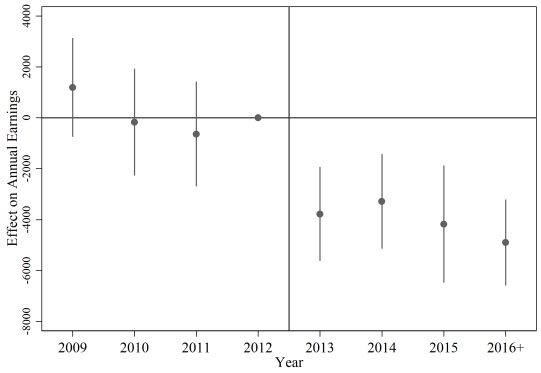
In Equation (2), the key difference versus Equation (1) is that I replace the indicator for "PrEP" with a set of indicators $1(t - T_i = k)$ interacted with the indicator for men in same-sex couples (Gay_{it}).[20] The indicator term equals 1 only for respondents in year t when it is k years away from 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 other men 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.[21] 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.

I present event study plots for my three outcomes of interest in Figure 1. There, I find relatively strong support for the parallel trends assumption for earnings and perhaps some support for hours worked. For employment, there appears to be a pre-trend that essentially continues after the advent of PrEP. Therefore, while PrEP may have had some effect on employment, it is not possible to make strong claims regarding causality. Speculating somewhat, it is possible that the advent of PrEP had effects on both labor demand and labor supply, particularly relative labor supply toward firms that offer ESI (because the cost of PrEP ensures that ESI is more valuable for gay men after 2012). It is best, therefore, to view my estimates as the net effect of the change in labor demand and supply relating to PrEP. However, that means that we might not expect to see a significant break from trend in employment even when we do see a clear effect on earnings.

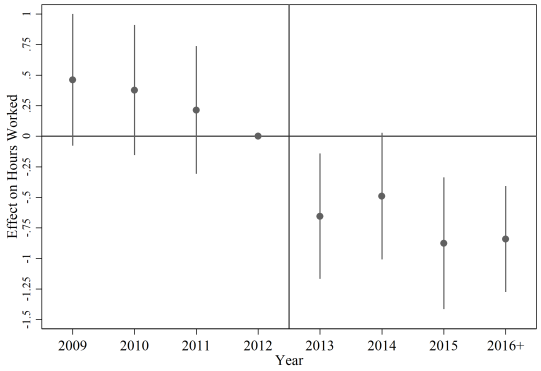
[20] The description and notation in my event study analysis borrows from Miller and Wherry (2019), Teltser et al. (2021), and Lennon (2021b).

[21] Sun and Abraham (2020) 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).

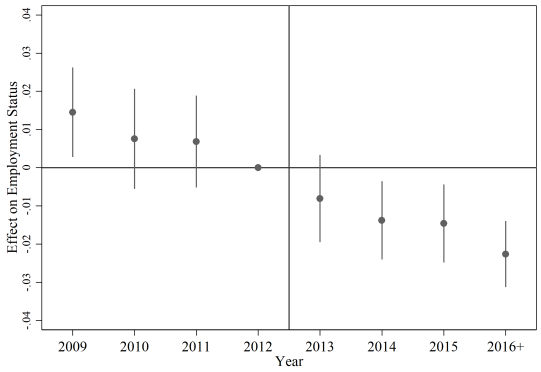
Figure 1: Event Studies for Main Outcomes



(a) Annual Income from Employment



(b) Usual Hours Worked Per Week



(c) Indicator for Employed (vs. Unemployed)

Notes: Each plot represents an event study where the sample is restricted to male ACS respondents age 18 to 64. The year 2012 is the "omitted" time period. The dependent variable is noted below the related figure. Bars represent 95% confidence intervals.

4.2 Heterogeneity, Sensitivity, Placebo Analyses

4.2.1 Heterogeneity by Age

In Table 3, I present estimates that show how outcomes for younger gay men changed compared to older gay men after the advent of PrEP. Specifically, I limit my sample to men in same-sex couples to isolate whether younger gay men experience greater effects on earnings and employment. Because younger males are more likely to be taking PrEP, we should find that negative effects on labor market outcomes are highly concentrated among gay men under 40, assuming all else equal. To produce the estimates in Table 3, I use an estimating equation similar to Equation (1) in Section 3:

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

Relative to Equation (1), in Equation (3) the Gay_{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 Equation (1) and I again use ACS-provided weights and cluster standard errors at the PUMA level.

Looking at earnings, in a specification with controls and fixed effects the estimates show that relative to older gay men, younger gay men experience a $2,426 decline in earnings after the advent of PrEP. That effect is, again, statistically significant at the 1% level. For context, my estimates in Table 2 suggested that gay men experience a $4,665 overall decline in earnings relative to other men. Because the sample is already restricted to a group that experiences a decline in earnings after 2012, the estimates in Table 3 imply that younger gay men experience more than a $4,665 relative decline in earnings. Naturally, that means that older gay men experience a smaller than $4,665 relative decline. Such a finding is consistent with the fact that younger males are significantly more likely to be taking PrEP. Providing more evidence on this issue, I present estimates comparing only younger gay men to non-gay young men as an appendix item.

