Beyond the Good Intentions:

Assessing Unintended Consequences of Labor and Health Policies

BY

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THESIS

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LIST OF ABBREVIATIONS

CASEN	*Caracterización Socioeconómica Nacional* (National Socioeconomic Characterization)
CDC	Centers for Disease Control and Prevention
CES-D	Center for Epidemiologic Studies Depression Scale
DID	Difference-in-Differences
ELPI	*Encuesta Longitudinal de la Primera Infancia* (Early Childhood Longitudinal Survey)
EPDS	Edinburgh Postnatal Depression Scale
FDA	Food and Drugs Administration
FONASA	*Fondo Nacional de Salud* (National Health Fund)
HAART	Highly Active Antiretroviral Therapy
HIV	Human Immunodeficiency Virus
HPV	Human Papilloma Virus
IDU	Injecting Drug Users
IV	Instrumental Variable
MSM	Men who have Sex with Men
NHANES	National Health And Nutrition Examination Survey
NSFG	National Survey of Family Growth
OCD	Obsessive-Compulsive Disorder
OECD	Organization for Economic Co-operation and Development
PPD	Postpartum Depression
PrEP	Pre-Exposure Prophylaxis
PSI	Parenting Stress Index
PTSD	Post-Traumatic Stress Disorder
STD	Sexually Transmitted Disease

SUMMARY

In the first chapter, I study the causal effect of longer maternity leave on postpartum depression and other measures of mental health. I take advantage of exogenous variation in paid maternity leave introduced by a 2011 policy change in Chile, which increased the paid postnatal leave period from 12 to 24 weeks. Using a difference-in-difference instrumental variable approach, I find that one extra week of paid maternity leave increases the probability of being diagnosed with postpartum depression by 0.3 percentage points. However, I find no evidence of an increase in self-reported maternal stress level during the first 18 months after delivery using the Parenting Stress Index (PSI) or an increase in longer term depressive symptoms measured by the CES-D scale. A likely mechanism driving the increased diagnosis is that longer paid maternity leave increases the likelihood of diagnosis given a fixed level of depressive symptoms. I show that longer leave increases labor market attachment of mothers and thereby affects insurance coverage and doctor visits. By remaining employed, women are not only less likely to be uninsured (1.2 percentage points), but they also get access to better health insurance (7 percentage points). The availability of better health insurance combined with more time to go to the doctor, increase doctor visits during the first year after delivery, especially mental health visits (1.2 percentage points). Taken together, the evidence suggests that longer maternity leave increases postpartum depression diagnosis mainly by increasing diagnosis as opposed to worsening mental health. This is important because diagnosis is an important prerequisite to get treatment.

In the second chapter, I study the impact of the Chilean paid maternity leave extension on labor market discrimination against women. Using a difference-in-differences approach, I find robust evidence that the aforementioned policy had an unintended effect: it reduced labor force participation of women of childbearing age by 3 percentage points and their employment by 2.4 percentage points, while it had no effect on the gender pay gap.

In the third chapter, I study how the availability of a drug that reduces the risk of sexually acquired HIV, Truvada for Pre-exposure prophylaxis (PrEP), is affecting men who have sex with men (MSM) sexual behavior, and if this change in behavior has a causal effect on the increase in sexually transmitted diseases (STDs) seen in the United States in the last years. Using data from NSFG and NHANES, and a propensity score matching differences-in-differences approach, I find evidence of risk compensation after the introduction of PrEP. I first document an increase in the probability of having more than 2 sexual partners in the last year as well as having a new sex partner in the last 12 months. I do not find evidence of an increase in sexual activity, however. This increase in the number of sexual partners translates into a large increase in the probability of having a non-monogamous relationship and a not statistically significant decrease in the probability of being married or cohabitating. Then, I study risky sexual behavior, specifically the probability of using condoms, finding a large increase in condomless sexual activity. I document individuals switching from always using condoms and using condoms less than 50% of the times to never using condoms. I find a large and significant increase in the probability of being tested for any STD of 10 percentage points. I also provide suggestive evidence of an increase in STDs (chlamydia and HPV).

1. Impact of Longer Maternity Leave on Maternal Mental Health and Wellbeing: Evidence from Chile

1.1. Introduction

The goals of providing paid maternity leave are multifaceted, but the literature has predominantly focused on analyzing how maternity leave affects labor market outcomes such as employment and wages (Waldfogel, 1998; Baum and Ruhm, 2016). A major objective of these policies is to provide mothers time to physically and mentally recover from childbirth and bond with their newborns, yet there is much less evidence on this topic. Furthermore, research has highlighted the importance of parental inputs, such as parental mental health, beyond the effect of wages and employment for children's development (Baum, 2003; Grace and Sansom, 2003). However, little is known about how maternity leave affects maternal postnatal health, especially mental health. The World Health Organization estimates that worldwide approximately 10% of pregnant women and 13% of women who recently gave birth experience mental disorders, predominantly depression. These rates are even higher in developing countries: 15.6% during pregnancy and 19.8% after childbirth.

This chapter answers the question of how maternity leave affects maternal postnatal mental health and wellbeing, placing emphasis on one of the most severe common mental diseases: postpartum depression. I exploit plausibly exogenous variation in maternity leave length introduced by a 12-week extension in Chile to examine how longer periods of paid maternity leave affect maternal postpartum mental health and wellbeing. After this policy took effect in 2011, women were allowed to take 24 weeks of paid full-time postnatal maternity leave or up to 30 weeks of paid part-time postnatal maternity leave, with an income replacement rate of 100% of previous earnings. This policy change only affected employed women since women are not eligible for paid leave if they are not employed. As such, it is possible to estimate a difference-in-difference style model where leave-eligible women are the treatment group and leave-ineligible women are the control. Though eligible women are likely to be different than ineligible women, as long as their health and wellbeing are trending similarly, the difference-in-difference analysis will give unbiased estimates. In order to estimate the effect of the policy on women induced to extend their leave, I estimate a model where I instrument for actual number of weeks of maternity leave taken with the maximum number of weeks of paid maternity leave allowed at the time of delivery interacted with the mother's paid leave eligibility status.

My first stage results indicate that there is a significant increase in the number of weeks of maternity leave taken after this policy was implemented. Being eligible to take one extra week of paid maternity leave induces women to take over 0.8 additional weeks of maternity leave. This high take-up rate translates into an average of almost 24 weeks of postnatal maternity leave from 2012 onward, when the policy went into effect. Using these results, I then estimate how this increased maternity leave duration affects maternal postpartum mental health. I find that taking one extra week of maternity leave increases the probability of being diagnosed with postpartum depression by almost 0.3 percentage points, implying that this 12-week extension policy increased the probability of diagnosis by approximately 3 percentage points. Since being diagnosed with postpartum depression depends on both being depressed and also seeking healthcare, either or both factors could be driving this result. Though it is not possible to disentangle these two factors definitively, I provide indirect evidence by examining the effect of postnatal leave on doctor visits and self-reported measures of mental health.

In the absence of data on self-reported postpartum depression symptoms, I use self-reported parental stress level during the first 18 months after childbirth as a proxy. I find no evidence of an increase in the probability of having a high stress level or a clinically significant stress level measured by the Parenting Stress Index (PSI). Since stress is a very imperfect proxy for depression, I also study other outcomes that are related to affective disorders. Specifically, I look at life events that have been associated with triggering depressive disorders such as divorce and hospitalizations. I do not find evidence of a differential increase in divorce rates for eligible women, but my results suggest an increase in the probability of being hospitalized due to an illness or accident among the treated group.

Overall, most of my evidence suggests that mothers' mental health did not worsen after the maternity leave period was extended, implying that an increase in diagnosis is driving my results. Further evidence that is consistent with my interpretation of the findings comes from looking at the effects on doctor visits during the first year postpartum. Though not statistically significant, there is a 1.4 percentage-point (5%) increase in the probability of visiting any doctor in the first year postpartum. Notably, there is a larger and statistically significant increase of 1.2 percentage points (52% relative to the baseline mean) in the probability of having a mental health visit during the first year after childbirth. Then, I investigate the possible mechanisms that are driving this increase in doctor visits by looking at employment and health insurance coverage. Having the possibility of spending 12 extra weeks at home taking care of their babies increases mothers' labor force attachment in the short-term. I find an increase of 5 percentage points in employment during the first year after delivery, and an increase of almost 3 percentage points in the subsequent year. This increase in employment due to the longer leave reduces the probability of being uninsured by 1.2 percentage points (61%) and significantly increases the probability of having access to better health insurance coverage that includes private healthcare providers by 7 percentage points (14%).

I also study if this increase in diagnosis translates into an increased probability of receiving treatment for depression. I find an increase of 26% (0.5 percentage points) in the probability of being treated for depression after delivery, although this estimate is not statistically significant. This result is in line with the 27% increase in the probability of being diagnosed with postpartum depression described earlier.

Finally, I examine longer-term effects of a longer maternity leave period. I start by looking at depressive symptoms between 4 and 9 years after childbirth and find no statistically significant effect.

Looking at broader measures of wellbeing, I find no differential change in the probability of being divorced between eligible and ineligible women; however, women who were exposed to this longer maternity leave period are significantly more likely to be employed between 4 and 9 years after delivery (6 percentage points).

These findings contribute to the literature in several ways. First, I provide a better identified causal effect of maternity leave on maternal postpartum mental health by using an exogenous instrument for actual maternity leave taking. Second, the previous literature studies the effect of maternity leave on self-reported depression symptoms, but this study is able to provide evidence on effects that go beyond symptoms of depression by estimating causal effects on depression diagnosis and treatment. Third, I provide evidence on longer-term effects of an extended maternity leave period on depressive symptoms and other measures of wellbeing. The results have important implications because improved diagnosis is important for mothers' wellbeing, for enabling them to get access to treatment, as well as for the effects on their children's wellbeing and development.

1.2. Literature Review

There is a small body of literature studying the effects of maternity leave taking on maternal mental health. Gjerdingen and Chaloner (1994), using the Mental Health Inventory, find that taking more than 24 weeks of postnatal leave versus taking 9 weeks or fewer, is associated with better mental health at 9 and 12 weeks after delivery. Similarly, McGovern et al. (1997), using the Mental Health Index (short-form), find that leave duration of more than 15 weeks postpartum is associated with better mental health at 7 months after giving birth. On the other hand, a study by Killien (1998) finds no association between leave duration and parental stress or with maternal separation anxiety. A number of studies have focused on depression after childbirth, finding mixed results. Hyde et al. (1995) find that taking 6 weeks of leave versus 12 weeks was associated with higher depression scores only among mothers reporting high marital concerns and unrewarding jobs. Klein et al (1998) use follow-up data at 1 year after delivery of the sample studied by Hyde et al. (1995) to show that longer leaves were associated with higher depression scores among mothers with higher levels of work involvement than family involvement. These two studies do not address the potential endogeneity of maternity leave taking.

Chatterji and Markowitz (2005) use an instrumental variable approach, using state-level labor market conditions and state-level maternal leave policies as the excluded instruments. They find that one extra week of maternity leave was associated with a 6-7 percent decrease in depressive symptoms 6 to 24 months after delivery. However, they do not find any significant effect on the probability of being depressed. Chatterji and Markowitz (2012) use state-level policies related to maternity leave and state-level labor market conditions in addition to cross-sectional variation in cost of childcare as identifying instruments, finding no significant impact of leave taking on depressive symptoms. One limitation of these two studies is that labor market conditions has the potential to affect depression through channels outside of leave taking as local labor market conditions can affect family income and spousal labor supply.

Most closely related to this study is Dagher et al. (2014) which studies the effect of maternity leave duration on depressive symptoms. Unlike past literature, they use the Edinburgh Postnatal Depression Scale (EPDS) and they aim to address endogeneity of leave taking behavior by instrumenting for actual leave taken using the policies at the employer[1]. Essentially, they compare the postpartum depression of women working at firms with generous leave policies to the postpartum depression of women working at firms with restrictive leave policies. Though this dissertation represents a major move forward relative to the literature, the analysis relies on two strong and

[1] The instruments are the maximum available duration of all paid leave (maternity, vacation, sick, and disability leave) according to the employer policy and the maximum available duration of job-protected leave according to employer policy.

fundamentally untestable assumptions. First, the authors must assume that conditional on covariates, women who work at firms with generous leave policies are comparable to women who work at firms with restrictive leave policies. This assumption may not hold since one's employer is the result of both employer and employee choices and unlikely to be random. Second, the authors must assume that companies that offer more generous leave only affect postpartum depression through that leave generosity – not through other concurrent policies. For example, if companies that offer generous leave also are more likely to have childcare on site or a less high-pressure work environment, the IV estimates will conflate these factors with the effect of leave.

1.3. Background

1.3.1. Postpartum Depression

Postpartum depression (PPD) is usually defined as an episode of non-psychotic depression with onset within one year of childbirth (Stewart et al., 2003). PPD typically starts within the first six weeks after delivery and it usually require professional treatment. PPD symptoms are usually the same as those associated with major depression happening at other periods, including tearfulness, loss of appetite, feelings of guilt, sleep difficulties, poor memory and concentration, feelings of being incapable to handle the newborn, irritability, and fatigue (Stewart et al., 2003). In some severe cases, it might lead depressed mothers to commit suicide.

Mothers affected with postpartum depression cannot function properly. They may fail to adequately eat, bathe or care for themselves in other ways, and to provide satisfactory care to their children. As a result, their children's growth and development may be negatively affected since infants are highly dependent and sensitive to the environment and the quality of care. Research suggests that young children of mothers suffering postpartum depression face more behavioral, interpersonal and cognitive problems than children of non-depressed mothers. Exposure to persistent or recurrent episodes of maternal depression are more likely to have long term effects on children (Stewart et al., 2003).

Childbirth can be the stressor that triggers the beginning of recurrent or chronic episodes of depressive disorder, both related to following deliveries and also unrelated to childbirth. After one episode of postpartum depression, the risk of recurrence is 25% (Wisner et al, 2001). Unfortunately, screening for postpartum depression can be challenging due to the many symptoms normally associated with having a newborn that are also symptoms of major depression, for example, appetite and sleep disruption, low energy, and reduced libido (Nonacs and Cohen, 1998). While severe postpartum depression is easily identified, less severe cases can be easily dismissed as natural consequences of childbirth. This means that postpartum depression often remains undiagnosed (Stewart et al., 2003).

1.3.2. Maternity Leave Policies in Chile

Prior to October 2011, Chilean working women were eligible for maternity benefits including 6 weeks of paid prenatal leave, 12 weeks of paid postnatal leave, and paid sick leave in case of serious illness of children younger than one year old. In October of 2011, the national government passed a law which modified the maternity leave period that all firms have to offer. The new law allows women to have an extra 12 weeks of paid postnatal maternity leave if they choose to be absent from work full-time, or 18 extra weeks if they choose to be away from work part-time (half of their regular working time). The legislation also incorporated a third option: women can transfer part of the maternity leave to the working father of the child.[2]

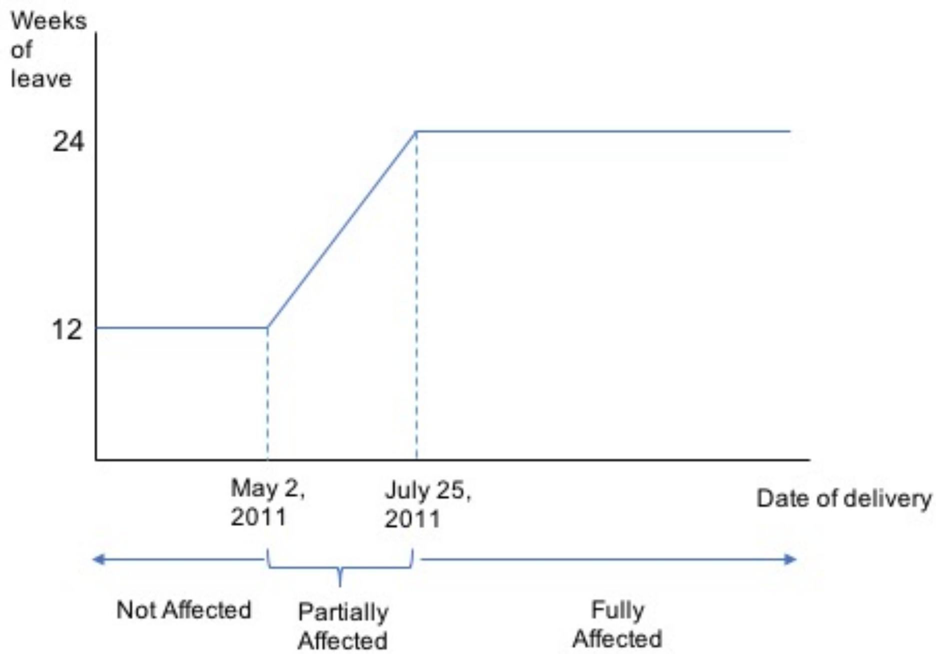
[2] This transference can only happen after the sixth week of the new leave period and for the number of weeks that the mother indicates, up to 6 weeks of full-time leave or 12 weeks of part-time leave. In this case, the amount of the subsidy is determined according to the father's salary.

The income replacement rate was kept at 100% of previous earnings, capped at the maximum amount used to determine the contribution to the social security system[3]. This subsidy is paid with public funds from the Ministry of Labor and Social Security's Social Benefits Fund (*Fondo Único de Prestaciones Familiares y Subsidios de Cesantía*), which is financed with general taxes. Eligibility requirements are relatively easy to meet for women who are participating in the labor force: women need to have had contributed to the social security system for the first time at least 6 months before the beginning of the prenatal leave and must have been working for three continuous months before the beginning of the prenatal leave.

The implementation of this policy included a retroactive effect. Women who delivered after the new law was enacted (October 17th, 2011) were eligible to enjoy longer maternity leave of up to 24 weeks of full-time postnatal leave, but also those women who were already on maternity leave when the new law was passed were eligible to prolong their leave for up to 12 weeks of full-time leave or 18 weeks of part-time leave. This means that those women who delivered on or after July 25th, 2011 were eligible for the full benefits of the new policy, whereas women who delivered between May 2nd, 2011 and July 24th, 2011 were entitled to take a fraction of the additional 12 weeks of paid postnatal leave, which depends on the date they gave birth. The number of extra weeks the mother was entitled to take was determined as follows: $w_i = \max[\min(12 - a_i, 12), 0]$, where a_i is the child's age in weeks on July 25th, 2011. Figure 1 depicts the relationship between number of weeks of maternity leave entitlement and date of delivery.

3 Approximately USD $3,300 per month.

Figure 1: Maximum number of weeks of full-time postnatal maternity leave entitlement.



The Chilean legislation includes additional benefits for women during pregnancy and after giving birth which are relevant for understanding the context in which the law is extending the duration of the maternity leave benefit but note that these other conditions did not change as a result of the law passed in 2011. A pregnant woman cannot be fired during her pregnancy and her job is also protected for one year after the end of the first 12 weeks of postnatal leave. Once the mother returns to work, she has the right to absent from work for one hour a day in order to feed her newborn until the child turns 2 years old. Also, those firms that employ 20 or more women are required to provide childcare to their workers' children until they turn 2 years old.

Since its implementation in October 2011, the majority of women have chosen the full-time option of the new benefit. When the new policy was just implemented, 7% of mothers chose the part-time option; this proportion started to decrease in early 2012 and stabilized at around 1% in 2013. The relatively high proportion of the part-time option usage at the beginning of the new policy is explained by women who had already given birth when the policy took effect, but still met the conditions set for using the new benefit. Since these women had already agreed with their employers to come back to work after 12 weeks, many of them opted to use the part-time option of the new benefit.

1.4. Conceptual Framework

This section outlines the determinants of postpartum depression, and how these may be affected by the maternity leave extension studied in this chapter. The medical literature documents through biological and genetic studies of mood disorders that they are complex conditions, with several factors that might cause the disorders. Even if an individual is genetically predisposed to developing depression, the genes must interact with experiential and/or environmental factors in order for those genes to be expressed and cause the illness (Dubovsky and Buzan, 1999).