Table 3: Estimates Focusing on the Effect of PrEP on Outcomes for Younger Gay Men

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp. Income	Emp. Income	Usual Hours Worked	Usual Hours Worked	Employed	Employed
Young (Age< 40)	-15,058.64***	-13,822.74***	0.39*	0.44**	-0.017***	-0.013***
	(967.53)	(895.61)	(0.21)	(0.20)	(0.005)	(0.004)
After PrEP	679.27	931.52	-0.06	-0.14	0.012***	0.011***
	(945.72)	(860.70)	(0.17)	(0.17)	(0.003)	(0.003)
Young × After PrEP	-3,247.80***	-2,426.29**	-0.65**	-0.46**	0.013***	0.012**
	(1,191.10)	(1,043.43)	(0.26)	(0.23)	(0.005)	(0.005)
Observations	51,025	51,025	51,025	51,025	64,958	64,958
R-squared	0.29	0.46	0.11	0.22	0.008	0.022
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes

Data: 2009 to 2019 data from ACS restricted to men in same-sex couples in the labor force and, in the first four columns, further restricted to men with ESI. Standard errors, clustered at the statistical area level (Public Use Microdata Area or PUMA), in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic Controls include race, education, age, marital status, and student status. Fixed effects include state of residence, PUMA, industry, and occupation.

4.2.2 Heterogeneity by Race

Kanny et al. (2019), using 2017 National HIV Behavioral Surveillance (NHBS) data, report that white men are much more likely to be aware of and taking PrEP. For example, among men who have sex with men, Kanny et al. report that 42% of white, 30% of Hispanic, and 26% of black men in urban areas report taking PrEP in 2017. To study whether these patterns are reflected in labor market outcomes, I present estimates stratified into white (Panel A) and non-white ACS respondents (Panel B) in Table 4. To produce the table, I use OLS to estimate Equation (1) from Section 3 separately for white and non-white respondents and I continue to use ACS-provided weights and cluster standard errors at the PUMA level.

The estimates in Table 4 suggest that the effects on earnings and employment are concentrated among white men in same-sex couples. In particular, in a specification with controls and fixed effects I find that white men experience a $5,123 relative decline in earnings while men who are not white experience a $3,051 decline. Each of these estimates is statistically significant at the 1% level. Looking at employment, the effects follow a similar pattern with white men experiencing greater declines in hours worked and the probability of employment. The pattern of findings strongly suggests that my findings are caused by the advent of PrEP

Table 4: Estimates Focusing on the Effect of PrEP by Race

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp. Income	Emp. Income	Usual Hours Worked	Usual Hours Worked	Employed	Employed
Panel A - White Men						
Gay	8,831.64***	5,682.64***	0.97***	0.93***	0.024***	0.018***
	(622.32)	(489.27)	(0.12)	(0.12)	(0.002)	(0.002)
After Prep	1,163.07***	950.88***	0.20***	0.22***	0.026***	0.023***
	(187.13)	(113.42)	(0.02)	(0.02)	(0.000)	(0.000)
Gay × After Prep	-7,006.33***	-5,122.92***	-1.54***	-1.04***	-0.020***	-0.018***
	(651.22)	(574.28)	(0.14)	(0.13)	(0.002)	(0.002)
Observations	4,357,214	4,357,214	4,357,214	4,357,214	5,680,908	5,680,908
R-squared	0.28	0.45	0.11	0.23	0.018	0.034
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Panel B - Non-White Men						
Gay	7,277.15***	4,692.07***	1.31***	1.17***	0.016**	0.009
	(1,046.43)	(896.60)	(0.29)	(0.28)	(0.007)	(0.006)
After Prep	921.69***	577.44***	0.08**	0.12***	0.029***	0.026***
	(263.18)	(139.77)	(0.04)	(0.04)	(0.001)	(0.001)
Gay × After Prep	-5,408.46***	-3,050.73***	-1.11***	-0.79**	-0.010	-0.007
	(1,226.03)	(985.83)	(0.34)	(0.32)	(0.007)	(0.007)
Observations	990,519	990,519	990,519	990,519	1,508,286	1,508,286
R-squared	0.32	0.48	0.09	0.20	0.021	0.045
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes

Data: 2009 to 2019 data from ACS restricted to men in the labor force and, in the first four columns, further restricted to men who have ESI. Standard errors, clustered at the statistical area level (Public Use Microdata Area or PUMA), in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic Controls include education, age, marital status, and student status. Because estimates are stratified by self-reported race I do not separately control for race. Fixed effects include state of residence, PUMA, industry, and occupation.

4.2.3 Placebo Analysis Using Female ACS Respondents

In Table 5, I present estimates that mirror the estimates in Table 2 but focus only on female ACS respondents. Again, I use Equation (1) in Section 3, OLS estimation, ACS-provided weights, and cluster standard errors at the PUMA level. The only difference is that the estimates compare outcomes for females in same-sex couples to other females after 2012, rather than looking at outcomes among groups of males. This analysis therefore serves as a placebo test; if I were to find that there were similar effects on earnings and employment outcomes for females, then it is likely some other change, that affects individuals in same-sex couples, is driving my findings.