After delivery, reproductive hormone levels decline rapidly, which has been suggested as a possible cause of postpartum mood disorders (Wisner et al., 2002). Some hormone levels return to pre-pregnancy levels in periods as short as 3 days, while others such as thyroid function return to pre-pregnancy levels approximately 4 weeks after childbirth (Robinson et al., 2001). Previous research has suggested that postpartum depression is caused by low levels of progesterone or estrogen (Bloch et al., 2000), or high levels of prolactin; however, no consistent relationships have been established (Harris, 1994; Hendrick et al., 1998). Given this, it is better to think of postpartum depression as being caused by multiple factors. Even if some women are more vulnerable to hormonal changes, the interaction with environmental factors might trigger the affective disorder (Stewart et al., 2003).

Stewart et al. (2003)'s meta-analysis documents several environmental factors that predict postpartum depression. This study finds that the strongest predictors are anxiety and depression during pregnancy, low social support levels, experiencing stressful life events during pregnancy or the early puerperium, and having a history of previous depression. Other predictors are obstetric and pregnancy complications, low self-esteem, high levels of childcare stress, infant temperament, quality of relationship with partner, neuroticism, negative cognitive attributions, and low socioeconomic status.

The effect of these environmental risk factors might be attenuated by the availability and extension of maternity leave. Taking time off work after childbirth is a strategy that can help mothers adjust to their new responsibilities imposed by their multiple roles (worker, partner, and mother) while physically recovering from childbirth (Dagher et al., 2014). Paid maternity leave provides women job protection and income replacement while they are taking time off work after delivery. Having no maternity leave benefit or a short leave duration might cause some women to drop out of the labor force after childbirth. Providing longer maternity leave duration along with job protection could incentivize women to remain attached to the labor market, hence reducing the probability of losing their job and facing unemployment, which could also prevent a decrease in long-term family income. Several studies have shown that losing a job is a stressor that can trigger depressive episodes (Brown and Harris, 1978), and that unemployment and low income are risk factors in mental health disorders (Bartley, 1994; Lee et al., 2000; Patel et al., 1999; Patel et al., 2002; Weich et al., 1997). Another postpartum depression risk factor that could be ameliorated by longer maternity leave is childcare related stress. By allowing women to spend more time with their newborns, mothers do not have to worry about finding appropriate childcare for their infant when they are only a couple months old. Beck (2001) documents that childcare stress as well as having and infant with a difficult temperament are predictors of postpartum depression.

1.5. Data

I use data from the Early Childhood Longitudinal Survey (ELPI) and the National Socioeconomic Characterization Survey (CASEN), both from the Chilean Ministry of Social Development. The ELPI survey consists of a representative sample of children and their primary caregiver, usually the mother. It collects health- and employment-related data about that mother and the child during pregnancy, delivery, and the first year after delivery. Additionally, it includes other socioeconomic characteristics of household members at the time of the interview. The first wave of this survey was conducted in 2010 and included children born between January 1st, 2004 and August 31st, 2009. The second wave added a representative sample of children born between September 1st, 2009 and December 31st, 2011. The third wave incorporated children born between January 1st, 2012 and December 31st, 2016. This last wave also included data regarding the secondary caregiver of the children. I complement the previous dataset by using repeated cross-sectional data from the CASEN survey, which is typically conducted every other year and consists of a representative sample of households of the whole country. It collects individual data on demographic characteristics, education, employment, income, health, and housing variables. I use the 2003, 2006, 2009, 2011, 2013, and 2015 waves.

ELPI survey data allow me to determine whether a woman was eligible to take paid maternity leave. Given the minimum eligibility requirements, over 95% of women who were employed before giving birth were eligible for the paid maternity leave benefit. My empirical approach compares changes in outcomes induced by the maternity leave extension for eligible women to those for ineligible women. Table I shows pre-2011 descriptive demographic characteristics separately for eligible and ineligible women, who have different levels of labor force attachment at baseline. A comparison of these baseline statistics shows that eligible women, with relatively higher labor force participation, are generally more advantaged including more educated, more likely to be married and older than the ineligible women, who have lower labor force participation. For example, eligible women are 4 years older than ineligible women, and close to 76% of eligible women are married or cohabitating with their partners versus only 69% of ineligible women. There is also a small but statistically significant difference among these two groups in terms of number of children: eligible women have fewer children on average than ineligible women. Interestingly, despite eligible women appearing to be relatively advantaged compared to ineligible women, eligible women are over 4 percentage points more likely to be diagnosed with postpartum depression than ineligible women.

Table I: Pre-2011 descriptive demographic characteristics, postpartum depression rate and weeks of maternity leave taken for eligible and ineligible women.

	Ineligible	Eligible	Difference
Age at childbirth	25.7	29.7	-4.0***
Married or living with partner	69.4%	75.6%	-6.2%***
Years of Schooling	11.0	13.3	-2.3***
High School Dropout	40.3%	11.4%	-28.9%***
High School Diploma	44.2%	47.9%	3.7%***
Some College	12.8%	21.8%	9.0%***
College Degree	2.7%	18.8%	16.1%***
Total children	2.1	2.0	0.1***
Employed before delivery	0.3%	100%	99.7%***
Postpartum depression	12.2%	16.5%	-4.4%***
Weeks postnatal leave	0.5	13.7	-13.2***
Observations	8,352	3,157	11,509

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Pregnancy and delivery characteristics of eligible and ineligible women at baseline are shown in Table II. Though less dramatic than maternal characteristics, this table shows several significant differences between the two groups. The largest difference is with regard to having a planned pregnancy. Eligible women are more likely to report having planned their pregnancy (56%), while only 43% of ineligible women report having planned their pregnancy. Eligible women are more likely to have a multiple birth, they have shorter pregnancy term and are less likely to have their children in an incubator. There are no statistically significant differences in having health problems or complications during pregnancy or delivery.

Table II: Pre-2011 descriptive pregnancy/delivery characteristics for eligible and ineligible women.

	Ineligible	Eligible	Difference
Planned pregnancy	42.8%	56.1%	-13.3%***
Health problem during pregnancy	41.2%	40.1%	1.1%
Multiple birth	1.7%	2.9%	-1.2%*
Problems during delivery	24.5%	25.4%	-0.9%
Average pregnancy term	38.7	38.4	0.3***
Moderately or extremely preterm infant	2.0%	1.9%	0.1%
Child was in incubator	7.1%	5.8%	1.3%***
Days in incubator	14.9	13.0	1.9
Low birth weight	0.39%	0.25%	0.1%
Observations	8,352	3,157	11,509

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

In terms of mental disorders during pregnancy, which are displayed in Table III, there is only a significant difference between eligible and ineligible women in the probability of being diagnosed with depression during pregnancy. Over 11% of ineligible women were diagnosed with depression during pregnancy, while only 9% of eligible women had a depression diagnosis. When it comes to postpartum depression, there are also significant differences between these two groups; however, this time eligible women are more likely to be diagnosed with postpartum depression: 16.5% versus 12.2%

of ineligible women. The many differences between eligible and ineligible women is the key concern that the empirical strategy must address.

Table III: Pre-2011 descriptive statistics of mental disorders during pregnancy for eligible and ineligible women.

	Ineligible	Eligible	Difference
Depression	11.4%	9.4%	2.0%***
Bipolar disorder	0.51%	0.53%	0.0%
Anxiety disorder	1.47%	1.26%	0.2%
OCD	0.20%	0.33%	-0.1%
Phobia	0.40%	0.53%	-0.1%
Panic disorder	1.0%	0.88%	0.1%
PTSD	0.66%	0.96%	-0.3%
Other mental disorder	0.35%	0.19%	0.1%
Observations	8,352	3,157	11,509

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

1.6. Empirical Strategy

1.6.1. Postpartum depression diagnosis

The primary challenge to identifying the causal effect of paid maternity leave is that women who choose to take more leave are likely different than women who take less leave on unobservable dimensions. In order to address this concern, I exploit the 2011 maternity leave policy change that induced women to take more leave. This was a national policy change, but only employed women were affected since women are not eligible for paid leave if they are not employed. As such, it is possible to estimate a difference-in-differences style model where leave-eligible women are the treatment group and leave-ineligible women are the control. Though eligible women are likely to be quite different than ineligible women, as long as their outcomes trend similarly, the difference-in-difference analysis will yield unbiased estimates. Unlike a simple difference-in-difference model, there is not a sharp pre/post period distinction because the policy incrementally increased maximum weeks of paid leave depending on birth month. I model this by defining each women's policy exposure based on the maximum number of weeks that she is eligible for. Equation 1 shows the baseline difference-in-differences model that compares eligible and ineligible women before and after the policy change.

$$PPD_{it} = \alpha + \beta_1 Max\ paid\ leave_t * Eligible_{it} + \beta_2 Eligible_{it} + \beta_3 Max\ paid\ leave_t + X'\delta + \gamma_t + \varepsilon_{it} \quad (1)$$

where PPD_{it} is a dummy variable equal to 1 if mother *i* who delivered in year *t* was diagnosed with postpartum depression, $Max\ paid\ leave_t$ is the maximum number of weeks of paid postnatal leave allowed by law on the date of delivery, $Eligible_{it}$ is a dummy variable equal to 1 if mother *i* who delivered in year *t* was eligible to take paid maternity leave, γ_t is year of delivery fixed effects, and X is a vector of covariates such as age, schooling, marital status, number of children, etc.

The coefficient β_1 is an intent to treat parameter since not all women eligible for the increased leave decided to take it. In order to estimate the effect of the policy on women induced to extend their leave, I estimate an IV model where the reduced form is equation (1) above and the first stage predicts weeks of leave from the policy. Specifically, the first stage is shown in equation (2) below.

$$Weeks\ of\ leave_{it} = \gamma + \beta_4 Max\ paid\ leave_t * Eligible_{it} + \beta_5 Eligible_{it} + \beta_6 Max\ paid\ leave_t + X'\delta + \gamma_t + e_{it} \quad (2)$$

where $Weeks\ of\ leave_{it}$ is the actual number of weeks of postnatal leave taken by mother *i* who delivered in year *t*. $Max\ paid\ leave_t$, $Eligible_{it}$, γ_t and X are defined in the same way as above.

This instrumental variable setup relies on several assumptions. First, it requires conditional exogeneity of the policy, essentially that the reduced form estimates can be interpreted as causal. I devote careful attention to this assumption in the subsequent analysis and provide specification tests supporting its validity. Second, the exclusion restriction requires that the policy only affects women's postpartum depression through its effect on leave taking behavior. This is a fundamentally untestable assumption, but it seems unlikely that the leave taking policy would have direct effects on postpartum depression. An example of a violation of the exclusion restriction would be if the maternity leave policy generated large general equilibrium effects that alter the postpartum depression of women regardless of their own leave taking behavior. Third, it is necessary to assume monotonicity, specifically the policy cannot induce some women to take less leave. In this case, the monotonicity assumption seems fairly innocuous. The fourth condition is the existence of a first stage relationship between the policy and leave-taking, which I document below.

Though level differences between the ineligible and eligible women do not necessarily cause bias, large differences make it less likely that the two groups would be trending similarly. I can directly assess trend similarity in the pre-period, but the difference-in-difference still relies on the fundamentally untestable assumption that trends would have be similar in the post period. To increase the likelihood that this assumption will hold and check the robustness of my estimates, I use propensity-score matching to make eligible and ineligible women look more similar. I match on demographic characteristics such as schooling, age, marital status, and number of children.

Following Dehejia and Wahba (2002), I first estimate the propensity score by using a logit model:

$$\Pr(T_i = 1 | X_i) = \frac{e^{\lambda h(X_i)}}{1 + e^{\lambda h(X_i)}} \qquad (3)$$

where T_i is the leave eligibility status and $h(X_i)$ consists of linear terms of the demographic characteristics on which I condition to obtain a predicted treatment assignment. Rearranging and taking logs, we obtain:

$$\lambda h(X_i) = \ln \frac{\Pr(T_i = 1|X_i)}{1 - \Pr(T_i = 1|X_i)} \qquad (4)$$

After estimating this propensity score, I verify if covariates are balanced between eligible and ineligible women. As Dehejia and Wahba (2002) point out, matching on the propensity score is essentially a weighting scheme, which determines what weights are placed on comparison units when calculating the estimated treatment effect using equations 1 and 2.

1.6.2. Other outcomes

My main outcome, postpartum depression diagnosis, depends on both being depressed and visiting the doctor to get diagnosed. Hence, any change in postpartum depression diagnosis could be reflecting a change in depression, a change in diagnosis due to a change in the probability of seeking healthcare given a fixed level of depression, or a combination of both. I examine different outcomes to provide indirect evidence on whether self-reported depression is changing, or if diagnosis is changing given a fixed level of depressive symptoms. I examine parental stress level, longer-term employment and depressive symptoms, and divorce. Data for these outcomes come from the ELPI survey. I also study health insurance coverage, doctor visits, and depression treatment using data from the CASEN survey. To estimate causal effects of the policy on these outcomes, I use a differences-in-differences approach. Treatment group status is based on paid maternity-leave eligibility for outcomes from the ELPI survey, and on employment status as a proxy for leave eligibility for outcomes from the CASEN survey. Unfortunately, CASEN data do not allow to identify leave eligibility. I use the following equation:

$$y_{it} = \beta_0 + \beta_1 Treated_{it} \times Post_t + \beta_2 Treated_{it} + X'\delta + \gamma_t + \varepsilon_{it} \qquad (5)$$

where y_{it} is the outcome of interest for mother *i* who delivered in year *t*, $Treated_{it}$ is a dummy variable equal to 1 if the woman *i* who delivered in year *t* was eligible to take paid maternity leave or if she was employed, $Post_t$ is a dummy equal to 1 for women who delivered on or after May 2, 2011, γ_t represents year-of-delivery fixed effects, and X is a vector of covariates such as age, schooling, marital status, etc. The coefficient of interest is β_1, which will tell us the change in a particular outcome for treated mothers that is attributable to the maternity leave policy.

I also study causal effects on short-run employment. To estimate the impact of longer paid maternity leave period on employment in the short-run, I use data from CASEN survey and a difference-in-differences approach. This approach compares employment of mothers of infants, the treatment group, to outcomes of mothers of older children, the control group. I use the following equation:

$$Employed_{it} = \beta_0 + \beta_1 Treated_i \times Post_t + \beta_2 Treated_i + \gamma_t + X'\delta + \varepsilon_{it} \qquad (6)$$

where $Employed_{it}$ is a dummy variable equal to 1 if mother *i* surveyed in year *t* was employed; $Treated_i$ is a dummy variable equal to 1 if the individual is a mother of a child younger than one year old or if she is a mother of a one year old child, and zero if such individual is a mother whose youngest child is between 5 and 10 years old; $Post_t$ is a dummy equal to 1 for 2011 onwards; γ_t represents year fixed effects, and X is a vector of covariates such as schooling, marital status, etc. The coefficient of interest is β_1, which will tell us the change in the employment rate for mothers of infants that is attributable to the maternity leave policy. The identifying assumption for this approach is that absent this maternity leave policy change, employment of childbearing age women and older women would have trended in the same way.

1.7. Results

1.7.1. Postpartum Depression Diagnosis

Figure 2 depicts maternity leave take-up rate in Chile. Before 2011, it was common that women take longer leave than the maximum paid leave allowance: almost 2 extra weeks on average. After the maternity leave period was extended, women take on average 24 weeks of leave, which is close to the maximum leave allowed by the policy. This high take-up rate should translate into a strong first stage. Table IV estimates confirm that there is a strong first stage. Being eligible to take one extra week of paid postnatal leave induces women to take almost 0.8 weeks of postnatal leave. The point estimate does not change much depending on whether controls are used in the regression; however, when propensity score matching is used, the point estimate increases by 4 percentage points.

Figure 2: Actual weeks of postnatal maternity leave taken and maximum full-time leave entitlement.

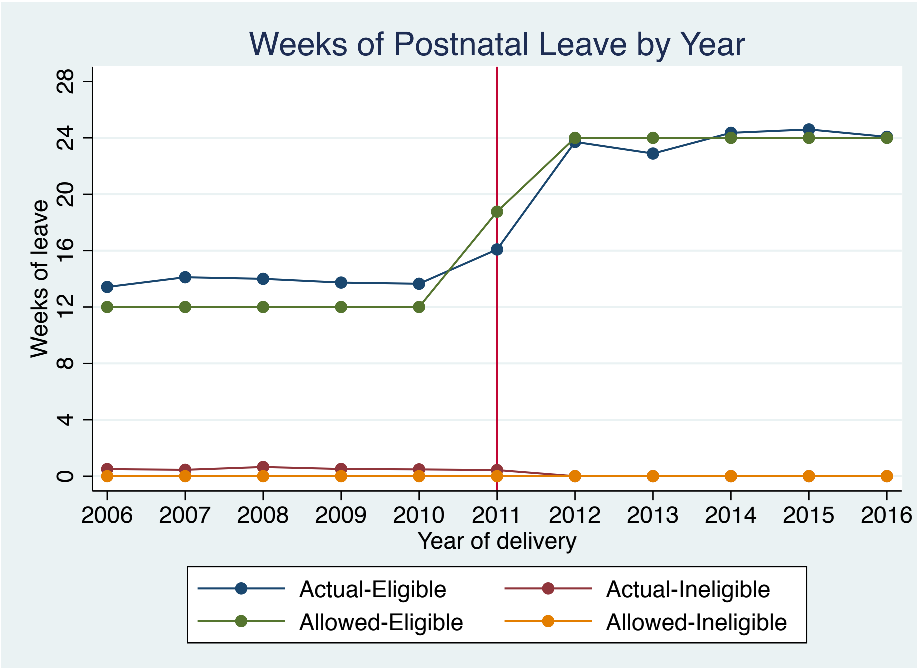


Table IV: First Stage results of the impact of one extra week of paid maternity leave on leave taking.

	1	2	3	4	5
			Weeks of Postnatal Leave		
Eligible x Max Weeks Paid Leave	0.8402***	0.8412***	0.8387***	0.8386***	0.8814***
	(0.0337)	(0.0344)	(0.0328)	(0.0332)	(0.0268)
Year FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	Yes	No	Yes	No
Pregnancy and Delivery Characteristics	No	No	Yes	Yes	Yes
Observations			16,345		

Robust standard errors in parentheses for columns 1 to 4. Column 5 uses propensity score matching. Bootstrapped standard errors in parentheses for column 5. All columns control for child's age in months at the time of survey. Demographic controls include age, marital status, schooling, number of children, and birth order. Pregnancy and delivery characteristics include depression during pregnancy, planned pregnancy, health complications during pregnancy, complications during delivery, preterm infant and low birth weight infant. Data come from ELPI survey.
* p<0.1, ** p<0.05, *** p<0.01

Figure 3 displays differences in postpartum diagnosis rates between eligible and ineligible women over time. Before 2011, diagnosis rates for both groups seemed to be trending parallel, which make plausible the common trend assumption needed for identification, and we can observe that rates start to diverge in 2011, increasing the gap between eligible and ineligible women.

Figure 3: Postpartum depression incidence rate by eligibility.

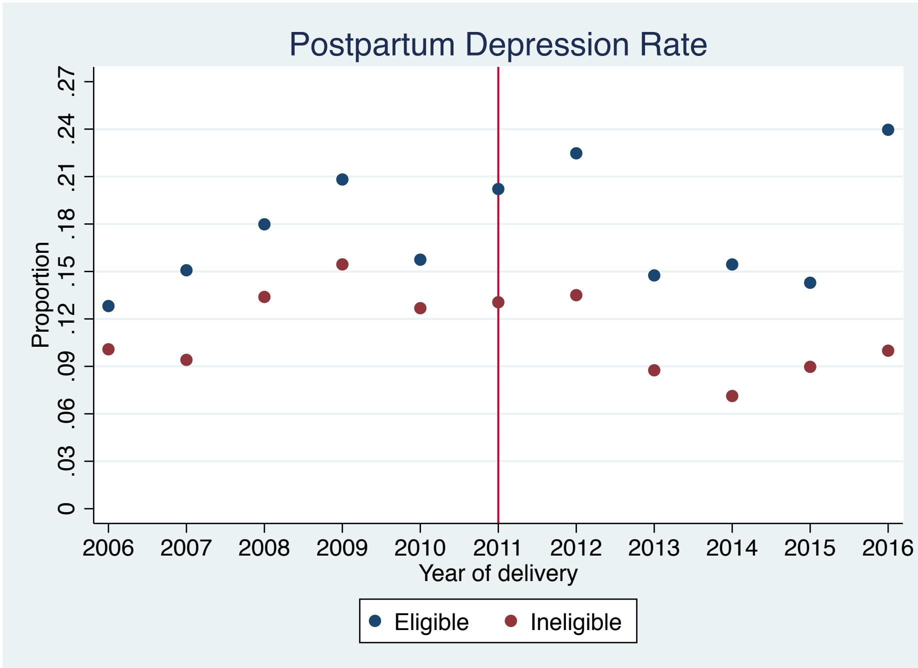


Table V shows the second stage results for the impact of an additional week of maternity leave on postpartum depression. Column 1 displays the results for a regression that includes year of delivery fixed effects and only controls for child's age in months at the time of the survey. Column 2 adds demographic controls to the specification in Column 1. Column 3 controls for pregnancy and delivery characteristics instead of demographic characteristics. When no demographic or pregnancy and delivery characteristics are used as controls, the point estimate is 0.39 percentage points. Including demographic controls slightly increases the point estimate to 0.43 percentage points. If pregnancy and delivery characteristics are used as control instead of demographics, the point estimate is reduced almost by half and loses statistical significance. My preferred specification is column 4, which includes year fixed effects and all control variables. This result suggests that taking one additional week of paid maternity leave increases the probability of being diagnosed with postpartum depression by 0.27

percentage points. This means that the total effect of the extra 12 weeks of leave allowed by the policy studied in this chapter is approximately 3.2 percentage points. Column 5 shows the estimate using the propensity score matching approach. The point estimate remains almost unchanged compared to the main specification in Column 4.

Table V: Second Stage results of the impact of one extra week of paid maternity leave on postpartum depression diagnosis.

	1	2	3	4	5
			Postpartum Depression		
Weeks of Maternity Leave	0.0039**	0.0043**	0.0022	0.0027*	0.0029**
	(0.00186)	(0.00181)	(0.00165)	(0.00162)	(0.0014)
Mean Control Group	0.125	0.125	0.125	0.125	0.1207
Year FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	Yes	No	Yes	No
Pregnancy and Delivery Characteristics	No	No	Yes	Yes	Yes
R^2	0.0229	0.0300	0.1344	0.1395	0.1140
Observations			16,345		
Effect of 12 extra weeks	0.0468	0.0516	0.0264	0.0324	0.0348

Robust standard errors in parentheses for columns 1 to 4. Column 5 uses propensity score matching. Bootstrapped standard errors in parentheses for column 5. All columns control for child's age in months at the time of survey. Demographic controls include age, marital status, schooling, total number of children, and birth order. Pregnancy and delivery characteristics include depression during pregnancy, planned pregnancy, health complications during pregnancy, complications during delivery, preterm infant and low birth weight infant. Data come from ELPI survey.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Being diagnosed with postpartum depression depends on both being depressed and visiting the doctor to get diagnosed. Hence, this increase in postpartum depression diagnosis could be reflecting an actual increase in depression, an increase in diagnosis due to a higher probability of seeking healthcare given a fixed level of depression, or a combination of both. Ideally, I would separate these possibilities by using data on both diagnosis and self-reported symptoms. Unfortunately, only the later waves of the data have information on postpartum depression symptoms, so there is no information on symptoms for women who gave birth prior to the policy. Thought this data limitation prevents estimating a difference-in-differences model, it is still informative to use the symptom data to examine the relationship between symptoms and treatment for eligible and ineligible women in the post policy period. To be clear, this analysis is not identifying the effect of the policy since there is no data on the pre-policy period. The 2012 wave of the ELPI survey evaluated symptoms of postpartum depression among women who had delivered between 6 and 12 months before the interview was conducted. Therefore, I only have data for women who delivered between August and December 2011. This evaluation was made using the Edinburgh Postnatal Depression Scale (EPDS). Looking at these data split by paid maternity leave eligibility status in Table VI, we observe that the proportion of eligible women who have symptoms of postpartum depression is smaller than the proportion of ineligible women, 31.4% versus 35.5% respectively. However, if we look at diagnosis rates for these two groups, we see that eligible women are much more likely to be diagnosed than ineligible women, 29.4% versus 15.3% respectively. This large difference in diagnosis rates despite the smaller difference in symptoms of depression suggests that diagnosis itself is playing an important role in my results. Though changes in diagnosis are not the same as changes in depression, it is important to note that changes in diagnosis are important in and of themselves. This is because women cannot be effectively treated for postpartum depression without being diagnosed.

Table VI: Symptoms of postpartum depression measured in 2012 and postpartum depression diagnosis rate for eligible and non-eligible women.

	Eligible	Ineligible
Symptoms of PPD	31.4%	35.5%
Postpartum Depression Diagnosed	29.4%	15.3%
Observations	51	124

Symptoms of postpartum depression measured by Edinburg Postnatal Depression Scale (EPDS) for women who at the moment of the evaluation had delivered between 6 and 12 months ago. Data come from ELPI survey.

In the next sections I examine different outcomes to provide additional indirect evidence on whether self-reported depression is increasing, or if diagnosis is increasing given a fixed level of depressive symptoms. First, I look at parental stress as a proxy for depression, along with other outcomes related to affective disorders. Stress is a very rough proxy for depression, but it is the closest self-reported mental health measure where data is available both before and after the policy. Then, I look at doctor visits, with special focus on mental health visits, and examine the mechanisms that affect healthcare access.

1.7.2. Depression-related outcomes

I start the analysis of depression-related outcomes by looking at parental stress. The ELPI survey measured the stress level of each child's primary caregiver in 2012 and 2017 using the Parenting Stress Index (PSI) Short Form (Abidin, 1995). PSI is a questionnaire that assesses the parent/caregiver perceptions of their child's characteristics, parents own characteristics and feelings about circumstances, and the interactions between child and parent. It has three main components: Parental Distress (PD), Parent-child Dysfunctional Interaction (PCDI), and Difficult Child (DC). The Parental

Distress section assesses parental stress that relates to factors associated with parenting. The Parent-Child Dysfunctional Interaction component assesses parental perceptions of their child's responses to their expectations and how these perceptions reinforce their parenting role. The Difficult Child section assesses the temperament of the child which impacts the parent-child relationship. A Total Stress score is calculated, which assesses the general levels of stress experienced by parents based on responses. Based on the score, an individual is categorized as having a normal level of stress, a high level of stress, or a clinically significant level of stress.

Because I use parental stress as a proxy for postpartum depression, I study changes in stress level among women who delivered at most 18 months before their stress level was assessed. Ideally, I would want to study stress only within the first 12 months after delivery, in order to match up with the period in which a mother can be diagnosed with postpartum depression. However, by doing so, I would not have any women who gave birth before the maternity leave period was extended. I stack 2012 and 2017 evaluations and I include wave fixed effects in the regressions. I study both the probability of having a high level of stress and a clinically significant level of stress in the total PSI score and each of its components. Table VII shows the results for having a high level of stress using equation 5. Because of the relatively small sample size of 959 women, these estimates are fairly imprecise and should be interpreted with caution. Taking the point estimates at face value, these results suggest that a longer leave has no effect on the probability of having a high level of parental stress. However, a longer leave seems to increase the stress related to having a difficult child. If by spending more time at home with the child a parent is more likely to be aware of certain child's behaviors, then it is not surprising to see an increase in stress related to having a difficult child. In Table VIII, I consider whether leave affects the probability of having a clinically significant stress level. These results are shown in Table VIII. Results are also imprecise, but they are qualitatively the same as those for a high level of stress. Overall, these results suggest that there is not an increase in stress level during the first 18 months postpartum due to this longer maternity leave.

Table VII: Effect of 12 extra weeks of paid maternity leave on the probability of being classified as having high stress level within the first 18 months after delivery (2012 and 2017 waves).

	Total PSI	Parental Distress	Parent-child Dysfunctional interaction	Difficult child
Eligible x Post	0.0066 (0.0669)	0.0172 (0.0767)	0.0120 (0.0582)	0.0369 (0.0540)
Mean control group	0.1675	0.2774	0.1257	0.1310
Observations	959	959	959	959

Robust standard errors in parentheses. All regressions control for child age in months, mother's age, schooling, marital status, total number of children, and birth order, and also include wave fixed effects. Data come from ELPI survey.
* p<0.1, ** p<0.05, *** p<0.01

Table VIII: Effect of 12 extra weeks of paid maternity leave on the probability of being classified as having clinically significant stress level within the first 18 months after delivery (2012 and 2017 waves).

	Total PSI	Parental Distress	Parent-child Dysfunctional interaction	Difficult child
Eligible x Post	0.0200 (0.0637)	0.0005 (0.0725)	0.0067 (0.0569)	0.0571 (0.0488)
Mean control group	0.1426	0.1952	0.1053	0.1179
Observations	959	959	959	959

Robust standard errors in parentheses. All regressions control for child age in months, mother's age, schooling, marital status, total number of children, and birth order, and also include wave fixed effects. Data come from ELPI survey.
* p<0.1, ** p<0.05, *** p<0.01

Since stress is an imperfect proxy for depression, I also study some life events that might trigger affective disorders to try to differentiate between diagnosis and symptoms effects. I specifically look at divorce and hospitalizations. Column 1 of Table IX shows results for the probability of being divorced after delivery using equation 5. There does not seem to be a differential increase in divorce for eligible women compared to ineligible women after the 2011 policy. The point estimate suggests a 0.5 percentage-point increase in the probability of divorce, although it is not statistically significant. This result reaffirms previous evidence that it is not likely that maternal postpartum mental health worsened after the 12-week leave extension.

Table IX: Effect of 12 extra weeks of paid maternity leave on life events that might trigger affective disorders. the probability of being hospitalized in the last 12 months.

	1	2
	Divorced	Hospitalized
Employed x Post		0.0153*
		(0.00796)
Eligible x Post	0.0051	
	(0.0111)	
Mean control group	0.0439	0.0327
Observations	16,345	11,751

Robust standard errors in parentheses. Both regressions include year fixed effects. Column 1 controls for age, schooling and number of children. Column 2 controls for age, schooling and marital status. Divorce data come from ELPI survey. Hospitalization data come from CASEN survey.
* p<0.1, ** p<0.05, *** p<0.01

Then I look at hospitalizations due to an illness or an accident in the last 12 months for women who delivered at least a year before the interview was conducted. Column 2 of Table IX reports the relevant coefficient for this outcome using equation 5. I find a significant increase in the probability of being hospitalized of 1.5 percentage points.

1.7.3. Diagnosis-related outcomes

Since it seems that maternal postpartum mental health did not worsen, I would expect that my main result is driven by an increase in diagnosis due to higher probability of seeking healthcare. I use equation 5 to estimate the effect of the maternity leave length extension on doctor visits. Columns 1 and 2 of Table X displays the relevant coefficients for any doctor visit and mental health visit. I find that this policy increases the probability of visiting any type of doctor in the last three months by over 1.4 percentage points, although this result is not precisely estimated. There is a significant increase in mental health visits, however. Three extra months of maternity leave increase the probability of visiting a mental health professional by 1.2 percentage points, which is a 57% increase. These results raise the question of what is driving this increase in doctor visits. One explanation could be an increase in time availability due to longer time off work after the benefit extension. Another explanation could be increased healthcare access due to higher probability of having health insurance. Unfortunately, my data do not allow me to examine the time availability mechanism, but I can look at employment and health insurance coverage after delivery to address the healthcare access mechanism.

Table X: Effect of 12 extra weeks of paid maternity leave on doctor visits in the last three months.

	1	2
	Any doctor visit	Mental health visit
Employed x Post	0.0141	0.0126*
	(0.0188)	(0.00742)
Mean control group	0.2795	0.0239
Observations	11,782	9,543

Robust standard errors in parentheses. All regressions include year fixed effects and control for age, schooling, marital status and health insurance. Data come from CASEN survey.
* p<0.1, ** p<0.05, *** p<0.01

I use equation 6 to study the causal impact of the maternity leave extension on short-run employment. First, I look at the effect on employment during the first year after delivery. Using data from the CASEN survey, I compare changes in employment before and after the policy change for women whose youngest child is less than one year old to changes in employment for women whose youngest child is between 5 and 10 years old. Column 1 of Table XI shows the results for this analysis. These extra 12 weeks of paid maternity leave significantly increased mothers of infants' employment by over 5 percentage points, which is a 14% increase with respect to baseline. This is a very large effect on employment, which could be driven in part by the fact that someone who is on maternity leave would be still counted as employed in my dataset.

Table XI: Effect of 12 extra weeks of paid maternity leave on short-run employment of mothers.

	1	2
	Employment	Employment
Treated x Post	0.0585***	0.0283**
	(0.00923)	(0.0101)
Mean control group	0.4112	0.4374
Observations	71,969	72,826

Robust standard errors in parentheses. Column 1 shows the effect of 12 extra weeks of paid maternity leave on employment of mothers of infants and Column 2 the effect of 12 extra weeks of paid maternity leave on employment of mothers of 1-year-old children. Regressions include year fixed effects and control for age, schooling, marital status and number of children. Data come from CASEN survey.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

The CASEN survey allows me to identify whether someone was on a leave at the time of the survey, but unfortunately, I cannot identify the type of leave. I try to address whether maternity leave affects employment in a way that is not mechanical by looking at employment of mothers of slightly older children, when maternity leave is over. Specifically, I estimate the same model (equation 6), but the outcome is employment when the child is one year old instead of employment when the child is less than one. Column 2 of Table XI reports this estimate and shows that the positive effect on employment persists even when children are one year old. The point estimate suggests an increase of 2.8 percentage points in employment, which is a 6.5% increase. Because mothers are more likely to return to work after the maternity leave ends than before the policy extension, I would expect this to translate into an increased health insurance coverage.

I start the discussion about health insurance coverage by providing a brief overview of the health insurance system in Chile. A large proportion of the population has health insurance coverage; less than 5% are uninsured. Approximately 15% are covered by a private insurance, and 80% of the population is covered by the public health insurance called FONASA. This public insurance program groups beneficiaries into four tiers: A, B, C, and D. Each tier differs in terms of the copayment needed at a public hospital and whether it offers coverage at a private provider. The characteristic that is most relevant for my analysis is whether the health insurance offers private provider coverage. Only those people within tiers B, C, and D have part of the cost of going to a private hospital covered by the insurer. Most, if not all, private insurances offer coverage at most private providers and public hospitals.

I get data on post-delivery insurance coverage from the CASEN survey. Using equation 5, I estimate differential changes between treatment and control groups in the probability of being uninsured and the probability of having a health insurance that offers private provider coverage. I define this last outcome as having private insurance or tier B, C or D of the public insurance. Table XII shows the results for both outcomes. Unsurprisingly given the large effect on employment, this longer maternity leave led to a significant 1.2 percentage-point decrease in the probability of being uninsured. This policy not only increased the proportion of mothers with health insurance, but also increased the proportion of them with better-quality health insurance. There is an increase of over 7 percentage points in the probability of having a health insurance that offers private provider coverage (a 14% increase). Therefore, I would expect that having a better health insurance would increase access to health care, especially mental health care, along with an increase in private provider visits.

Table XII: Effect of 12 extra weeks of paid maternity leave on health insurance coverage and place of last mental health visit.

	1	2	3
	Uninsured	Insurance with private provider coverage	Private provider
Employed x Post	-0.0124*	0.0757***	0.00971**
	(0.00715)	(0.0232)	(0.00482)
Mean control group	0.0203	0.5370	0.0055
Observations	10,615	10,615	9,542

Robust standard errors in parentheses. All regressions include year fixed effects and control for age, schooling and marital status. Data come from CASEN survey.
* p<0.1, ** p<0.05, *** p<0.01

I estimate the policy effect on the place of the last mental health visit. Column 3 of Table XII shows this estimate using equation 5. I find that eligible women are 0.9 percentage points (163%) more likely to visit a private provider for a mental health visit after the maternity leave period was extended. This result and the increase in mental health visits are in line with the increase in the probability of having health insurance coverage and the switch to a health insurance that provides coverage for private provider visits.

1.7.4. Depression Treatment

Finally, it is important to test whether this increase in postpartum depression diagnosis leads to an increase in depression treatment. The ELPI survey only provides depression treatment information in its 2017 wave, so it is not possible to estimate the difference-in-differences model with this outcome. However, it is informative to use the depression treatment data to examine the differences in treatment for eligible and ineligible women in the post policy period. Looking at this information, I observe that eligible women are not only more likely to be diagnosed with postpartum depression, but also more likely to seek treatment. Table XIII shows that over 17% of eligible women interviewed in 2017 were diagnosed with postpartum depression versus only 9% of ineligible women. Of those diagnosed with postpartum depression in the eligible group, 86% received treatment; while in the ineligible group only 65% of women who were diagnosed received treatment. Similarly, the proportion of women diagnosed with depression in the 12 months prior to the interview is higher among those women eligible for extended leave: 9% versus 6% of ineligible women. Among these two groups, treatment rates are similar to those for postpartum depression treatment: 85% for eligible women and 66% for ineligible women diagnosed with depression.

Table XIII: Postpartum depression and general depression diagnosis and treatment rates for 2017 wave of ELPI survey.

	Ineligible	Eligible
Postpartum depression diagnosed	0.0941	0.1768
Postpartum depression treated	0.6551	0.8642
Depression diagnosed in last 12 months	0.0608	0.0929
Depression treated in last 12 months	0.6613	0.8576
Undiagnosed depression in last 12 months	0.2655	0.2025
Observations	2,554	1,663

In order to estimate the causal effect of maternity leave on depression treatment, I use data from the CASEN survey. Table XIV shows the relevant coefficient using equation 5. I do not have power to identify a precise treatment effect, but the point estimate suggests a 0.5 percentage-point increase in the probability of being treated for depression, which is an almost 26% increase with respect to the control group. This 26% increase is in line with the almost 26% increase in postpartum depression diagnosis found earlier using the preferred specification. One caveat in interpreting this result is that there is likely measurement error in the outcome variable. The CASEN survey only records the most severe disease for which a person was treated in the past year. If the treatment group (employed women) is systematically more likely to seek treatment for any disease given that they have better health insurance, then it is likely that the underreporting of depression treatment for employed women is larger than that for not-employed women, leading to downward bias. In that case, this estimate could represent a lower bound of the true treatment effect.

Table XIV: Effect of 12 extra weeks of paid maternity leave on the probability of being treated for depression in the last 12 months.

	Treated for Depression
Employed x Post	0.00555
	(0.00838)
Mean control group	0.0215
Observations	7,619

Robust standard errors in parentheses. Regression includes year fixed effects and controls for age, schooling, marital status and health insurance. Data come from CASEN survey.
* p<0.1, ** p<0.05, *** p<0.01

1.7.5. Longer-term outcomes

Depression can be a chronic condition for over a quarter of patients (Derubeis et al., 2008). Identifying and treating depression could lead to a better mental health in the longer term, as well as improvement in other outcomes related to maternal wellbeing. I start this analysis by studying if this longer leave that led to an increase in postpartum depression diagnosis had any impact on depressive symptoms between 4 and 9 years after delivery. I use depressive symptoms data, measured with the Center for Epidemiological Studies Depression Scale (CES-D), from the ELPI survey. Column 1 of Table XV displays the results for this analysis using equation 5. There is not a statistically significant effect on depressive symptoms. Eligible women are less likely to be depressed than ineligible women, both those who delivered before and after the 2011 policy change; however, there is a narrowing of the gap, which is driven by a decline of 1 percentage point in the probability of being depressed among the ineligible group. Women who delivered before the maternity leave expansion in 2011 and were eligible to take paid maternity leave at childbirth are as likely to be depressed as their counterparts who delivered after the policy change in 2011. This is similar to what Baranov et al. (2018) observed among a group of Pakistani mothers: the share of women in the treatment group who were assessed as having no depression 7 years after intervention was similar to that 1-year post intervention, while the control group had lower depression rates due to spontaneous recovery within that period of time.

Table XV: Effect of 12 extra weeks of paid maternity leave on longer-term outcomes.