Looking at earnings, in a specification with fixed effects and controls, I find a post-2012 decline in earnings of just over $600 that is statistically significant only at the 10% level. This compares to a $4,665 decline for men, statistically significant at the 1% level, in a similar specification in Table 2. Further, looking at employment on the intensive margin (hours), it seems that females in same-sex couples work roughly 30 minutes more each week after 2012. In contrast, hours worked decline by about 1 hour per week for men in same-sex couples in the same specification in Table 2. I find negligible effects on employment at the extensive margin.[22]

If my findings for men were somehow caused by changing attitudes towards the LGBTQ population or due to changes in same-sex marriage laws, we would expect to find similar effects when focusing on females. My event study analyses for men, showing a sharp change 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 Figure A2.

[22]Note that in Table 5, I use the term "Lesbian" to refer to lesbian, bisexual, and other non-heterosexual sexual orientations, among those who identify as female.

Table 5: Estimates Focusing on Females

	(1) Emp. Income	(2) Emp. Income	(3) Usual Hours Worked	(4) Usual Hours Worked	(5) Employed	(6) Employed
Lesbian	9,247.54***	5,896.63***	2.11***	1.26***	0.005**	0.004**
	(382.19)	(305.85)	(0.11)	(0.10)	(0.002)	(0.002)
After PrEP	465.34***	218.58***	0.22***	0.23***	0.017***	0.016***
	(121.54)	(71.94)	(0.02)	(0.02)	(0.000)	(0.000)
Lesbian × After PrEP	-1,219.65***	-626.96*	0.41***	0.48***	-0.003	-0.002
	(442.03)	(343.03)	(0.12)	(0.11)	(0.002)	(0.002)
Observations	5,285,740	5,285,740	5,285,740	5,285,740	6,868,590	6,868,590
R-squared	0.23	0.44	0.08	0.20	0.014	0.029
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes

Data: 2009 to 2019 data from ACS restricted to females in the labor force and, in the first four columns, to females with ESI. Standard errors, clustered at the statistical area level (Public Use Microdata Area or PUMA), in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic Controls include race, education, age, marital status, and student status. Fixed effects include state of residence, PUMA, industry, and occupation.

4.2.4 Sensitivity

In Table 6, I examine the sensitivity of my findings to choices regarding clustering, weighting, and the analysis sample period. Note that, to avoid the table becoming cluttered, I only present the coefficient on the interaction terms from each sensitivity analysis.

To begin, Panel A presents the same estimates as in Table 2 but with no clustering. Panel B's estimates use state level clustering. Panel C examines what happens if I trim my sample and focus only on the years 2010 to 2016, rather than 2009 to 2019. Panel D provides estimates without using the ACS-provided person weights (as I do in all other estimates). In each case, my estimates remain broadly similar both in magnitude and in statistical significance.[23] However, the estimates in Panel C suggest that excluding 2017, 2018, and 2019 from my sample reduces the measured effect of PrEP on labor market outcomes for men in same-sex couples. 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.[24] Therefore, with PrEP use rising rapidly, it makes sense that I find even larger effects when using a longer sample period.

[23] Of course, changes in clustering only change the estimated standard errors.
[24] See https://aidsvu.org/prep/.

Table 6: Sensitivity Analyses

	(1) Emp. Income	(2) Emp. Income	(3) Usual Hours Worked	(4) Usual Hours Worked	(5) Employed	(6) Employed
Panel A - No Clustering						
Gay × After PrEP	-6,585.12***	-4,665.49***	-1.40***	-0.96***	-0.018***	-0.016***
	(392.94)	(343.78)	(0.11)	(0.10)	(0.002)	(0.002)
Observations	5,347,733	5,347,733	5,347,733	5,347,733	7,189,194	7,189,194
R-squared	0.29	0.46	0.11	0.22	0.020	0.037
Panel B - State-level Clustering						
Gay × After PrEP	-6,585.12***	-4,665.49***	-1.40***	-0.96***	-0.018***	-0.016***
	(552.92)	(563.30)	(0.16)	(0.16)	(0.002)	(0.002)
Observations	5,347,733	5,347,733	5,347,733	5,347,733	7,189,194	7,189,194
R-squared	0.29	0.46	0.11	0.22	0.020	0.037
No. of Clusters (State)	51	51	51	51	51	51
Panel C - Sample Period 2010-2016						
Gay × After PrEP	-5,104.21***	-3,411.67***	-1.25***	-0.87***	-0.014***	-0.013***
	(673.65)	(562.73)	(0.16)	(0.15)	(0.003)	(0.003)
Observations	3,324,897	3,324,897	3,324,897	3,324,897	4,509,918	4,509,918
R-squared	0.29	0.46	0.11	0.23	0.019	0.038
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Panel D - No Weights						
Gay × After PrEP	-7,059.10***	-4,816.90***	-1.41***	-0.91***	-0.021***	-0.018***
	(582.06)	(457.72)	(0.12)	(0.11)	(0.002)	(0.002)
Observations	5,347,733	5,347,733	5,347,733	5,347,733	7,189,194	7,189,194
R-squared	0.28	0.46	0.10	0.21	0.020	0.038
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes

Data: 2009 to 2019 data (unless noted) from ACS restricted to men in the labor force and, in the first four columns, further restricted to men who have ESI. Standard errors, clustered as noted, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic Controls include education, age, race, marital status, and student status. Fixed effects include state of residence, PUMA, industry, and occupation.