	1	2	3
	Depressed	Divorce	Employment
Eligible x Post	0.0139	0.0009	0.0665**
	(0.0315)	(0.0205)	(0.0319)
Mean control group	0.2864	0.0750	0.5535
Observations	5,791	6,309	6,309

Robust standard errors in parentheses. All regressions include year of delivery fixed effects. Columns 1 and 3 control for age, marital status, schooling, age of child, and number of children. Column 2 controls for age, schooling and number of children. Column 1 is the probability of being depressed as measured by depressive symptoms using the CES-D scale. Data come from ELPI survey.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Using the same sample as in the previous analysis, I also look at the impact on divorce and employment. Columns 2 and 3 of Table XV shows the relevant coefficients for both outcomes using equation 5. There is no difference in the probability of being divorced in 2017 between eligible women and ineligible women who delivered after the maternity leave expansion. On the other hand, there is a large significant effect on employment. Even after 4 years postpartum, there seems to be an increase of 6 percentage points (12%) in employment of women who were exposed to the longer maternity leave compared to those who were not exposed to the extended leave period. Both the divorce and employment results are not consistent with mental health worsening. If getting divorced causes depression or if being depressed leads to divorce, a null effect on the probability of being divorced is loosely suggestive of no change in mental health in the longer-term. In the same way, a higher probability of employment would be associated with better mental health either if not being employed causes depression or if being depressed makes you less likely to be employed. Overall, a longer paid maternity leave seems to improve maternal wellbeing in the longer-term.

1.8. Conclusions

I study the causal effect of maternity leave period on maternal postpartum mental health by using exogenous variation in maternity leave length generated by a 12-week extension in Chile. This longer maternity leave increased the probability of being diagnosed with postpartum depression by almost 0.3 percentage points (2.3% increase) for every extra week of paid leave. I show that this seemingly adverse effect of maternity leave extension most likely reflects an increase in diagnosis due to the increased probability of seeking healthcare rather than worsening of mental health. I find no evidence of an increase in maternal stress level during the first 18 months after childbirth using the Parental Stress Index (PSI). Furthermore, most of my evidence on life events that might trigger depression also yields findings that are inconsistent with maternal postpartum mental health worsening as a result of the policy. Additionally, Albagli and Rau's (2018) find that this this maternity leave expansion improved children's cognitive and non-cognitive skills, which again would not be consistent with a scenario where mothers' mental health was worsening.

This study also tests additional mechanisms behind the increased in diagnosis. I find that the policy leads to a significant increase of 1.2 percentage points in the probability of visiting a mental health professional during the first year after delivery. This increase seems to be driven by improvements in health insurance coverage: a reduction in the probability of being uninsured and an increased probability of having an insurance that provides coverage at private healthcare providers. Women get access to better health insurance by remaining attached to the labor market after childbirth. After the maternity leave extension, maternal employment increased by 5 percentage points during the first year postpartum and by almost 3 percentage points in the following year.

Overall, my results suggest that providing women with more paid time off work to recover and take care of their newborns can have positive effects on their mental health and wellbeing. By getting their affective disorder diagnosed, women can get treatment, as my analysis suggests that this increased diagnosis seems to have translated into an increase in depression treatment of 26%, although this is not statistically significant. I find suggestive evidence of longer-term improvements in employment.

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2. Impact of Chilean Maternity Leave Expansion on Female Labor Market Outcomes & Gender Discrimination

2.1. Introduction

The effects of maternity leave benefits on female labor market outcomes such as labor force participation, employment, and wages have been studied for developed countries, especially European countries. However, there is little evidence for less developed countries. These countries tend to have greater gender gaps in labor market outcomes such as employment rates and wages than developed countries. For example, Chile is the OECD country with the worst income distribution and the highest gender pay gap. This gap gets worse as women get more schooling: highly educated women make 37 percentage points less than men, conditional on education level. Part of these gender differences are attributed to the conventional social role of women as caregiver of children, which influences women's decision of participating in the labor force. Maternity leave policies are designed to help mothers recover after giving birth and help them bond with their newborns, while providing mothers job security and income replacement. As Rossin-Slater, Ruhm, and Waldfogel (2013) point out, short maternity leaves may increase female labor force attachment, while longer leaves might have a negative effect on women's position in the labor market due to possible human capital depreciation and because employers might view childbearing age women differently from other workers.

In this chapter, I study how increasing maternity leave benefits affects labor market discrimination against women in Chile. I use a policy change that in 2011 increased the paid maternity leave period from 18 weeks to 30 weeks, to study its impact on female labor force participation and employment rates, and on the gender pay gap. This study uses CASEN survey data from the Chilean Ministry of Social Development, and a differences-in-differences approach that compares pre- versus post-policy implementation labor market outcomes. In order to assess the impact of the policy on labor market discrimination against women, the strategy is to compare outcomes of childbearing age women (18 to 38 years old), the treatment group, to older women's outcomes (39 to 50 years old), the control group. In the preferred econometric specification, I find robust evidence that the 2011 maternity leave policy change reduced labor force participation of childbearing age women by 3 percentage points, while it had no effect on the gender pay gap. These findings contrast to those of Ruhm (1998), who found that paid parental leave mandates in nine European countries led to an increase in women's employment and a reduction in their relative wages.

This study expands the literature on the impact of maternity leave policies on female labor market outcomes on several dimensions. First, it studies if a policy that intends to help and improve the labor market position of mothers of young children and women who desire to become mothers, has any impact on labor market attitudes towards women in general. Second, this study provides evidence from a less developed country of the impact of extending maternity leave benefits on women's labor force participation and employment rates, and the gender pay gap.

2.2. Conceptual framework

The effects of maternity leave rights on employment are theoretically ambiguous (Klerman and Leibowitz (1994)). After giving birth, a woman can be on leave from her job or not employed. If the labor market is competitive, with perfect information and no externalities, maternity leave policies diminish economic efficiency by restricting employers' and workers' ability to voluntarily choose the optimal compensation package (Ruhm (1998)).

Kamerman (1998) argues that maternity leave rights reduce unemployment among women and can increase firm-specific human capital by decreasing women's necessity to switch jobs if they desire to spend more time with their newborns. If labor markets are competitive, then those more likely to use maternity leave will pay for it by obtaining a lesser compensation, which implies that childbearing age women will get lower compensation if the leave is mandatory (Ruhm (1998)).

Moreover, if the leave period is long enough, employers might limit women to positions where absences are less costly, leading to increased occupational segregation[4].

Under the presence of asymmetric information, an adverse selection problem might arise as a big proportion of "high-risk" childbearing-age women might self-select into companies that are required to or voluntarily provide more maternity benefits, which will force these companies to pay lower wages. On the other hand, "low-risk" women will sort out of these firms. As Ruhm (1998) points out, a government policy could potentially eliminate the incentives for this sorting and raise welfare.

Maternity leave entitlements will likely induce a rightward shift of the labor supply curve for those groups more likely to use the benefits. The demand curve would simultaneously shift to the left to the extent that non-wage costs increase[5], as the maternity leave benefits are paid by with public funds in Chile. It is important to mention that increase in costs would be partially offset by a reduction in employers' childcare expenses, as the Chilean legislation requires companies hiring twenty or more women to provide childcare for their female employees' children from the moment women return to work after maternity leave until the child turns 2 years old. Therefore, it can be expected that the shift in the labor supply is going to be larger than the shift in the labor demand, which would result in an increase in the relative employment of childbearing age women and a drop of their relative wages in equilibrium. Employment might be reduced right after childbirth due to increased leave-taking; however, as Klerman and Leibowitz (1997) argue, employment could increase during this time period if some women who otherwise would have quitted their jobs to spend more time at home than previously allowed by the leave policy, now remain in their jobs and return to the workforce sooner than before.

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[4] See Stoiber (1990) for some evidence on this issue in Sweden.

[5] Expenses associated with hiring and training temporary replacements, or efficiency losses in case of not hiring a replacement.

There are some arguments for a shift to the right of the labor demand curve. There could be productivity gains if longer maternity leave increases firm-specific human capital by allowing women to remain in their jobs. This demand shift will further increase employment and partially or completely offset the decline in wages (Ruhm (1998)).

2.3. Literature Review

Studies for Canada and Europe have consistently found that the take-up of paid maternity leave mandates is close to universal (Baker and Milligan (2008), Dustmann and Schonberg (2012), Burgess et al. (2008)). In the case of Chile, the take-up is almost universal for all eligible women.

For the case of Europe, studies such as Ruhm (1998), Dustmann and Schonberg (2012), and Gregg et al. (2007) have found mixed effects of maternity leave policies. Ruhm (1998) studies the economic consequences of paid parental leave rights in nine European countries from 1969 to 1993. He finds that paid leave rights raise the percentage of women employed (female employment-to-population ratio) between 3 and 4 percentage points, with a substantial effect for shorter durations of guaranteed work absence. Short leave periods have small effect on women's earnings, but longer leaves are associated with a substantial reduction in relative wages of between 2 to 3 percentage points. Dustmann and Schonberg (2012) analyze different maternity leave policy changes in Germany, finding a substantial effect on mothers' short-run labor supply; however, they only find a small effect on the proportion of women who returned to the labor market after the job protection period had expired (long run effect). They also find no support that the expansions in leave coverage improved children's long-term outcomes, such as children's educational attainment, high track school attendance, and wages at the age of 28. Similarly, Dahl et al (2016) study the effects of a series of policy reforms that expanded paid maternity leave from 18 to 35 weeks in Norway. Using a regression discontinuity approach for each of the 6 reforms they evaluate, they find a small effect on children's schooling, parents' earnings and labor force participation, fertility and marriage. In the United Kingdom, Gregg et al. (2007) document that maternity rights have had a great effect on employment, which varies by mothers' wage opportunities. Maternity leave policies induce a change in mothers return to work behavior: several women who previously would have returned to work only when their children were 3-5 years old are now returning to work within the first year. Their evidence suggests that this effect is most marked among better educated and higher earning mothers.

Labor markets tend to punish maternity. As Waldfogel (1998) points out, there is a "family gap" between the wages of mothers and non-mothers in the United States and the UK. About 40% to 50% of the gender gap is explained by differential returns to marital and parental status. Women's lower level of experience and lower returns to experience explain another 30% to 40% of the gap. Maternity leave policies affect mainly younger women in fertile age; hence, such policies might have a role to play in explaining gender pay gap. In her research, Waldfogel (1998) documens that among women who had maternity leave coverage and returned to work after childbirth, the negative wage effects of children were offset by receiving a wage premium, suggesting that maternity leave reduces the family gap. Similarly, Baker and Milligan (2008), using Canadian data, find that the introduction of modest leaves of 17-18 weeks rises the fraction of mothers employed and on leave, but has almost no effect on leave duration.

Turning the attention to the United States, Rossin-Slater, Ruhm, and Waldfogel (2013) studied the effects of California's Paid Family Leave program, which took effect in 2004, on mothers' leave taking and labor market outcomes. The authors found evidence that this California program doubled the use of maternity leave, with evidence of especially large growth for less advantaged groups, such as unmarried women, black and Hispanic women. Their findings also suggest that the program raised weekly work hours of mothers of children between 1 and 3 years old in the range of 10-17%.

Low and Sanchez-Marcos (2015) develop a life-cycle model of women's labor supply and savings behavior adjusted to the economy of the United States to study the effect of introducing a Scandinavian-type maternity leave policy on gender differences in wages and participation rates. They differentiate between the job protection effect of the maternity leave and the income replacement effect. They find that job protection leads to increased participation of mothers with children under 6 years old and it has minor effects on wages, with the negative selection effects offsetting the reduced human capital depreciation. On the other hand, they found that the income replacement effect was limited on participation and wages.

2.4. Institutional background

Chilean working women have had maternity benefits for over 100 years now[6]. It was in 1917 when the first benefit was introduced. By then, those companies employing 50 or more women 18 or older needed to provide a space in their premises where women could leave their children younger than 1 year old, and at this place, women could also breastfeed their children for up to one hour a day. This time feeding their babies had to be paid. Later, in 1925 the first maternity leave benefit was enacted. This benefit consisted of 60 paid days of leave, 40 days before giving birth and 20 days after giving birth. Women were entitled to receive 50% of their salary, and it was completely paid by their employers. Years later, in 1931, the leave period was extended to a total of 12 weeks and the cost of the leave was now shared by the employers and the social security system. By 1952, income replacement was increased to 100% and was fully paid by the social security system.

In October of 2011, maternity leave benefits were extended for working mothers. The new policy allows women to have extra 12 weeks of paid postnatal maternity leave if they choose to absent

[6] Romanik (2014).

from work full-time (for a total of 24 weeks), or 18 extra weeks if they choose to be away from work part-time (half of their regular working time, for a total of up to 30 weeks). The legislation also introduced a third option, women can transfer part of the maternity leave to the working father of the child after the sixth week of the new leave period for the number of weeks that the mother indicates, for up to 6 weeks of full-time leave or 12 weeks of part-time leave. In this last case, the amount of the subsidy is determined according to the father's salary. This benefit also applies to self-employed mothers that are part of the social security system.

The subsidy is for the full salary amount of the beneficiary and it is paid with public funds, which are financed with general taxes. The amount of the subsidy is capped at the maximum amount used to determine the contribution to the social security system[7]. This provides lower benefits for highest earning women. Eligibility requirements are easily met: women need to have had contributed to the social security system for the first time at least 6 months before the beginning of the prenatal leave and had been working for three continuous months before the beginning of the prenatal leave.

The Chilean legislation includes additional benefits for women during pregnancy and after giving birth. A pregnant woman cannot be fired during her pregnancy and her job is also protected for one year after the end of the first 12 weeks of the postnatal leave. Once the mother has come back to work, she has the right to absent from work for one hour a day in order to feed her newborn until the child turns 2. Also, those firms that employ 20 or more women are required to provide childcare to their workers' children until they turn 2. These benefits impose higher costs of hiring women, especially in fertile age, which should explain part of the gender pay gap. According to OECD statistics, Chile is the OECD country with the highest mean gender pay gap conditional on education level, the pay gap among full-time-employed highly-educated men and women is over 37 percentage points, while the gap for full-time-employed low-skilled workers is around 23 percentage points.

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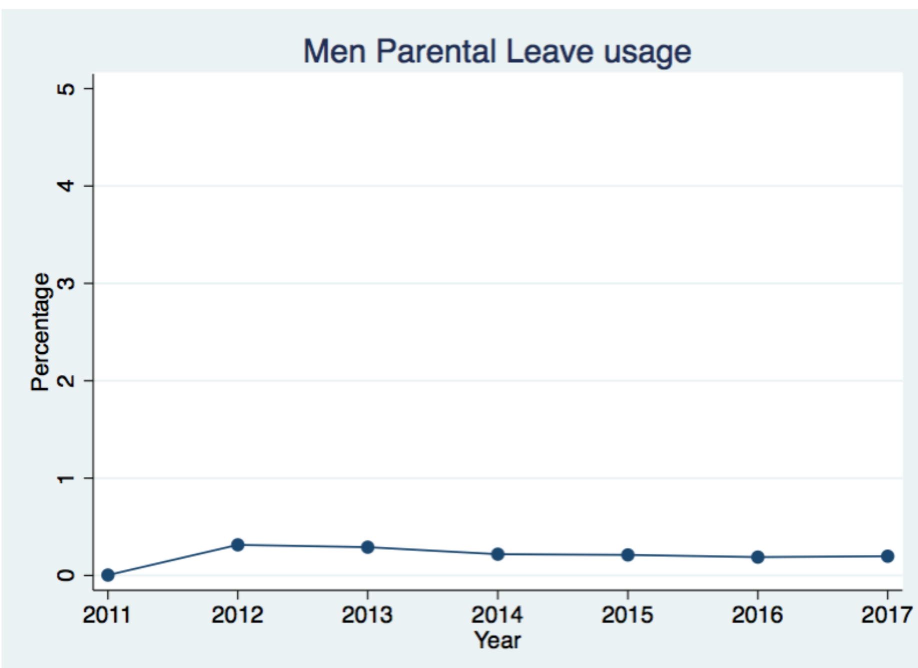
[7] Approximately USD $3,300 per month.

One of the objectives of the extension of the maternity leave benefit was to reduce the costs associated with hiring women. This new benefit was expected to reduce uncertainty regarding when women will reincorporate to work, as it was a common practice to use the paid sick leave in case of serious illness of children younger than one year old. Also, there is a reduction in childcare costs for employers. Now employers have to finance 12 weeks less of childcare. Part of the cost of having children could be assume by fathers' employers, if men make use of the leave. Hence, hiring women should be relatively less expensive compared to men than before. However, men are not using the benefit, as it can be seen on Figure 4.

Figure 4: Parental Leave usage by men



Note: Proportion of eligible men taking the parental leave.

The proportion of men using the benefit has remained relatively constant at around 0.3%. Figure 4 shows men's usage of the extra 12 weeks of maternity leave benefit since its implementation in 2011.

Maternity leave policies differ vastly in terms of duration of the benefit, job protection and income replacement across countries. The International Labour Organization standard is 14 weeks, so with the new policy, Chile is exceeding this standard by 16 weeks, but it is still below the OECD average of 52.6 weeks as of 2015. Among OECD countries, the average income replacement rate offered by maternity leave policies for a mother on average wages is approximately 79%, with 13 OECD countries providing full earnings replacement (in most cases with a cap). Replacement rates tend to be lower in English-speaking countries, such as Canada, Ireland, the United Kingdom, Australia, and New Zealand, which replace less than 50% of previous earnings on average. Higher income mothers tend to have lower replacement rates due to the ceilings of the maternity leave benefits. For instance, in the Netherlands and Norway, the relatively low caps mean that effective replacement rate for a mother making 1.5 times the average earnings is around 30-40 percentage points lower than that for a mother on average earnings (OECD, 2016). Eastern European and Nordic countries offer the most generous benefits: several months of paid leave, possibility of unpaid leave, and extended period of job protection.

2.5. Data

For this chapter, I use repeated cross-sectional survey data from the Chilean Ministry of Social Development. The CASEN survey consists of a representative sample of households of the whole country. It collects individual data on demographic characteristics, education, employment, income, health, and housing variables. I use all the available waves from 2003 to 2015.

My sample consists of 721,055 women and men between 18 and 50 years old. Women account for 51.51% of the sample. Table XVI shows a summary of descriptive statistics of the sample by gender and cohort in 2009. The younger cohort consist of people between 18 and 38 years old, and the older cohort comprises people between 39 and 50 years old. Women are a bit older than men, on average, more likely to be single (except younger women), and more likely to live in an urban area. Women also have a higher level of schooling, 10.2% of younger women have a college degree versus only 7.9% of younger men. Among the older cohort, gender college attainment difference is only 1 percentage point. In the same way, the greatest difference in schooling is across cohorts, younger men and women get 2 more years of schooling on average than their older counterparts. Despite this big difference in schooling across cohorts, older women make 9.5% more than the younger cohort and this cohort difference for men is 28.1%, suggesting that experience and/or tenure play an important role determining earnings. Surprisingly, despite the fact that women are more educated than men, they make significantly less than men (25.26% less on average). The unconditional gender pay gap is larger for the older cohort than the younger cohort, 31 percentage points versus 19 percentage points, respectively.

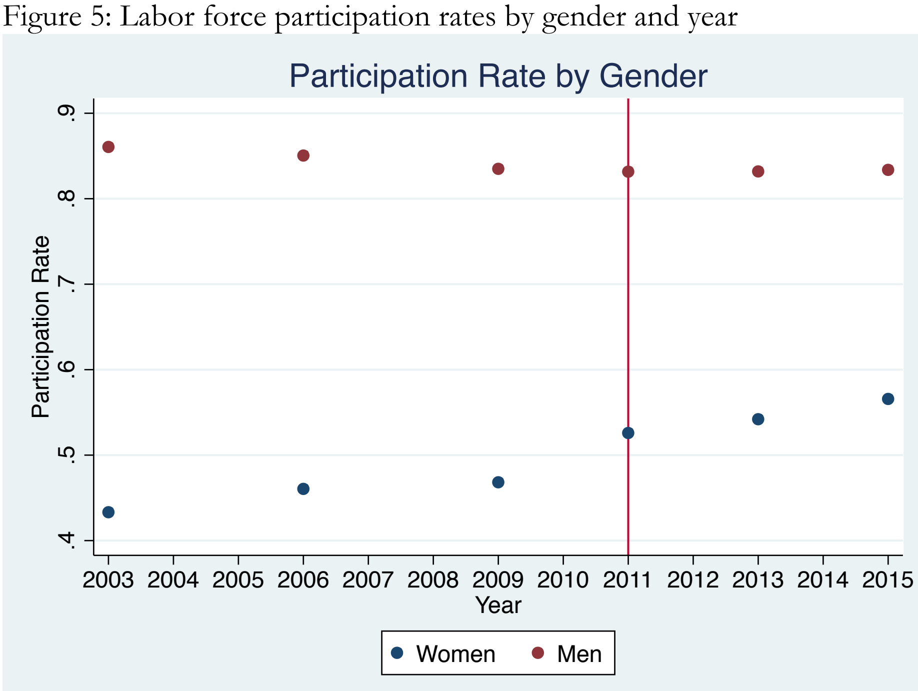
Analyzing the labor force participation rate by gender, I find that men's participation rate slightly declined until 2009 and from there onwards has remained stable around 83%. Female participation rate, on the other hand, has been continuously increasing, with a hike of almost 10 percentage points between 2009 and 2011. By 2015, women's participation was close to 60%. Figure 5 depicts the participation rates by gender for the sample period. Similarly, employment-to-population ratios by gender follow the same pattern, which is shown in Figure 6.

Table XVI: Descriptive statistics by gender and age group

	Older Men	Younger Men	Older women	Younger women
Age	44.6	26.9	44.5	27.2
Schooling	9.16	11.15	9.31	11.41
College degree	7.6	7.9	8.5	10.2
Single	25.1	63.5	28.3	55.3
Urban	61.8	65.5	65.7	66.9
People in household	4.2	4.5	4.3	4.6
Head of household	71	26.8	22.6	9.6
Number of children	1.47	0.46	1.64	0.74
Have child 4 and younger	10.9	13.9	7	18.7
Have child 2 and younger	5.8	8.7	3	11.5
Monthly labor income[8]	506,812	395,630	349,772	319,272
Labor force participation rate	92.1	77.9	49.8	48.3
Employment rate	87.2	68.2	45.5	39.7
Observations	120,665	228,933	132,998	238,459

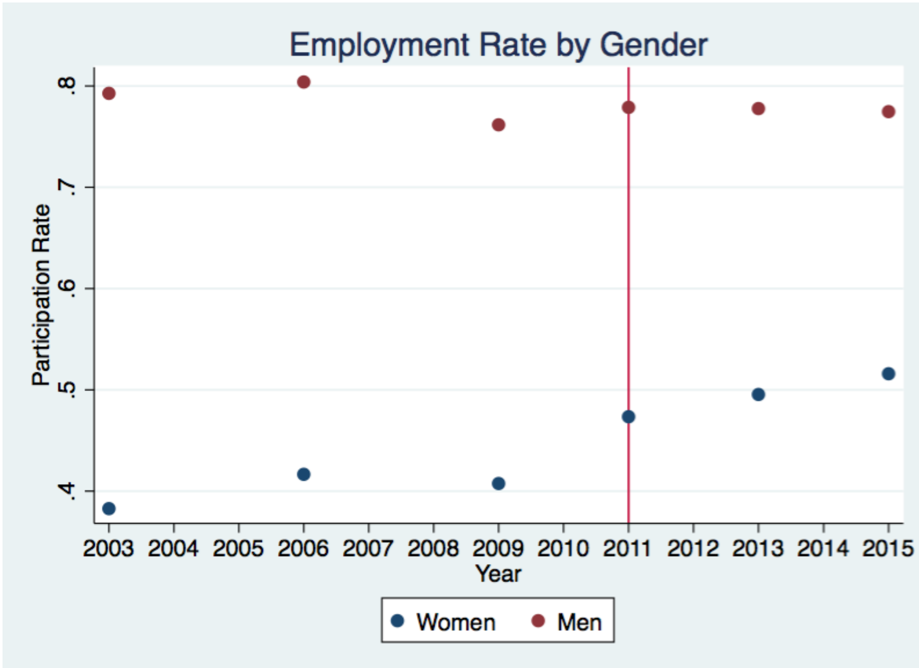
Notes: Younger women and men include 18- to 38-year-old people. Older women and men include 39- to 50-year-old people.

Figure 5: Labor force participation rates by gender and year



[8] Figures in Chilean pesos (CLP). Approximately 600 CLP = 1 USD.

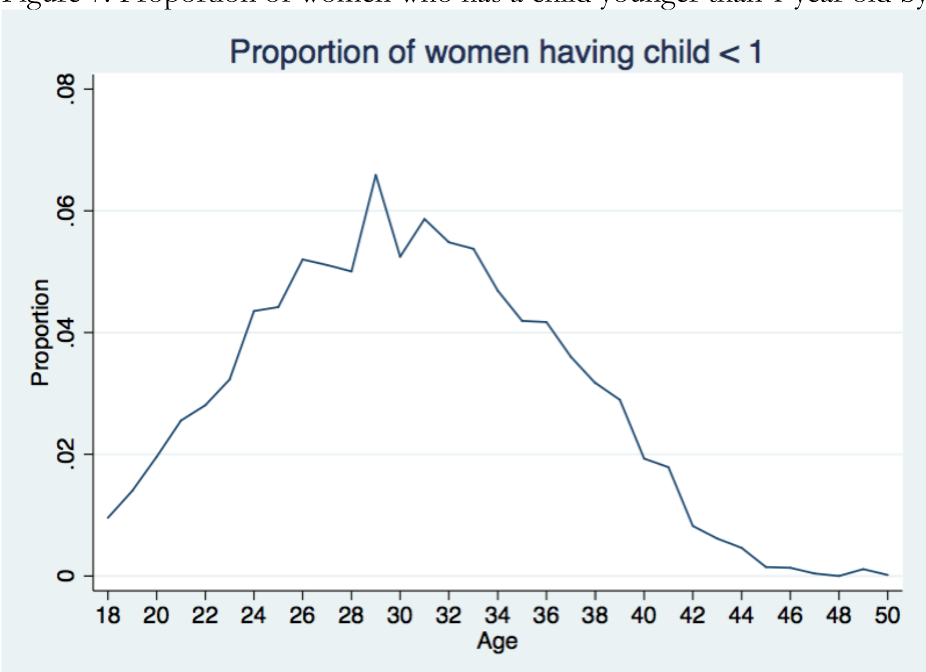
Figure 6: Employment-to-population ratios by gender and year



There is only a 1.5 percentage points difference in labor force participation rate between childbearing age women and their older counterpart, while young women's participation is 48.3%, older women's participation is almost 50%. However, difference in employment-to-population ratio between 18- to 38-year-old women, treatment group, and 39- to 50-year-old women, control group, is larger: 5.8 percentage points higher for older women.

Younger women have slightly larger households than older women. While women between 39 and 50 years old have 1.64 children living in the household, childbearing women have only 0.74 children on average. Analyzing the age of the children in the household, unsurprisingly 19% of younger women have at least one child who is 4 or younger versus 7% of older women. Similarly, 11.5% of childbearing age women have at least one child who is 2 or younger, while only 3% of older women do. Finally, looking at infants in the household, Figure 7 shows that the proportion of women who have a child younger than one year old starts to drop after turning 30 years old, and by age 39 only 2% of women are mother to infants, by age 45 almost no women have recently given birth. This provides some support to the validity of choosing the older women group as the control group for the labor market discrimination question.

Figure 7: Proportion of women who has a child younger than 1 year old by age.



2.6. Empirical Strategy

I use a differences-in-differences approach that compares changes in labor market outcomes for the treatment group to changes in outcomes for the control group before and after the policy change in 2011. Since maternity leave policies are more likely to affect only women in childbearing age, the treatment group is composed of women between 18 and 38 years old. As a control group, I use older women between 39 and 50 years old, since they are less likely to get pregnant and hence should be viewed by employers as unlikely to use the policy benefits[9].

For employment rate and labor force participation, I estimate the following equation:

$$y_{it} = \beta_0 + \beta_1 Treatment_i \times Post_t + \gamma_t + \zeta_a + \mathrm{X}'\delta + \varepsilon_{it} \qquad (1)$$

Where y_{it} is a dummy variable for the relevant outcome for individual *i* surveyed in year *t*, Treatment is a dummy indicating being 18 to 38 years old, Post is a dummy equal to 1 for 2011 onwards, γ_t represents year fixed effects, ζ_a are age dummies, and X is a vector of covariates such as schooling, marital status, number of children, etc. The coefficient of interest is β_1, which will tell us the change in employment rate or participation rate for childbearing age women that is attributable to the maternity leave policy.

As a way to test for differential pre-trends and examine how the treatment effect varies across time, I estimate the following variation of equation 1:

$$y_{it} = \sum_{k=1}^{6} \beta_k Treatment_i \times Year_t + \gamma_t + \zeta_a + \mathrm{X}'\delta + \varepsilon_{it} \qquad (1a)$$

Each β_k coefficient represent the treatment effect for each of the 6 years of data used in this study. If the common trend assumption is plausible, then all β_k until 2009 should be zero.

[9] Even though it is biologically possible for a woman to get naturally pregnant after age 40 and there are higher chances with assisted methods (IVF), there seems to be a social age deadline for childbearing of women, which is perhaps more important. Billari et al (2011) show that across 25 European countries a maternal social age deadline of ≤40 years of age is perceived for the majority of the population.

To study the effects of the maternity leave extension on the gender pay gap, I estimate the following equation:

$$y_{it} = \beta_0 + \beta_1 Female_i + \beta_2 Treat_i \times Post_t \times Female_i + \beta_3 Treat_i \times Female_i + \beta_4 Post_t \times Female_i + \gamma_t + \zeta_a + \pi_o + \mathrm{X}'\delta + \varepsilon_{it} \quad (2)$$

where y_{it} is the log of monthly labor income for individual *i* surveyed in year *t*, Treat is a dummy indicating being 18 to 38 years old, Post is a dummy equal to 1 for 2011 onwards, Female is dummy variable for gender, γ_t represent year fixed effects, ζ_a are age dummies, π_o represents occupation fixed effects, and X is a vector of covariates such as schooling, experience, etc.

For this outcome, I also estimate a variation of equation 2 (equation 2a) in order to test for pre-trends and differential treatment effects over time. Each β_k coefficient before the year of implementation of the policy, 2011, is expected to be zero.

$$y_{it} = \sum_{k=1}^{6} \beta_k Treat_i \times Year_t \times Female_i + \beta_7 Female_i + \beta_8 Treat_i \times Female_i + \sum_{j=1}^{6} \beta_j Year_t \times Female_i + \gamma_t + \zeta_a + \pi_o + \mathrm{X}'\delta + \varepsilon_{it} \quad (2a)$$

One condition to get consistent estimates with a differences-in-differences strategy is the common trend assumption. In this case, the identification assumption is that absent this maternity leave policy change, outcomes for childbearing age women and older women would have trended in the same way. Figure 8 shows labor force participation rates since 2003 until 2015 for both groups. It can be seen that before the policy change, both groups followed a similar upward trend on the participation rate, making plausible the common trend assumption. Between 2009 and 2011 there was a big jump in the participation rate for both groups[10].

Figure 8: Female labor force participation Rates by age group and year.

Employment-to-population ratio followed a similar pattern, which is shown in Figure 9. Figure 10 show the estimated conditional gender pay gap for childbearing age women and for older women. Gender pay gap for both groups was trending in the same way before the policy change, which gives support to the plausibility of the common trend assumption for this outcome.

[10] This increase coincides with the economic recovery after the 2009 recession and the reconstruction work after the big earthquake of February 2010. During this period there was a generalized increase in employment for both men and women.

Figure 9: Female employment-to-population ratios by age group and year.

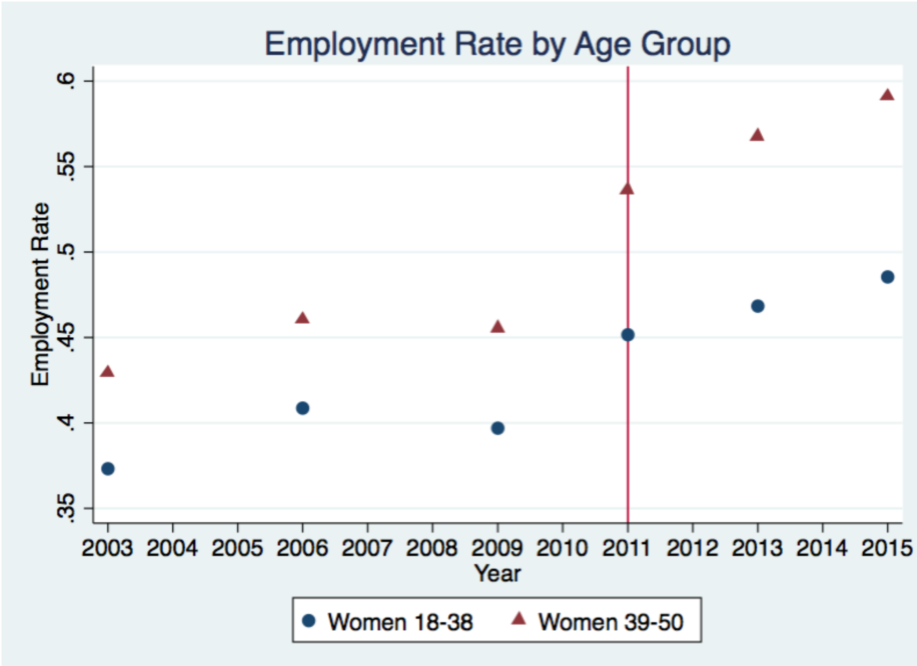
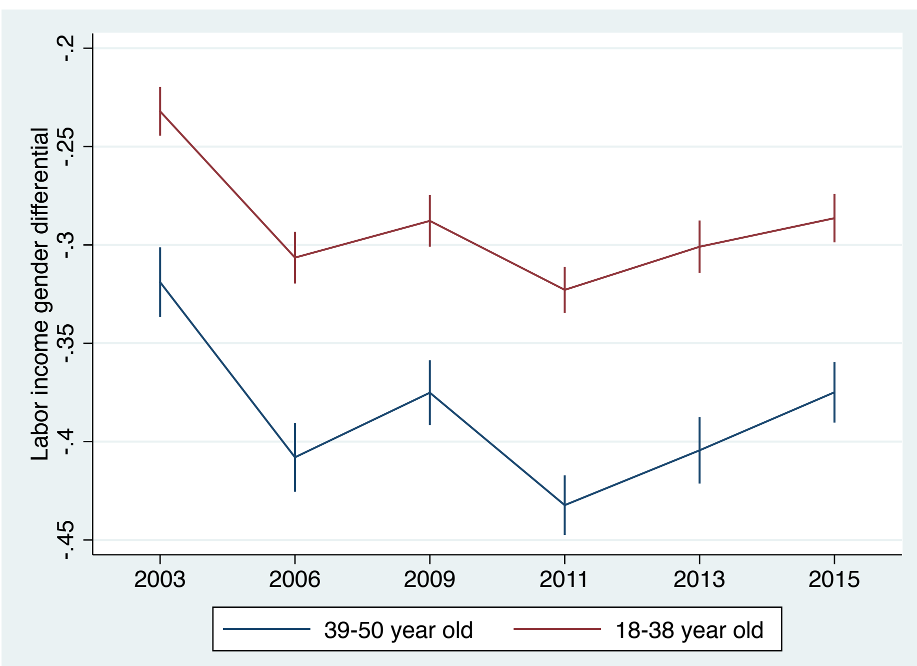


Figure 10: Conditional gender pay gap by year.

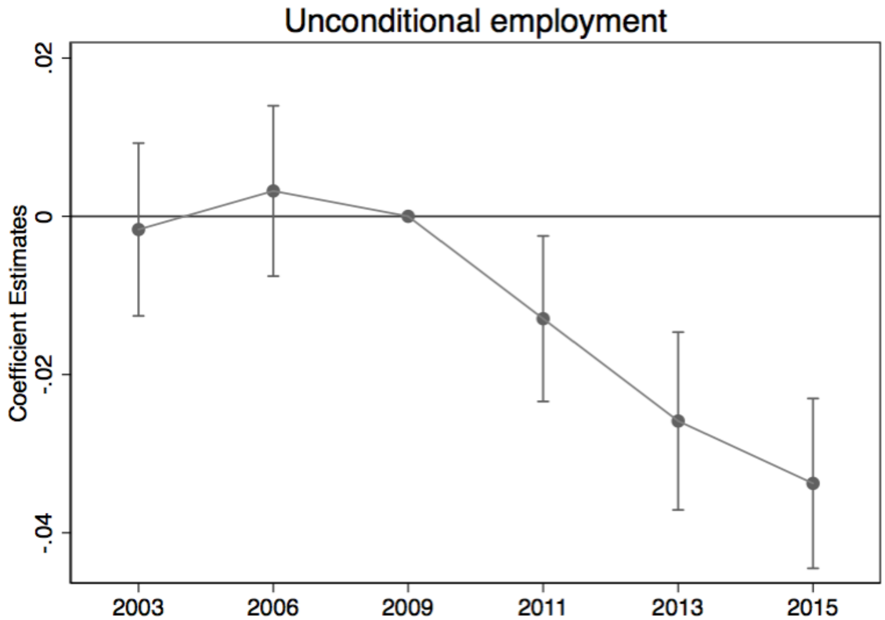


Notes: Point estimates and 95% confidence intervals are plotted. Gender pay gap is estimated conditional on schooling level and experience for each year.

2.7. Results

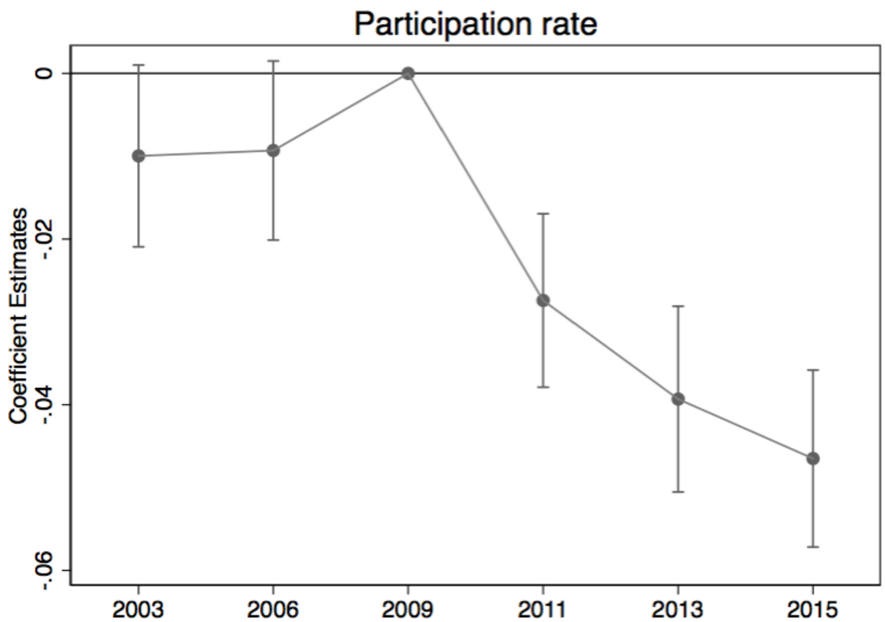
In this section I investigate how the extension of 12 weeks in paid postnatal leave affected employment, labor force participation and the conditional gender pay gap for childbearing age women. I first look at female employment and labor force participation effects. One key concern with difference-in-difference designs is the common trend assumption; hence, my first approach is to do a detailed event study which will tell me about the pre-trends and the behavior of the treatment effect over time. Figure 11 shows the estimates for the policy effect on employment-to-population ratios by year, and Figure 12 shows the estimates for labor force participation rates by year. As expected, "treatment" effects before the policy took place in 2011 are statistically indistinguishable from zero, which suggests that both groups' unconditional employment and labor force participation rates were trending parallel. This provides strong support for the validity of the research design. From 2011 onwards, the effect of the policy on both outcomes is negative and it has been increasing over time. Then, I estimate equation 1, which results are shown in columns 1 and 2 of Table XVII. The maternity leave extension policy decreased childbearing age women's unconditional employment rate by 2.4 percentage points and decreased female labor force participation rate by 3 percentage points. These results are significant at the 0.1% level.