5 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 gay 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 completely 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 fraction of men who take PrEP would otherwise contract HIV. Indeed, to the extent that PrEP reduces the expected cost of HIV treatment among gay men who contract the virus, it would bias my estimates towards zero.

Using data from the American Community Survey my findings show that after the advent of PrEP (in 2012), annual earnings for gay men decline by 7.6% ($4,665) relative to other comparable men. 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 reduced hours of work and lower levels of employment, although it is less clear that the parallel trends assumption holds for these changes in employment. My estimates therefore support the idea that the cost of PrEP is mostly passed on to workers who are more likely to take PrEP in the form of lower wages.

To the extent that PrEP reduces risk for gay men, the advent of PrEP means that ESI becomes increasingly valuable for gay men after 2012. As ESI becomes more valuable, it is likely that my estimates reflect the net result of 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, gay men would still be "paying" for the cost of PrEP via lower earnings. When looking at changes in earnings, because we should see a decrease in labor demand and an increase in supply, the effects work together to reduce earnings for gay men. However, because increased willingness to work would counteract any PrEP-induced decline in labor demand, it is therefore more difficult to make strong claims regarding effects on employment. Note that with Truvada/PrEP being potentially so valuable to those at risk of HIV infection, it is also 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 gay men who do not have a partner. We might expect these "single" gay men to be more likely to take PrEP, even though PrEP is also used among gay couples, especially in cases where the partners are serodifferent. To the extent that single gay men are more likely to use PrEP, however, then including them in my "All Other Men" category might bias my difference-in-difference estimates towards zero, if they also experience declines in earnings due to the advent of PrEP.

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 gay men that could affect earnings but are unrelated to PrEP.[25] I cannot directly refute such alternate explanations for my findings. I can, however, point to the fact that gay men under 40 in my sample experience much larger reductions in earnings compared to older gay men. 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 gay and lesbian couples, are unlikely to be driving my estimates (Sansone, 2019).

Like other similar work on the effect of changes in the cost of providing ESI for various groups (Gruber, 1994; Lahey, 2012), I am not claiming that all gay men experienced overnight nominal reductions in earnings after PrEP is approved. The effects I observe are, instead, likely to be the outcome of several complementary changes including (1) some nominal but mostly relative reductions in earnings (i.e., increases in wages for some workers, flat wages for others), (2) gay

[25]The ACA's effect on uninsured workers makes comparisons across ESI status over this time period difficult. To avoid the confounding effects of the ACA, I focus only on those workers who have ESI in my main estimates. However, I present and discuss estimates that try to leverage ESI status as appendix items.

men not being chosen for jobs they would otherwise have been selected for, (3) gay men having to accept lower wage offers than they would otherwise be offered when switching jobs, and (4) longer periods of unemployment leading to lower reservation wages for gay men. Moreover, 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 gay men, my work provides a novel (and complementary) explanation for differences in earnings among gay and non-gay men over time. 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 gay men's earnings over time.

Finally, a 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 to individuals and families primarily as an employment benefit. More work is needed to help us understand the empirical regularities associated with expensive new pharmaceuticals.

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A Additional Estimates

A.1 Estimates of Effect of ESI on Gay Men

To examine whether ESI leads to diminished labor market outcomes for gay men, I use the advent of an expensive drug (Truvada) that primarily affects gay men to aid identification. In contrast, earlier work on the effect of ESI on outcomes for certain groups 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 section, I therefore use a similar approach comparing earnings for men in same-sex couples to those of other men with and without ESI. When using this approach to look at how ESI affects gay men, the estimating equation is;

$$Y_{it} = \alpha + \beta_1 Gay_{it} + \beta_2 ESI_{it} + \beta_3 ESI_{it} \times Gay_{it} + X_{it}\Pi + \epsilon_{it}. \tag{A1}$$

In Equation A1, Y_{it} refers to some outcome of interest for individual i in time period t. Because my estimation sample is restricted to males, the Gay_{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 zero otherwise. The ESI_{it} indicator term equals one if individual i reports that they have ESI at time t and zero otherwise. These indicator variables control for differences in outcomes for gay men that persist regardless of ESI status and for differences in outcomes for men who have ESI versus those who do not. The coefficient on the interaction of the indicator terms represents the difference between outcomes for gay men versus other men 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 A1, I present estimates where I use Equation A1 to examine how ESI and earnings are related for men in same-sex couples, relative to other men.[A1] In columns (1) and (2) of the

[A1] Again, please note that for brevity and ease of exposition, I abbreviate gay, bisexual, and other non-heterosexual sexual orientations as gay. I also, for the sake of simplicity rather than trying to erase any orientation or preferences, often refer to men in same-sex couples as representing gay men.