Figure 11: Event study for unconditional employment.



Notes: Point estimates and 95% confidence intervals for each year are plotted. Estimates are relative to 2009, since it is the closest year prior to the implementation of the maternity leave expansion. Treatment group: childbearing age women. Control group: older women (39 to 50 years old).

Figure 12: Event study for labor force participation.



Notes: Point estimates and 95% confidence intervals for each year are plotted. Estimates are relative to 2009, since it is the closest year prior to the implementation of the maternity leave expansion. Treatment group: childbearing age women (18 to 38 years old). Control group: older women (39 to 50 years old).

Table XVII: Childbearing age women main estimates.

	1	2	3
	Labor Force	Employment	Log Labor Income
Treatment x Post	-0.0307***	-0.0241***	
	(0.00315)	(0.00316)	
Treatment x Post x Female			0.00409
			(0.00850)
Observations	371440	371440	342898

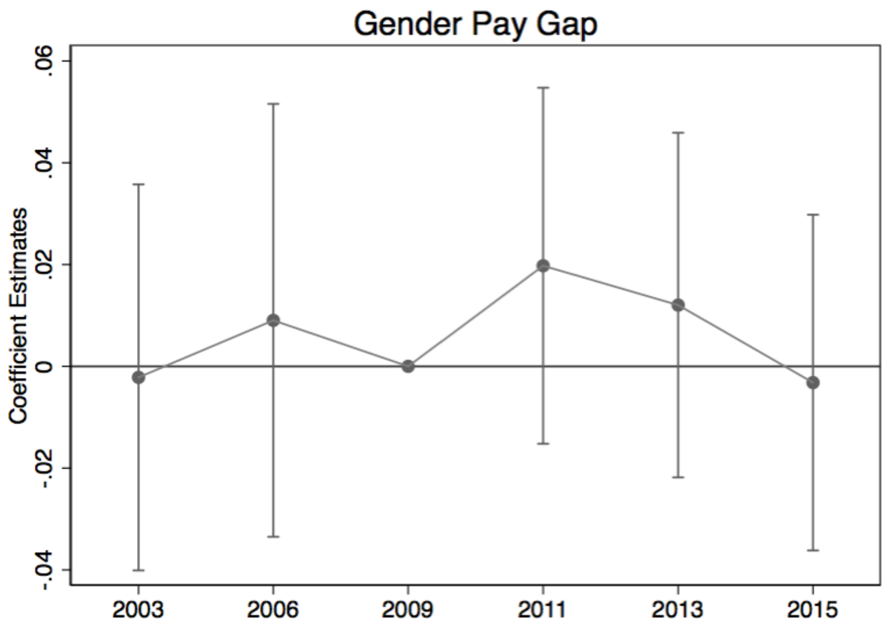
Robust standard errors in parentheses. All regressions include covariates, year fixed effects and age dummies. Column 3 include years of schooling dummies and occupation dummies.
* p<0.05, ** p<0.01, *** p<0.001

Given that women already had maternity leave benefits, it is likely that women who want to work and become mothers in the near future were already participating in the labor force; thus, this postnatal leave extension only induced a small number of women to enter the labor force, if any. Although one could expect a very small response of the labor demand to this new policy, as non-wage costs of having newly-mother employees absent from work for 12 extra weeks should be small[11], the result suggests that labor demand shifted more than labor supply did.

Second, I proceed to study the effects on the gender pay gap. As with the previous outcome, my first approach is to conduct an event study. Figure 13 shows the estimates of the maternity leave extension on the conditional gender pay gap. Except for 2003, the estimated treatment effects are all indistinguishable from zero. In this case the evidence for the validity of the research design is not as strong as in the labor force participation rate case, but it is still plausible that the gender pay gap for both groups was trending in a similar fashion before the policy change. Moving to the regression framework, I estimate equation 2. Results are shown in column 3 of Table XVII. This maternity leave extension had no impact on the gender pay gap for childbearing age women.

[11] If a company was going to replace a mother-to-be employee before for only 18 weeks, it is likely that with the new policy the company was still going to replace the employee and just keep the temporary worker for the 12 extra weeks. Now, if a company would have not replaced the employee before, probably with the new policy is more likely to employ a replacement as the total absence would be for over 7 months.

Figure 13: Event study for gender pay gap.



Notes: Point estimates and 95% confidence intervals for each year are plotted. Estimates are relative to 2009, since it is the closest year prior to the implementation of the maternity leave expansion. Treatment group: childbearing age women. Control group: older women (39 to 50 years old).

This result is somewhat surprising. A policy like this that intended to balance costs of hiring women and men by entitling men with parental leave rights should reduce the gender pay gap for childbearing age women. However, as Figure 3 shows, men are not using this benefit; hence, one could expect that hiring childbearing age women would become more expensive relative to hiring older women compared to men, increasing the gender pay gap. This null effect on the gender pay gap is not consistent with statistical discrimination against women. Employers discriminate in the same way younger women and older women compared to men, even though hiring younger women is relatively more expensive.

2.8. Specification and robustness tests

A differences-in-differences design does not require any additional control if the identifying assumptions hold, in this case that labor market outcomes for childbearing age women and older women would have trended in the same way absent this maternity leave policy. I start by running these simple regressions for each outcome, which are shown in Column 1 of Tables XVIII, XIX and XX, for employment, labor force participation, and gender pay gap, respectively. These regressions would give an unbiased estimate of the policy effect if there are not unobserved characteristics that would have made outcomes of both groups trend differentially. There are some reasons for which this assumption might not hold. As people age, they might have different preferences about working, which might be also influenced by the general conditions of the labor market at different points in time. To control for unobserved differences in labor market attachment across the life path of people, I include age dummies. To eliminate any possible bias arising from unobserved conditions affecting the labor market in a particular year, I include year fixed effects. Column 2 of each specification table reports the estimates that include both year fixed effects and age dummies. Another threat to identification is that factors that might determine employment and wages, such as education attainment might vary differentially across younger and older women, as general educational attainment has been rising over time in Chile. To address this last point, I include covariates such as schooling, marital status and whether the individual is the head of the household to the regression.

For the case of employment-to-population ratio, the estimated effect of the policy using the simple DID regression is a reduction of 3.96 percentage points in childbearing age women's employment rate. Column 2 of Table XVIII suggest that there are differential trends across younger and older women due to age or time, as the point estimate remains unchanged. However, covariates seem to play a role, when they are added instead of fixed effects, I find a smaller effect of the policy, only 2.46 percentage points reduction in employment rate. Column 4 shows the estimates of a regression that adds geographical dummies to the main specification. The point estimate is similar to Column 3, 2.69 percentage points reduction in female employment.

Table XVIII: Alternative specifications estimates: Unconditional employment.

	1	2	3	4
Treatment x Post	-0.0396***	-0.0396***	-0.0246***	-0.0269***
	(0.00339)	(0.00335)	(0.00322)	(0.00314)
Covariates	No	No	Yes	Yes
Year FE	No	Yes	No	Yes
Age Dummies	No	Yes	No	Yes
Region Dummies	No	No	No	Yes
Observations	371,457	371,457	371,440	371,440

Robust standard errors in parentheses.
* p<0.05, ** p<0.01, *** p<0.001

For the case of female labor force participation, the simple DID model (Column 1 of Table XIX) gives an estimated effect of the policy of 4.4 percentage points reduction in the participation rate of childbearing age women. When year fixed effects and age dummies are added (Column 2), I find a similar decrease of 4.65 percentage points in labor force participation. Column 3 shows the estimates of a regression that instead of fixed effects adds controls. Now I find a smaller effect of the policy, only 2.94 percentage points reduction in participation rate. Finally, Column 4 adds geographic dummies to the main specification, finding a larger reduction in childbearing age women labor force participation of 3.4 percentage points.

Table XIX: Alternative specifications estimates: Labor force participation.

	1	2	3	4
Treatment x Post	-0.0443***	-0.0465***	-0.0294***	-0.0340***
	(0.00340)	(0.00336)	(0.00320)	(0.00313)
Covariates	No	No	Yes	Yes
Year FE	No	Yes	No	Yes
Age Dummies	No	Yes	No	Yes
Region Dummies	No	No	No	Yes
Observations	371,457	371,457	371,440	371,440

Robust standard errors in parentheses.
* p<0.05, ** p<0.01, *** p<0.001

I repeat the above exercise for the gender pay gap estimates (equation 2). Results for these regressions are shown in Table XX. Column 1 includes no year fixed effect or age dummies, and controls flexibly for schooling (schooling dummies). Column 2 controls linearly for schooling instead and includes year fixed effects and age dummies. The point estimates in both columns are small, negative and not statistically different from zero. Column 3 also controls linearly for schooling but includes no year fixed effects or age dummies. Column 4 includes both year fixed effects plus age dummies, controls flexibly for schooling, and adds geographical fixed effects. Point estimates for these last two columns of Table XX are similar to the main results presented earlier, always small and statistically insignificant.

Table XX: Alternative specifications estimates: Gender pay gap.

	1	2	3	4
Treatment x Post	-0.00249	-0.000349	0.00574	0.00264
	(0.00859)	(0.00866)	(0.00857)	(0.00833)
Schooling Dummies	Yes	No	No	Yes
Year FE	No	Yes	No	Yes
Age Dummies	No	Yes	No	Yes
Region Dummies	No	No	No	Yes
Observations	342,898	342,898	342,898	342,898

Robust standard errors in parentheses. All columns include occupation dummies.
* p<0.05, ** p<0.01, *** p<0.001

Overall, results for all outcomes are robust to different specifications. Thus, the evidence for the maternity leave extension of 12 weeks in Chile studied here suggests that the policy reduced employment and labor force participation for childbearing age women by 2.4 and 3 percentage points, respectively, and had no impact on the gender pay gap for younger women.

2.9. Conclusions

This chapter studies the impact of a maternity leave policy that in 2011 increased the paid postnatal leave period by 12 weeks in Chile. I find robust evidence that this policy had an unintended effect by reducing labor force participation of women of childbearing age by 3 percentage points and their employment by 2.4 percentage points, while it had no effect on the gender pay gap. These results suggest that probably the shift to the right of the labor supply curve was smaller than the shift to the left of the labor demand, employers are more responsive to this policy change than childbearing age women are. These findings are somewhat opposite to previous work that found that paid parental leave mandates in European countries led to increase in women's employment and a reduction in their relative wages.

A policy that intended to improve mothers' position in the labor market, by allowing them to remain attached to the labor market and help balancing the costs of hiring women and men by entitling men with parental leave rights, had a negative impact on childbearing age women's employment and did not help reducing the gender wage inequality. This last point suggests that a public policy that could potentially reduce the gender pay gap is one that gives more mandatory benefits to fathers, as in the current policy is the mother's decision to transfer part of the leave period to the newborn's working father. Overall, these results are consistent with a scenario where women already in the labor force are incentivized to remain attached to it after giving birth, but other childbearing age women willing to enter the labor force are having difficulties finding a job, as hiring them is relatively more expensive for employers than hiring men or older women.

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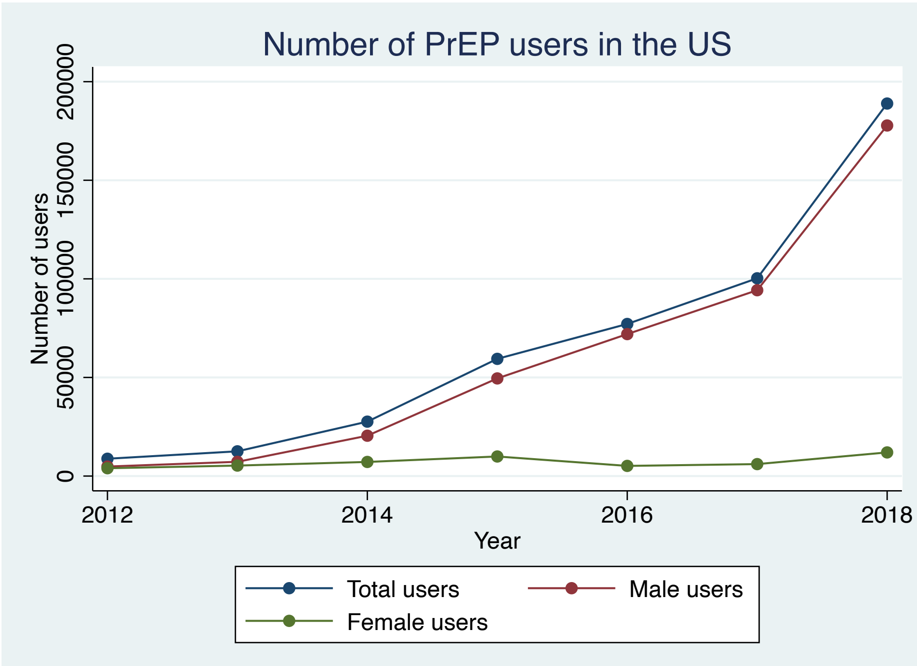
3. PrEPare for Trouble: Risk Compensation and the Unintended Consequences of PrEP

3.1. Introduction

The Centers for Disease Control and Prevention (CDC) estimate that 1.1 million people in the United States are at high risk of acquiring HIV. Most of this population is composed of men who have sex with men (MSM). The most effective HIV prevention method is the use of condoms, which was the only preventive tool until 2012. In July 2012, the U.S. Food and Drugs Administration (FDA) approved the use of Truvada for pre-exposure prophylaxis (PrEP) among HIV negative people as a way to reduce the risk of sexually acquired HIV in adults at high risk. Later, in May 2018, the FDA approved the use of Truvada for PrEP in at-risk adolescents and in October 2019 a second drug was approved for HIV prevention, Descovy for PrEP. PrEP consist of taking one pill a day, and it should be used in combination with safer sex practices. If PrEP is taken as recommended, it can reduce the risk of acquiring HIV by over 92%. Figure 14 shows that the number of PrEP users has rapidly grown in the last years. By the end of March 2020 there were approximately 240,000 people on PrEP in the United States, according to Gilead (Truvada manufacturer)[12]. Men represent over 94% of total PrEP users.

[12] See Gilead's Q1-2020 Earnings release.

Figure 14: Number of PrEP users in the United States by gender.



The availability of PrEP raises concerns about risk compensation around sex. As the risk of acquiring HIV through sexual intercourse is lower when using PrEP, people could be more likely to engage in risky sexual behavior. This risky sexual behavior might include increase in the number of sexual partners and/or increase in condomless sex. As a possible result of this change in behavior, individuals' type of romantic relationships might be affected, we could expect a change towards non-monogamous or open relationships. An increased number of sexual partners, especially if accompanied with an increase in unprotected sex, might make people more prone to contracting STDs other than HIV.

In this chapter, I study how the availability of PrEP is affecting sexual practices and romantic relationships of the group more likely to take the drug, MSM, and if this change in behavior has a causal effect on the increase in sexually transmitted diseases (STDs) seen in the United States in the last years. I develop a theoretical model of sexual activity among MSM that gives ambiguous predictions regarding how PrEP would affect sexual activity and STD rates. The lower risk of acquiring HIV due to PrEP should increase sexual activity and the probability of acquiring other STDs. For most PrEP users, PrEP became a substitute for condoms, and as such it should increase the level of comfort during the sexual act (Fischer and Boroditsky, 2000). This increased comfort should increase sexual activity and STD rates. As PrEP offers no protection against other STDs, it should increase the probability of acquiring a STD per sexual act, decreasing sexual activity and having an ambiguous effect on STD rates. Therefore, I conduct empirical analysis to assess the impact of PrEP on sexual behavior and STD rates.

I use data from the National Survey of Family Growth (NSFG) and the National Health and Nutrition Examination Survey (NHANES), and a propensity score matching difference-in-differences approach to compare the sexual behavior of MSM to that of heterosexual men before and after PrEP approval. I document an increase in the number of sexual partners among MSM after PrEP approval, specifically a 5-percentage-points increase in the probability of having more than two sexual partners in the last year and an increase of 8 percentage points in the probability of having a new sexual partner in the last year. However, I do not find evidence of a change in the extensive or intensive margin of sexual activity. PrEP is also affecting MSM's romantic relationships. I find a large increase of 10 percentage points in the probability of being in a non-monogamous relationship and a non-statistically significant decrease in the probability of being married or cohabitating with their partner. Another way PrEP is increasing risky sexual behavior is by increasing unprotected sexual activity. After PrEP became available, MSM are 14 percentage points more likely to never use condoms. MSM who used to always use condoms and those who used condoms less than 50% of the times are now not using condoms at all.

I assess the impact of PrEP on STD rates using two different approaches. First, I use individual level data from NHANES and the same propensity score matching difference-in-differences technique to document a large increase of 10 percentage points in STD testing among MSM after PrEP approval. I find suggestive evidence of an increase in chlamydia and HPV infections, although not statistically significant. The second approach uses state-level data and difference-in-difference model that compares STD rates of adult men to adolescent men before and after PrEP approval. I document an increase in chlamydia and gonorrhea incidence rates among adult men after PrEP approval. These results, however, could be reflecting a true increase in the underlying infections, or just a mechanical increase in diagnosis due to the increased testing.

My results provide support to the concern that PrEP would increase risky sexual behavior and reduce condom usage. While other studies have also documented similar results along with rising rates of gonorrhea and chlamydia after starting PrEP (Marcus et al., 2016; Liu et al., 2016; Alaei, Paynter, Juan, & Alaei, 2016), none of them can claim causality. I contribute to the literature by providing causal estimates of the effect of reduced risk of acquiring HIV on sexual behavior and on STD testing and infection rates.

These results are important for public policy matters. Health policymakers along with healthcare providers should reinforce the importance of keep practicing safer sex and promote regular STD testing. This would not only help people in the United States, but it could benefit people all over the world, as PrEP is currently available in almost 70 countries and many other countries are considering its adoption.

3.2. Institutional Background

In July 2012, the U.S. Food and Drugs Administration (FDA) approved the use of Truvada for pre-exposure prophylaxis (PrEP) in adults at high risk of acquiring HIV. Later, in May 2018, the

FDA approved the use of Truvada for PrEP in at-risk adolescents weighting at least 35 kilograms. In October 2019, the FDA approved the use of Descovy for PrEP in at-risk adults and adolescents, but excluded individuals at risk from receptive vaginal sex. Both Truvada and Descovy are also used to treat HIV positive individuals.

PrEP consists of taking one pill a day, and if it is taken as recommended it can reduce the probability of acquiring HIV through sex by over 92% (cite). PrEP also reduces the probability of acquiring HIV among injecting drug users by at least 74% if it is taken daily (cite). Both Truvada and Descovy for PrEP must be prescribed as part of a comprehensive prevention strategy. Healthcare providers must counsel patients on PrEP to strictly adhere to the recommended dosing schedule, inform them that PrEP only provides protection against HIV and no against other STDs, and point out the importance of condoms as a prevention strategy. PrEP can only be prescribed to individuals confirmed to be HIV negative, both immediately before the initial use and before each prescription refill (every three months)[13]. The reason behind the periodic HIV testing is that in PrEP clinical trials, individuals with undetected acute HIV infection developed drug-resistant HIV variants with the use of Truvada. If a PrEP user becomes HIV positive, he must stop taking PrEP and initiate a proper HIV treatment.

Individuals are considered to be at high risk of acquiring HIV if they have partner(s) known to be HIV-1 infected, have inconsistent condom use, or have been diagnosed with a sexually transmitted infection in the last six months. Injecting drug users are considered at high risk if they have an injection partner who is HIV positive or if they share needles, syringes or other equipment to inject drugs. The Centers for Disease Control and Prevention (CDC) estimate that approximately 1.1 million people in the United States are at high risk of acquiring HIV. Most of these people are men who have sex with men (MSM). Gay and bisexual men are the population most affected by HIV and

[13] See FDA's Highlights of Prescribing Information for Truvada.

other STDs such as syphilis and gonorrhea. In 2017, gay and bisexual men accounted for 66% of all HIV diagnoses and 82% of diagnoses among males. Over 60% of people living with HIV in 2016 were MSM. The most recent CDC report about STDs estimates that over 42% of gonorrhea cases and over 53% of syphilis cases were among MSM. The CDC recommend that people on PrEP should be periodically tested for STDs such as gonorrhea and syphilis, at least every six months.