Table A1: ESI and Gay Men's Labor Market Outcomes Using Standard Approach

	(1) Emp. Income	(2) Emp. Income	(3) ESI	(4) ESI	(5) Emp. Income	(6) Emp. Income
Gay	3,527.38***	2,312.29***	0.048***	0.049***	-1,058.39***	-1,205.07***
	(400.10)	(225.86)	(0.003)	(0.002)	(359.83)	(299.76)
ESI					15,791.14***	10,600.68***
					(154.85)	(90.10)
Gay × ESI					4,883.32***	3,833.76***
					(457.44)	(402.07)
Observations	6,886,619	6,886,619	6,886,619	6,886,619	6,886,619	6,886,619
R-squared	0.31	0.47	0.096	0.203	0.33	0.48
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes

Data: 2009 to 2019 data from ACS limited to working males age 18 to 64. Standard errors, clustered at the statistical area level (Public Use Microdata Area or PUMA), in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic Controls include race, education, age, marital status, and student status. Fixed effects include state of residence, PUMA, industry, and occupation.

table, before examining whether gay men 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 location, industry, and occupation fixed effects to produce the estimates in the second column. In all specifications, I use OLS estimation, ACS-provided weights, and, although it is likely an overly-conservative approach, I cluster standard errors at the Public Use Microdata Area (PUMA) level.

In a specification that includes demographic controls and fixed effects, I find that men in same-sex couples earn $2,312 *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 4.9 percentage points more likely to have ESI. These patterns follow the summary statistics that I present in Table 1.

In the fifth and sixth columns, 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 $1,205 less per year than other men and that men with ESI earn considerably more ($10,601) 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 having several thousand dollars greater annual medical expenditures relative to other men (Gavulic and Gonzales, 2021). In a specification with controls and fixed effects, my estimates suggest that - relative to the gap between earnings for men in same-sex couples and other men when they do not have ESI - men in same-sex couples experience a $3,834 wage premium relative to other men, 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 $10,601 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 any 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 gay men, the advent of PrEP provides the necessary variation because it costs $24,000 per year. While workers may pay some of that cost, 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 gay men using the advent of Truvada in my main estimates in the text of the paper.

A.2 Estimates and Event Studies for Sample Restricted to Couples Only

In Table A2, I provide estimates that mirror Table 2 but limit the sample to married men or men in cohabiting relationships. Those estimates follow the same pattern as my main findings (in Table 2) with similar effects on earnings, hours worked, employment. However, in each case the point estimate is a little smaller in magnitude. To complement those estimates, in Figure A1, I present event study plots (using the estimating equation from Section 4.1 in the main body of the paper) where I again eliminate men who are neither married nor cohabiting with a partner of any gender.

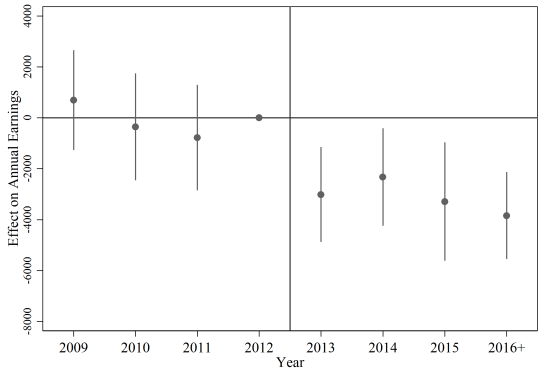
For completeness, in Table A3, I provide estimates that mirror those in Table A1 but limit the sample by eliminating men who are neither married nor cohabiting with a partner of any gender. The estimates follow the same pattern in either case, suggesting that gay men experience greater earnings whenever they have ESI. Although, there is less evidence of an overall earnings premium for gay men with this sample restriction.

Table A2: Estimates Focusing on Men in Couples Only

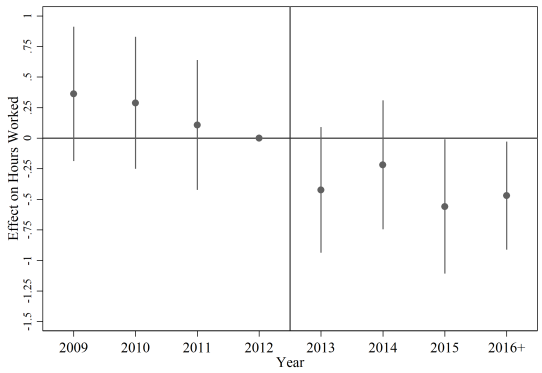
	(1)	(2)	(3)	(4)	(5)	(6)
	Emp. Income	Emp. Income	Usual Hours Worked	Usual Hours Worked	Employed	Employed
Gay	3,895.71***	2,308.06***	-0.46***	-0.04	0.015***	0.011***
	(614.71)	(488.77)	(0.12)	(0.11)	(0.002)	(0.002)
After PrEP	1,946.84***	1,403.84***	0.25***	0.25***	0.022***	0.020***
	(219.80)	(129.96)	(0.03)	(0.02)	(0.000)	(0.000)
Gay × After PrEP	-5,076.53***	-3,610.56***	-0.73***	-0.54***	-0.012***	-0.010***
	(634.03)	(546.82)	(0.14)	(0.13)	(0.002)	(0.002)
Observations	3,803,188	3,803,188	3,803,188	3,803,188	4,854,210	4,854,210
R-squared	0.21	0.41	0.02	0.13	0.012	0.028
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes

Data: 2009 to 2019 data from ACS restricted to married men or men in cohabiting couples in the labor force and, in the first four columns, further limited to just those with ESI. Standard errors, clustered at the statistical area level (Public Use Microdata Area or PUMA), in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic Controls include race, education, age, marital status, and student status. Fixed effects include state of residence, PUMA, industry, and occupation.

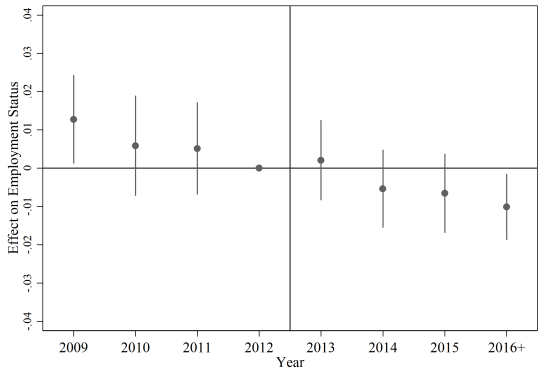
Figure A1: Event Studies for Couples Only



(a) Annual Income from Employment



(b) Usual Hours Worked Per Week



(c) Indicator for Employed (vs. Unemployed)

Notes: Each plot represents an event study where the sample is restricted to male ACS respondents age 18 to 64 who are in a married or unmarried couple. The year 2012 is the "omitted" time period. The dependent variable is noted below the related figure. Bars represent 95% confidence intervals.

Table A3: Estimates Using Standard Approach: Men in Couples Only

	(1) Emp. Income	(2) Emp. Income	(3) ESI	(4) ESI	(5) Emp. Income	(6) Emp. Income
Gay	636.52	29.04	0.061***	0.062***	-2,946.26***	-2,265.10***
	(406.25)	(226.55)	(0.003)	(0.002)	(349.41)	(313.00)
ESI					19,320.80***	13,358.68***
					(202.00)	(115.94)
Gay × ESI					3,144.83***	1,918.33***
					(500.92)	(418.31)
Observations	4,972,753	4,972,753	4,972,753	4,972,753	4,972,753	4,972,753
R-squared	0.24	0.43	0.106	0.221	0.27	0.44
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes

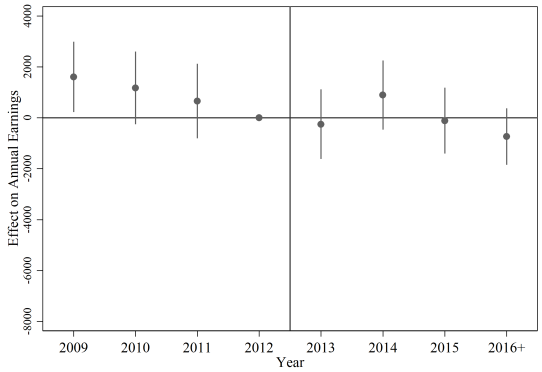
Data: 2009 to 2019 data from ACS limited to working males age 18 to 64 who are in a married or cohabiting couple either with a man or a woman. Standard errors, clustered at the statistical area level (Public Use Microdata Area or PUMA), in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic Controls include race, education, age, marital status, and student status. Fixed effects include state of residence, PUMA, industry, and occupation.

A.3 Event Studies for Sample Restricted to Females

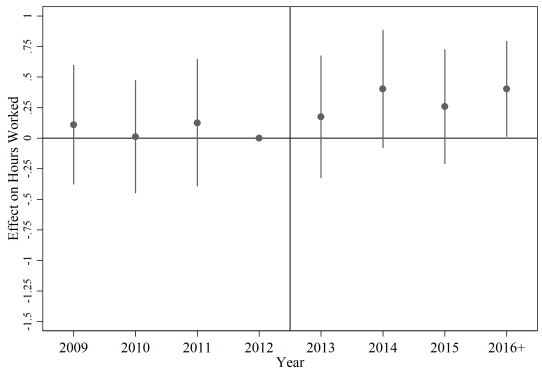
In Figure A2, I present event study plots (using the same estimating equation as I present in Section 4.1 in the body of the paper) where I limit my sample only to women. I examine the same earnings and employment outcomes as in Figure 1 in Section 4 of the paper, but now the estimates compare outcomes 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. Moreover, I observe a small positive effect on hours worked. These event study analyses therefore illustrate that, when looking at outcomes for gay men, 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 gay men and lesbian women. Instead, I only observe effects for gay men, and only after 2012, strongly supporting the idea that the advent of PrEP is causing the effects that I report in Section 4.