Since the initial approval of Truvada for PrEP by the FDA, PrEP have been approved and implemented in several countries around the world. Currently it is available in almost 70 countries, mostly in the developed world (North America, Western Europe, Japan, and Australia), but it is also available in many Sub-Saharan African countries.

3.3. Conceptual Framework

In this section, I provide a theoretical framework to study how a new technology that reduces the probability of acquiring HIV affects individuals' sexual activity decisions. Then, I provide predictions regarding how specifically PrEP affects sexual activity and the likelihood of acquiring a STD other than HIV.

Following Nguyen's model (2019), I develop a model of sexual activity in the context of homosexual sexual encounters among men. Individuals get utility from sexual activity (s), which can be written as $v(s)$. In this context, the only risk associated with sexual activity is acquiring a sexually transmitted disease (STD). I will distinguish between HIV and all other STDs, as the cost associated to HIV is higher than the cost associated to other STDs. The monetary cost of HIV treatment is much higher than the treatment cost of other STDs. HIV remains incurable, while other STDs can be cured. HIV can be deadly if left untreated, and there is a greater social stigma associated to HIV. The expected cost of becoming HIV positive is the product of the probability of acquiring HIV (h) and the exogenous, fixed cost of acquiring HIV (H). The probability of acquiring HIV is a function of the quantity of sexual activity (*s*) and the probability of acquiring HIV per act (*p*). This can be written as follows:

$$h(s,p) = 1 - (1-p)^s$$

The expected cost of getting any other STD is the product of probability of acquiring an STD (*d*) and the exogenous, fixed cost of acquiring an STD other than HIV (*D*). The probability of acquiring any other STD is a function of the quantity of sexual activity and the probability of acquiring an STD other than HIV per act (*r*). This probability can be written as follows:

$$d(s,r) = 1 - (1-r)^s$$

A rational, utility maximizing individual will choose the amount of sexual activity that maximizes the utility he gets from *s* amount of sex, considering the cost of acquiring HIV (*hH)* and the cost of acquiring any other STD (*dD)*. The quantity of sex *s* is a function of the level of comfort offered by the protection method used during sex (*q*). For simplicity, I assume that $s(q)$ is continuous and that the utility from a sex act increases with higher levels of *q* ($v_{sq} > 0$).

The optimal amount of sex is going to be given by the point where marginal utility of a sex act equals its marginal cost.

$$v_s = h_s H + d_s D$$

I am interested in two things: first, how sexual activity changes when there a new technology changes the probability of acquiring HIV and at the same time changes the level of comfort during sex; and second, how this technology affects the probability of acquiring other STDs. To find the impact of a technology like PrEP on sexual activity, I take the derivative of *s* with respect to *p*, *q*, and *r*. By solving the utility maximization problem, it can be shown that:

$\frac{\partial s}{\partial p} < 0$: The optimal amount of sex decreases if the probability of acquiring HIV increases.

$\frac{\partial s}{\partial q} > 0$: The optimal amount of sex increases if the comfort offered by the protection method used during the sexual act increases.

$\frac{\partial s}{\partial r} < 0$: The optimal amount of sex decreases if the probability of acquiring other STDs increases.

Changes in the probability of acquiring other STDs due to changes in the probability of becoming HIV positive, changes in the comfort level offered by the protection method, and changes in the probability of acquiring other STDs per sex act can be obtained by taking the derivative of $d(s, r)$ with respect to *p*, *q*, and *r*, respectively.

$$d_p = -\ln(1-r)(1-r)^s \frac{\partial s}{\partial p} < 0$$

$$d_q = -\ln(1-r)(1-r)^s \frac{\partial s}{\partial q} > 0$$

$$d_r = s(1-r)^{s-1} + \left[-\ln(1-r)(1-r)^s \frac{\partial s}{\partial r}\right] \gtreqless 0$$

The model provides two clear predictions: the probability of acquiring other STDs decreases as the probability of getting HIV increases, and as the comfort offered by a protection method raises, the probability of acquiring other STDs increases. The model, however, provides an ambiguous answer in terms of how changes in the probability of acquiring an STD per act r affects the total likelihood of getting a STD. Changes in r affect this likelihood through two opposing channels: first, an increase in r directly rises d, and second, as r rises, it reduces sexual activity, decreasing the likelihood of acquiring a STD d. Hence, the total effect depends on which effect is larger.

To see how exactly the availability of PrEP affect sexual behavior and STD rates, it is necessary first to understand how PrEP is affecting the main parameters of the model previously described. PrEP decreases the probability of acquiring HIV per sexual act (p), it offers greater comfort during a sexual act (higher q) compared to condoms; however, it offers no protection against other STDs, hence it increases the probability of contracting other STDs per sexual act (r) if it is used as a substitute for condoms. Using the above model predictions, it is unclear how PrEP affects sexual behavior. Sexual activity increases as PrEP reduces the probability of acquiring HIV and offers greater level of comfort during sex compared to condoms, but it reduces sexual activity since it increases the likelihood of acquiring other STDs. Similarly, the effect of PrEP on STD rates is ambiguous. The lower probability of acquiring HIV along with the greater comfort during sex should increase STD rates; however, the effect of a higher probability of getting a STD per sex act is ambiguous, making the total effect ambiguous.

3.4. Literature Review

Risk compensation has been studied in several contexts. For example, wearing seat belts makes drivers feel safer and they drive faster and less carefully than those not wearing seat belts (Peltzman, 1975; Richens, Imrie, and Copas, 2000). It has also been suggested that sunscreen use encourages recreational sun exposure (Autier et al., 1998). There are a few papers that have study changes in sexual behavior after the introduction of contraceptive pills and emergency contraceptive pills (Mulligan, 2016; Durrance, 2013; Girma and Paton, 2011; Zuppann, 2001; Nguyen, 2019). They have generally found that access to better contraceptive technologies increases intercourse frequency and the number of sexual partners, reduces condom use, and it leads to an increase in sexually transmitted infections.

Most closely related to this study is the case of how the increased availability of highly active antiretroviral therapy (HAART) affected sexual behavior among some risk groups. For example, Stolte et al (2004) found that HIV-negative homosexual men were more likely to switch to risky sex when they perceived a lower level of threat of HIV/AIDS due to the availability of HAART. HAART also influenced sexual and injection practices among injecting drug users (IDU). Tun et al (2003) document that among HIV-positive IDU, perceiving that HIV treatments decrease HIV transmission is significantly associated with unprotected sex practices.

A couple of papers have studied different behaviors among PrEP users. Marcus et al (2016) studied a cohort of Kaiser Permanente Northern California members initiating PrEP in 2012 and focus on adherence, discontinuation and STD incidence, among others. They found a mean adherence of 92% and that women were more likely to discontinue PrEP. In terms of STD incidence, they observed high quarterly positivity (35% were diagnosed with at least one STD), which increased over time (after 12 months of PrEP use, the cumulative incidence was 41%). Jansen et al (2020) studied STD rates among German MSM. They document that PrEP users have an overall STD rate 15 percentage points higher than non-PrEP HIV-negative MSM, and almost 10 percentage points higher than HIV-positive MSM. Over 27% of HIV-negative MSM in their sample were PrEP users and among PrEP users, almost 92% did not use condoms.

3.5. Data

In this section I describe the sources of data used in this chapter, explain how the main variables are constructed and provide descriptive statistics. I get data from several sources: data on the number of PrEP users in the United States are obtained from AIDSVu, data on STD rates are gathered from the CDC, and data on sexual behavior come from two different surveys from the U.S. Department of Health and Human Services, the National Survey of Family Growth (NSFG) and the National Health and Nutrition Examination Survey (NHANES).

AIDSVu[14] reports data on PrEP users aggregated to the county and state levels by year. A person is considered a user if they had at least one day of prescribed Truvada for PrEP in a calendar year. These data represent an estimated number of weighted PrEP users in each state and county by year, and it underestimates the actual number of PrEP users. AIDSVu obtained these data from Symphony Health, who collects national, electronic, patient-level prescription data from a large sample of pharmacies, hospitals, outpatient facilities, and physician practices across the U.S. A validated algorithm was employed by Gilead to exclude prescriptions for Truvada that were made for other known indications, such as HIV treatment, post-exposure prophylaxis, and chronic hepatitis B management.

The CDC reports STD incidence rates aggregated at different geographical levels by gender and age group. I collect men's STD rates from 2006 to 2017 at the state level for different ages groups (15 to 19, 20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44, 45 to 54, 55 to 64, and 65+ years old). I focus on three infections: chlamydia, gonorrhea and syphilis.

The NSFG survey collects information on marriage and divorce, use of contraception, and people's health. I use four waves that span from 2011 to 2017. The NHANES survey assesses the health and nutritional status of adults and children in the United States. It combines interviews and physical examinations. The interview includes demographic, socioeconomic, and health-related

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[14] AIDSVu is a joint initiative by Emory University's Rollins School of Public Health, Gilead Sciences, Inc. and the Center for AIDS Research at Emory University (CFAR).

questions, and the examination component incorporates several laboratory tests, including STD testing. I use five waves of this survey from 2007 to 2016.

Both NSFG and NHANES allow me to observed individuals sexual behavior, including the sex of their sexual partners. Using this information, I classify an individual as man who has sex with men (MSM) if the individual has ever had a sexual encounter with a man, whether that encounter included anal or oral sex, or both. If an individual has never had any type of sexual encounter with another man, then he is classified as heterosexual man. Table XXI shows pre-PrEP approval descriptive demographic characteristics for MSM and heterosexual men. MSM are slightly older than heterosexual men, they are also more likely to be white and hence less likely to be part of a racial or ethnic minority. This could be representing either real differences in the racial/ethnic composition of these two groups, or differences in sexual behavior reporting by racial/ethnic group. MSM are more educated than heterosexual men: they are less likely to be a high school dropout and more likely to have a college degree. Unsurprisingly, MSM are less likely to be married than heterosexual men. At the time that these statistics were measured, only a few states in the United States allowed same sex marriage, so it is expected a lower marriage rate among MSM.

Table XXI: Pre-PrEP approval demographic characteristics.

	Heterosexual	MSM
Age	36.4	37.2
White	0.618	0.733
Black	0.120	0.093
Hispanic	0.183	0.145
Other race	0.079	0.028
High School Dropout	0.171	0.091
High School Diploma	0.247	0.267
Some College	0.318	0.323
College Degree	0.262	0.339
Married	0.561	0.232
Married or living with partner	0.651	0.471
Observations	3889	234

In terms of baseline sexual behavior, which is reported in Table XXII, there also large differences between these two groups. MSM are almost 12 percentage points more likely to have had sex in the last 12 months. They are also 12 percentage points more likely to have more than two sex partners and 17 percentage points more likely to have had a new sex partner in the last year. Along the same lines, MSM are 23 percentage points more likely to engage in non-monogamous relationships than heterosexual men. This riskier sexual behavior seems to be compensated in part by a more frequent use of condoms and higher likelihood of being tested for a STD. Among MSM, 32% report always using condoms during sex, while only 25% of heterosexual men always use condoms. The proportion of men who never uses condoms is almost 5 percentage points larger among heterosexual men. While 24% of MSM have been tested for a STD in the last year, only 15% of heterosexual men were tested during the same period.

Table XXII: Pre-PrEP approval individuals' sexual behavior in the last 12 months.

	Heterosexual	MSM
Had sex	0.733	0.850
More than 2 sex partners	0.095	0.216
Had sex with a new partner	0.150	0.319
Non-monogamous relationship	0.083	0.310
Got tested for any STD	0.147	0.240
Always used condoms	0.253	0.320
Never used condoms	0.471	0.424
Observations	4,206	239

3.6. Empirical Strategy

There are a few challenges to identifying the causal effect of taking PrEP on sexual behavior and STD rates. The primary challenge is that men who choose to take PrEP are likely different than men who choose not to take it on unobservable dimensions. Another equally important challenge is that my data do not allow me to identify who is taking PrEP. A third challenge is that PrEP should increase STD testing due to CDC recommendation of periodic testing, which would mechanically increase STD rates even if the underlying level of STDs remains unchanged. In order to address some of these concerns, I use a difference-in-differences approach where the treatment group is composed of the group of people more likely to take PrEP, men who have sex with men (MSM), and the control group is composed of people less likely to be on PrEP, heterosexual men. Although MSM are likely to be fairly different than heterosexual men, as long as their sexual behavior outcomes trend in a similar fashion, this difference-in-differences analysis will produce unbiased estimates. This approach compares sexual behavior outcomes of MSM to heterosexual men before and after PrEP approval in 2012. Equation 1 shows this difference-in-differences model.

$$y_{it} = \alpha + \beta_1 MSM_{it} * Post_t + \beta_2 MSM_{it} + \gamma_t + \varepsilon_{it} \qquad (1)$$

where y_{it} is the outcome of interest for individual *i* observed in year *t*, MSM_{it} is a dummy equal to 1 if individual *i* observed in year *t* has ever had sex with men, $Post_t$ is a dummy equal to 1 for observations after PrEP approval in 2012, and γ_t corresponds to year fixed effects.

Though level differences between MSM and heterosexual men do not necessarily cause bias, large differences make it less likely that these two groups' outcomes would be trending similarly. I can directly assess trend similarity in the pre-period, but the difference-in-differences setup still relies on the fundamentally untestable assumption that trends would have be similar in the post period. To increase the likelihood that this assumption will hold, I use propensity-score matching to make MSM and heterosexual men look more similar. I match on demographic characteristics such as schooling, age, and race/ethnicity.

It is important to note that the coefficient β_1 is an intent to treat parameter since not all MSM choose to take PrEP. In the context of risk compensation, I would expect that not only people taking PrEP would perceive a lower risk of getting HIV, but also people who are engaging in sexual activity with these people on PrEP. For example, if a potential sex partner is on PrEP, it must be the case that he is HIV negative and that would make more likely to accept engaging in risky sex. Even though the number of PrEP users in the U.S at the end of March 2020 is approximately 240,000, the number of people whose behavior could be affected by this perceived lower risk of HIV could be easily three times that number, considering that MSM have a larger number of sexual partner and are less likely to engage in monogamous relationships.

3.7. Results

3.7.1. Sexual Behavior

I first focus on outcomes related to sexual activity and sexual partners. Figure 15 depicts the proportion of men who had sex in the last 12 months. There are two main takeaways from this picture. First, MSM are more likely to have had sex in the last 12 months than heterosexual men. Second, these proportions were trending in a similar way before PrEP approval, which makes plausible that the common trend assumption will hold. Figure 16 displays the intensive margin of sexual activity. Specifically, it shows the proportion of both MSM and heterosexual men who had sex more than once a week in the last 12 months. Before PrEP approval, heterosexual men were more likely to have sex more than once a week than MSM. Columns 1 and 2 of Table XXIII show the point estimates using the propensity score matching difference-in-differences technique for both outcomes. The point estimates suggest no change in both the extensive margin and the intensive margin of sexual activity among MSM after PrEP approval.

Figure 15: Proportion of men who had sex in the last 12 months.

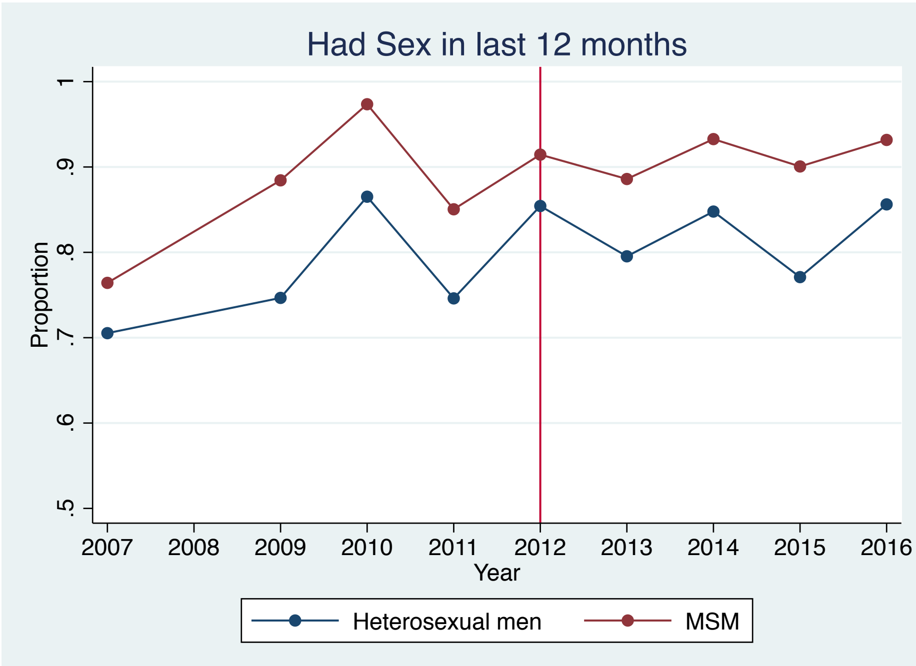


Figure 16: Proportion of men who had sex more than once a week in the last 12 months.

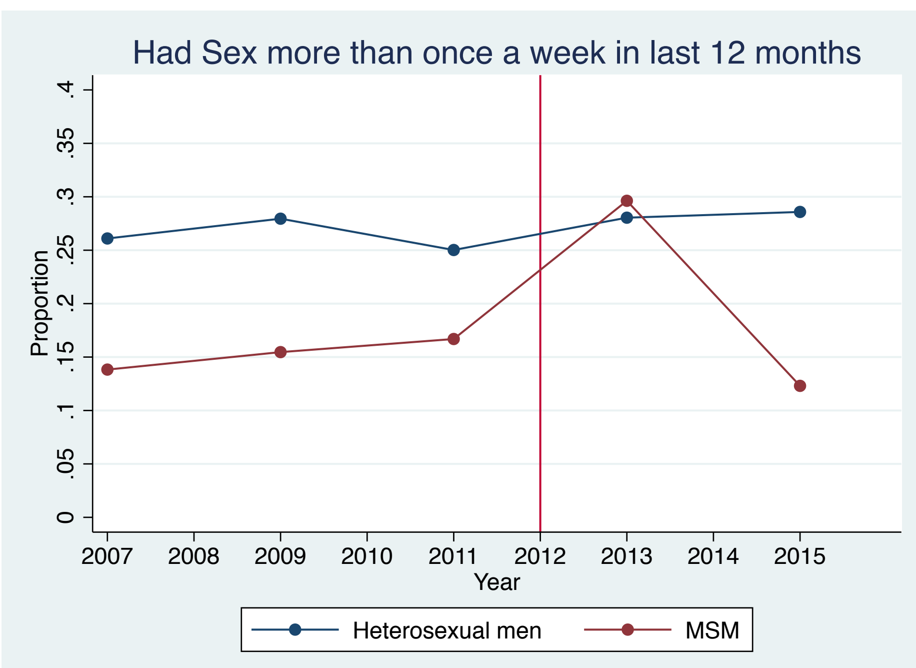


Table XXIII: Sexual behavior results: intercourse frequency, number of partners and relationships.

	1	2	3	4	5	6
	Had sex	More than 52 times	More than 2 sex partners	New sex partner	Non-monogamous relationship	Married or cohabitating
MSM	0.115***	-0.0886***	0.181***	0.163***	0.224***	-0.183***
	(0.0170)	(0.0248)	(0.0221)	(0.0262)	(0.0372)	(0.0322)
MSM x Post	-0.0125	-0.0059	0.0559**	0.0836*	0.105**	-0.0567
	(0.0207)	(0.0381)	(0.0278)	(0.0450)	(0.0426)	(0.0495)
Observations	23867	10006	23867	10006	13861	10006

Robust standard errors in parentheses. All regressions include Year FE.