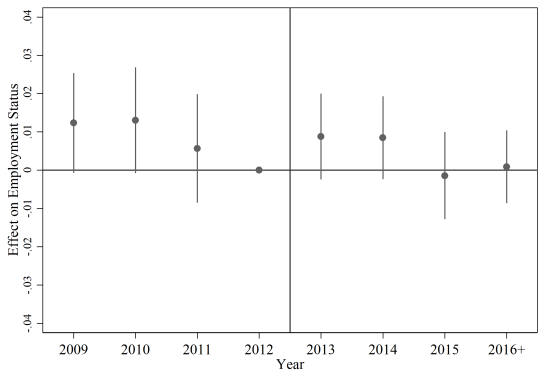
Figure A2: Event Studies for *Female* Labor Market Outcomes



(a) Annual Income from Employment



(b) Usual Hours Worked Per Week



(c) Indicator for Employed (vs. Unemployed)

Notes: Each plot represents an event study where the sample is restricted to female ACS respondents age 18 to 64. The year 2012 is the "omitted" category. The dependent variable is noted below the related figure. Bars represent 95% confidence intervals. See Section 4 for more details on these event study specifications.

A.4 Limit Sample to Men Aged 25 to 45

Because younger men are more likely to be prescribed Truvada, we might expect younger workers to experience a relatively larger negative effect of PrEP on labor market outcomes. In Section 4, I provide evidence that, among a sample restricted to only men in same-sex couples, younger men experience a large relative decline in earnings after 2012. Those estimates suggest that, given gay men experience an overall decline in earnings, young gay men experience a majority of that decline.

Another way to illustrate the effect of PrEP on younger men is by comparing outcomes for young men in same-sex couples to young men who I cannot identify as gay. In Table A4, I repeat the estimates in Table 2 when limiting my sample only to men age 25 to 45 who have ESI. I focus on that group because, in 2016, two-thirds of PrEP users were age 25 to 45.[A2] While the dollar value of the negative effect on earnings is a little smaller relative to my findings in Table 2, it is important to note that average earnings for younger men are also smaller.[A3] Therefore, a similar dollar decline in earnings represents a larger proportional decline. Moreover, the 95% confidence intervals overlap considerably with the relevant comparable estimates in Table 2, indicating that the estimates are not statistically different from one another.

Further, among a sample of workers with relatively lower earnings, there are limits to the ability of employers to pass along changes in the cost of ESI. Instead, employers might have to reduce hours for, or employ fewer of, these younger workers. The estimates in Table A4 suggest that is what happened. In particular, while I find a 1.6 percentage point decline in employment when looking at men in same-sex couples age 18 to 64 (see Table 2), I find between a 2 and 2.7 percentage point decline in employment when using a sample limited only to men age 25 to 45. Together the estimates suggest that, relative to non-gay men age 25 to 45, young gay men experience relatively large declines in both earnings and the likelihood of employment after PrEP becomes available.

Note that I do not include these estimates in the main paper because there are a few important limitations that could bias these estimates. First, among men age 25 to 45, the proportion of gay men who are in couples is likely be smaller relative to the proportion among those aged up to 64. This ensures that the "other men" group, when focusing on younger men, consists of relatively

[A2] See https://www.aidsmap.com/news/mar-2018/prep-use-growing-us-not-reaching-all-those-need.

[A3] Earnings for gay men under 40 are $53,616, significantly less than the average earnings of those over 40. See Table 1.

Table A4: Estimates Focusing on the Advent of PrEP for Men Age 25 to 45

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp. Income	Emp. Income	Usual Hours Worked	Usual Hours Worked	Employed	Employed
Gay	3,172.30***	1,659.30***	-0.59***	-0.32*	0.033***	0.020***
	(619.61)	(585.95)	(0.17)	(0.16)	(0.003)	(0.003)
After PrEP	279.26	569.68***	-0.03	0.19***	0.039***	0.035***
	(196.00)	(117.13)	(0.03)	(0.02)	(0.001)	(0.001)
Gay × After PrEP	-4,599.59***	-2,852.27***	-0.75***	-0.31*	-0.027***	-0.020***
	(747.57)	(659.94)	(0.20)	(0.19)	(0.004)	(0.004)
Observations	2,493,924	2,493,924	2,493,924	2,493,924	3,611,173	3,611,173
R-squared	0.23	0.41	0.04	0.28	0.035	0.138
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185	1,185	1,185
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes	No	Yes

Data: 2009 to 2019 data from ACS restricted to men in the labor force age 25 to 45 and, in the first four columns, to men with ESI. Standard errors, clustered at the statistical area level (Public Use Microdata Area or PUMA), in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic Controls include race, education, age, marital status, and student status. Fixed effects include state of residence, PUMA, industry, and occupation.

more men who are gay and not in a same-sex couple. That becomes increasingly problematic when two-thirds of all Truvada users are aged 25 to 45. If gay men who are not in a same-sex couple are more likely to take Truvada, then it will bias estimates of the wage gap between the groups downwards after PrEP is approved. Without being able to identify gay men who are not in same-sex couples, my analysis is inherently limited, and especially so when I focus on precisely those workers who may be hardest to classify as gay and not gay. The second limitation is that rates of ESI coverage and earnings are generally lower among young men and the ACA had a number of effects on younger workers - the dependent coverage mandate and Medicaid eligibility expansion, in particular (Antwi et al., 2013; Heim et al., 2018; Miller and Wherry, 2019). Because there could be significant ACA-related changes in the labor supply of younger non-gay men (i.e., a large fraction of the "control" group in these estimates) I present these estimates only as an appendix item. In any case, the size of the negative effect on earnings and employment outcomes remains large and statistically significant.

A.5 Triple Difference Estimates

In Table A5, I present triple difference estimates that examine the change after PrEP (i.e., time is the first difference) by sexual orientation (the second difference) and by ESI status (the third difference). If PrEP only has effects on labor market outcomes for men who have ESI, then using those without ESI as an additional control group could boost confidence in my estimates. Using a triple difference approach, my estimating equation is;

$$\begin{aligned} Y_{it} = \zeta &+ \psi_1 Gay_{it} + \psi_2 After\ PrEP_{it} + \psi_3 ESI_{it} \\ &+ \psi_4 After\ PrEP_{it} \times Gay_{it} + \psi_5 After\ PrEP_{it} \times ESI_{it} + \psi_6 ESI_{it} \times Gay_{it} \\ &+ \psi_7 After\ PrEP_{it} \times ESI_{it} \times Gay_{it} \\ &+ X_{it}\Pi + \epsilon_{it}. \end{aligned}$$

The coefficient of interest is ψ_7, which tells us the difference in the change in outcome Y by ESI status and gay status after 2012 (i.e., when PrEP was approved). The identifying assumption here is that, without PrEP, the difference in earnings and employment among men in same-sex couples versus other men who do and do not have ESI would follow parallel trends. What I find is that the triple difference term suggests that there is a statistically significant ($1,903) decline in earnings after PrEP is approved for men in same-sex couples relative to other men whenever they have ESI relative to the same difference among men without ESI.

That being said, I am hesitant to consider these estimates as causal. As I mention earlier, the ACA changed incentives for workers with lower incomes (such as Medicaid expansion) and without ESI (i.e., the additional third difference here). Workers without ESI tend to earn less (the coefficient on the ESI term is large and positive in all of my estimates). Therefore, adding a third difference for ESI status here means that we are essentially comparing high-earning men who are mostly unaffected by the ACA (they already have high earnings and ESI) to lower-income men who are more affected by the ACA (they have lower earnings, don't have ESI, may qualify for Medicaid, and might adjust their labor supply to qualify for health insurance exchange subsidies, and so on). Moreover, there are potentially changes in labor demand because of the ACA. Firms without ESI but with more than 50 employees had to offer ESI from 2015 (100+ employees) or 2016

Table A5: Estimates Focusing on the Advent of PrEP Using Triple Difference Approach

	(1) Emp. Income	(2) Emp. Income	(3) Usual Hours Worked	(4) Usual Hours Worked
Gay	1,470.83**	583.45	-0.09	0.28
	(616.45)	(549.41)	(0.24)	(0.23)
After Prep	1,278.35***	1,174.17***	0.24***	0.25***
	(87.89)	(94.31)	(0.04)	(0.03)
Gay × After Prep	-3,672.96***	-2,698.90***	-1.19***	-0.77***
	(719.64)	(676.62)	(0.29)	(0.28)
ESI	16,950.00***	12,022.89***	2.35***	1.86***
	(157.37)	(105.80)	(0.04)	(0.03)
Gay x ESI	7,223.38***	5,600.87***	1.47***	1.05***
	(765.98)	(697.95)	(0.25)	(0.25)
After PrEP × ESI	-223.72	-448.90***	-0.04	-0.06*
	(173.05)	(118.30)	(0.04)	(0.03)
Gay × After PrEP × ESI	-2,722.69***	-1,903.42**	-0.51*	-0.44
	(875.20)	(832.34)	(0.31)	(0.30)
Observations	6,886,619	6,886,619	6,886,619	6,886,619
R-squared	0.34	0.48	0.12	0.22
No. of Clusters (PUMA)	1,185	1,185	1,185	1,185
Demographic Controls	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	No	Yes

Data: 2009 to 2019 data from ACS restricted to employed men age 18 to 64. Standard errors, clustered at the statistical area level (Public Use Microdata Area or PUMA), in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic Controls include race, education, age, marital status, and student status. Fixed effects include state of residence, PUMA, industry, and occupation.

(50+ employees) under the employer mandate's provisions. It is possible that firms without ESI prior to 2015 therefore also respond to the high cost of providing Truvada (and gay men's medical expenditures, more generally) by demanding relatively fewer gay men as workers. Without being able to control for firm size, I cannot explore this potential issue in greater detail. It should be clear, however, that men without ESI are likely not a valid comparison group during this time period.

While the pattern of estimates still points towards large negative effects for gay men with ESI after the advent of PrEP, comparing outcomes for workers with and without ESI in this particular sample time period is inherently unconvincing for the reasons I lay out above. To avoid the confounding effects of the ACA, I focus on workers who have ESI in my main estimates in the paper and present estimates that try to leverage ESI status here only as appendix items.