* p<0.1, ** p<0.05, *** p<0.01

Figure 17 shows the proportion of men who had more than two sexual partners in the last 12 months. MSM are more likely to have more than two sexual partners than heterosexual men both before and after PrEP approval. The proportion of men who had a new sexual partner in the last 12 months is displayed in Figure 18. MSM are also significantly more likely to have had a new sexual partner in the last year than heterosexual men. Column 3 of Table XXIII shows the estimate for the probability of having more than two sexual partners. After PrEP approval, MSM are over 5 percentage points more likely to have more than two sexual partners than heterosexual men. The regression estimate displayed in Column 4 of Table XXIII shows that MSM are 8 percentage points more likely to have a new sexual partner after PrEP approval compared to heterosexual men. Overall, these results show an increase in the number of sexual partners among MSM after PrEP approval.

Figure 17: Proportion of men who had more than 2 sexual partners in the last 12 months.

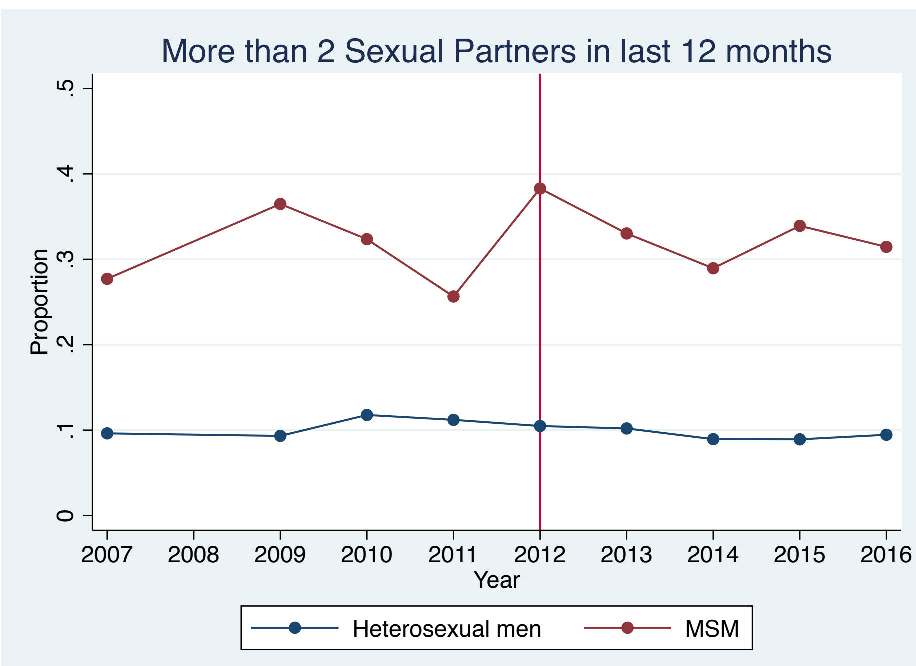
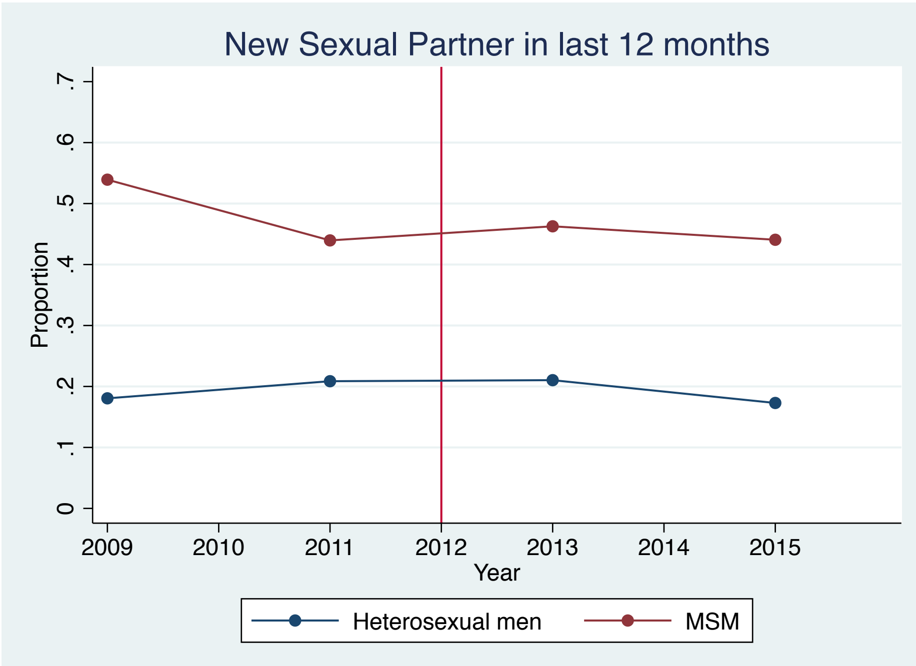


Figure 18: Proportion of men who had a new sexual partner in the last 12 months.



Since the number and composition of sexual partners is changing after PrEP approval, there could be a change in romantic relationships. Figure 19 portrays the proportion of men who engage in non-monogamous relationships. MSM are significantly more likely to have a non-monogamous partner than heterosexual men. There is a large increase in 2012 in the proportion of MSM who has a non-monogamous partner after PrEP approval, while this proportion remain stable among heterosexual men. The proportion of men who are married or cohabitating with their partner is shown in Figure 20. Heterosexual men are significantly more likely to be married or cohabitating than MSM, both before and after PrEP approval. The point estimate in Column 5 of Table XIII shows a large increase of 10 percentage points in the probability of having a non-monogamous partner among MSM after PrEP approval. The point estimate displayed in Column 6 of Table XXIII suggests a decrease in the probability of being married or cohabitating among MSM; however, this is not statistically significant.

Figure 19: Proportion of men who had a non-monogamous partner in the last 12 months.

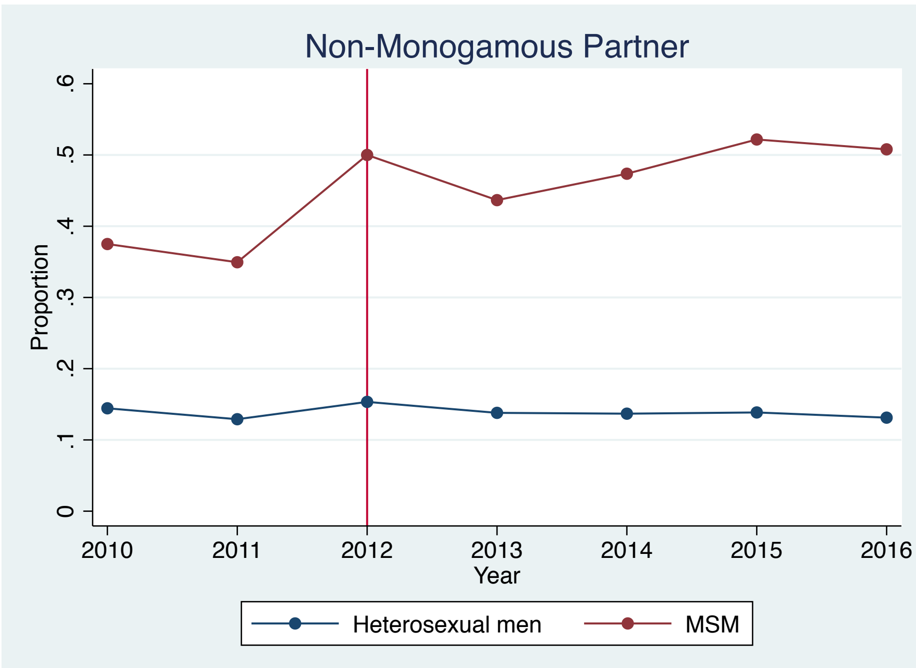
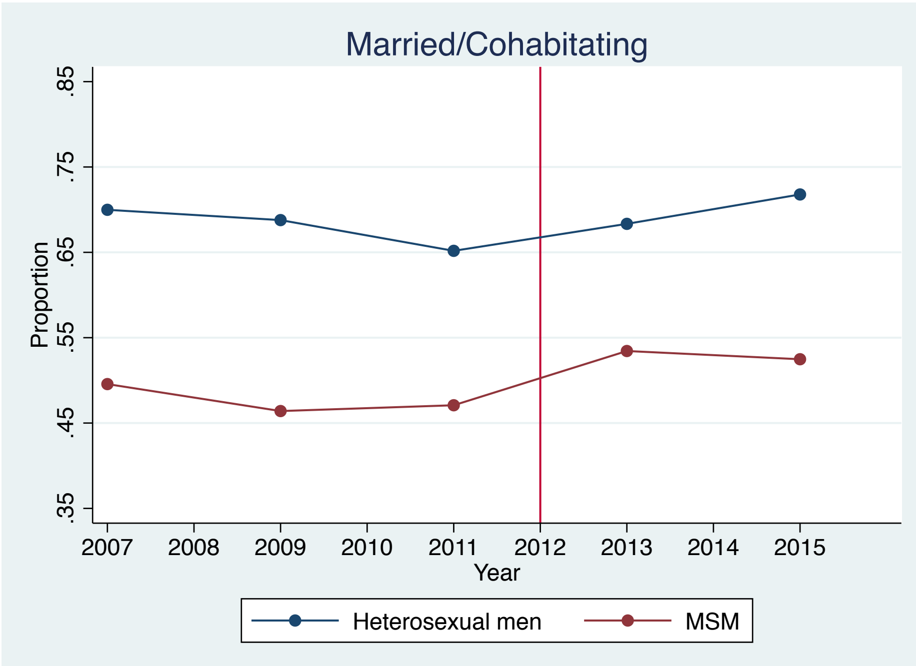


Figure 20: Proportion of men who were married or cohabitating with their partner.



The results previously discussed suggest that MSM are taking more risk after PrEP approval by having larger number of sexual partners. Now, I analyze if PrEP is also increasing risky sexual behavior by changing condom use patterns. Figure 21 shows the proportion of men who report never using condoms in the last 12 months. Before PrEP approval in 2012, heterosexual men were significantly more likely to report never using condoms than MSM. After PrEP approval, there is a large increase in the proportion of MSM never using condoms. In fact, by 2015 this proportion is almost 4 percentage points larger among MSM than heterosexual men. Regression coefficients displayed in Table XXIV confirm this change in condom use. Column 5 shows that there is a large increase of 14 percentage points in the probability of never using condoms among MSM after PrEP approval in 2012. This change seems to be coming from people switching from always using condoms to never using condoms and from people using condoms less than 50% of the times to never using condoms.

Figure 21: Proportion of men who never used condoms in the last 12 months.

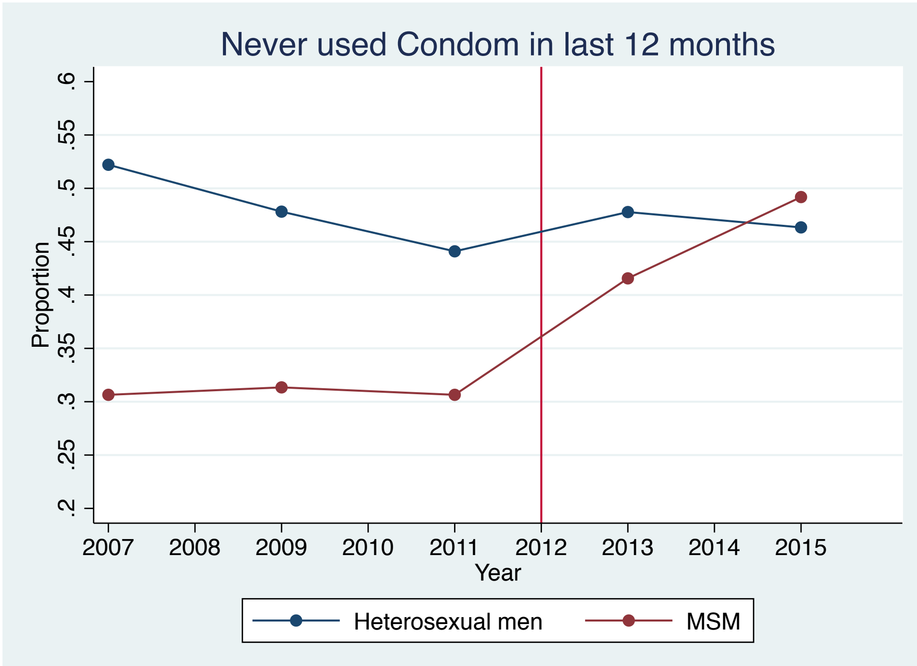


Table XXIV: Sexual behavior results: condom use.

	1	2	3	4	5
	Always	More than 50%	Half of the times	Less than 50%	Never
MSM	0.0767***	0.0308	0.00881	0.0560***	-0.172***
	(0.0277)	(0.0208)	(0.0136)	(0.0197)	(0.0323)
MSM x Post	-0.0693*	0.00294	-0.0117	-0.0628**	0.141***
	(0.0403)	(0.0326)	(0.0206)	(0.0259)	(0.0492)
Observations	10006	10006	10006	10006	10006

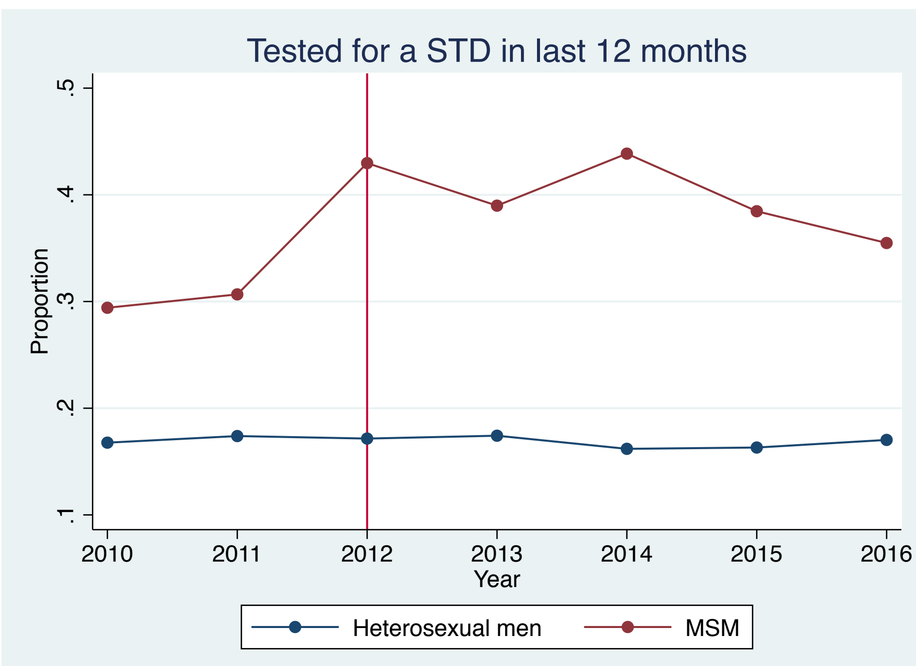
Robust standard errors in parentheses. All regressions include Year FE.

* p<0.1, ** p<0.05, *** p<0.01

3.7.2. STDs: Testing and Incidence

The previous section documented an increase in risky sexual activity among MSM after PrEP approval. This increase in risky Actual behavior could translate into an increase in STD incidence. I start this analysis by looking at the probability of being tested for any STD. Figure 22 shows the proportion of men who were tested for any STD in the last 12 months. Before 2012, MSM where approximately 13 percentage points more likely to be tested for STD than heterosexual men. After PrEP approval, there is a large increase of approximately 10 percentage points in the proportion of MSM who were tested for STDs, while this proportion remains flat for heterosexual men. The point estimate in Column 1 of Table XXV confirms this increase of 10 percentage points.

Figure 22: Proportion of men who were tested for any STD in the last 12 months.



I assess the impact of PrEP on STD rates by using two different approaches. First, using individual level data from NHANES and the same propensity score matching difference-in-differences approach, I compare STD rates of MSM and heterosexual men, before and after PrEP approval. NHANES tested for a few STDs, but I can only consistently analyze chlamydia and HPV. Columns 2 and 3 of Table XXV display the point estimates for these two outcomes. These results suggest an increase in the probability of testing positive for chlamydia and HPV, but they are not statistically significant. These results are still relevant, since they are not affected by increased STD testing.

Table XXV: STD results: testing and infections.

	1	2	3	4	5	6
	Tested for any STD	Chlamydia	HPV	Log Gonorrhea	Log Syphilis	Log Chlamydia
MSM	0.137***	0.0157	0.0476**			
	(0.0353)	(0.0157)	(0.0221)			
MSM x Post	0.1000**	0.0156	0.0238			
	(0.0406)	(0.0275)	(0.0378)			
Adult x Post				0.336***	-0.0453	0.299***
				(0.0276)	(0.0672)	(0.0316)
Observations	13861	5409	7696	5374	4758	5425

Robust standard errors in parentheses. Regressions include Year FE. Chlamydia only tested for people aged 18-39.

* p<0.1, ** p<0.05, *** p<0.01

My second approach uses a difference-in-differences model at the state level. I use male STD rates at the state level obtained from the CDC. This approach compares STD rates of adult men (20+ years old) to STD rates of adolescent males (15 to 19 years old), before and after PrEP approval in 2012. Adult men are the treated group, since they were allowed to take PrEP since 2012, and adolescent are the control group since they were only allowed to take PrEP after 2018. Figures 23, 24 and 25 show rates among men for chlamydia, syphilis and gonorrhea, respectively. Adolescents have higher incidence rates of chlamydia and gonorrhea, before and after 2012. However, the gap between adolescents and adult men is closing, especially for gonorrhea. The last three columns of Table XXV present the point estimates for these three outcomes. There is a large increase in gonorrhea and chlamydia among adult men after PrEP approval. However, there is no statistically significant differential change in syphilis rates between adolescents and adult men after 2012. This large increase in gonorrhea and chlamydia incidence rates could be reflecting a true increase in the underlying infections, or just be a mechanical increase in diagnosis due to the increased testing. A limitation of this approach is the possibility of contamination of the control group for a couple of reasons. First, part of the control group was eligible to be prescribed PrEP (18- and 19-year-old men). Second, some adults, especially the younger ones, could be having sex with adolescents. Unfortunately, CDC data do not allow me to address any of these issues. Therefore, these results should be interpreted with caution as they are only suggestive of a true causal relationship.

Figure 23: Chlamydia incidence rates among men by age group.

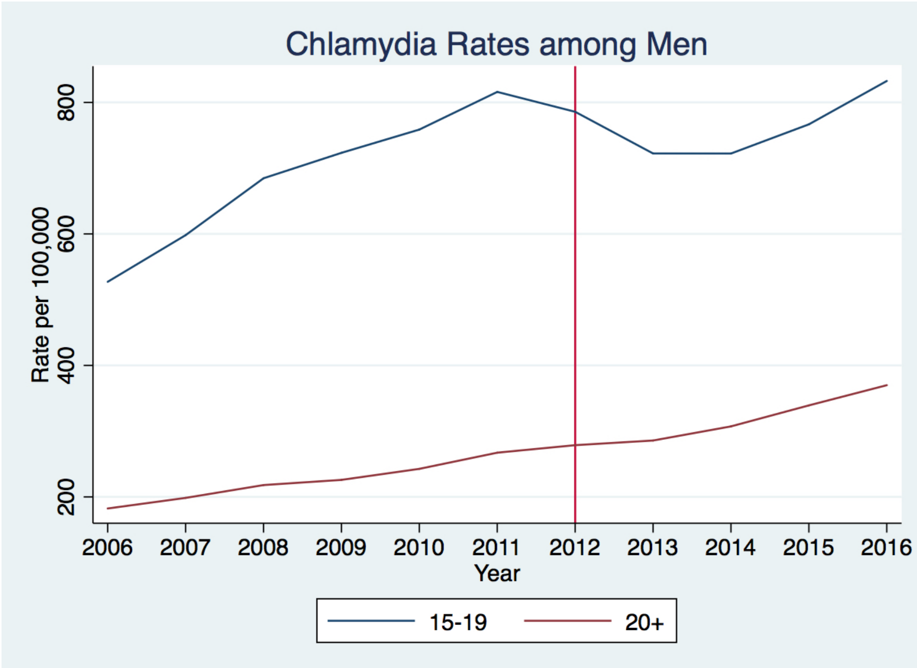


Figure 24: Syphilis incidence rates among men by age group.

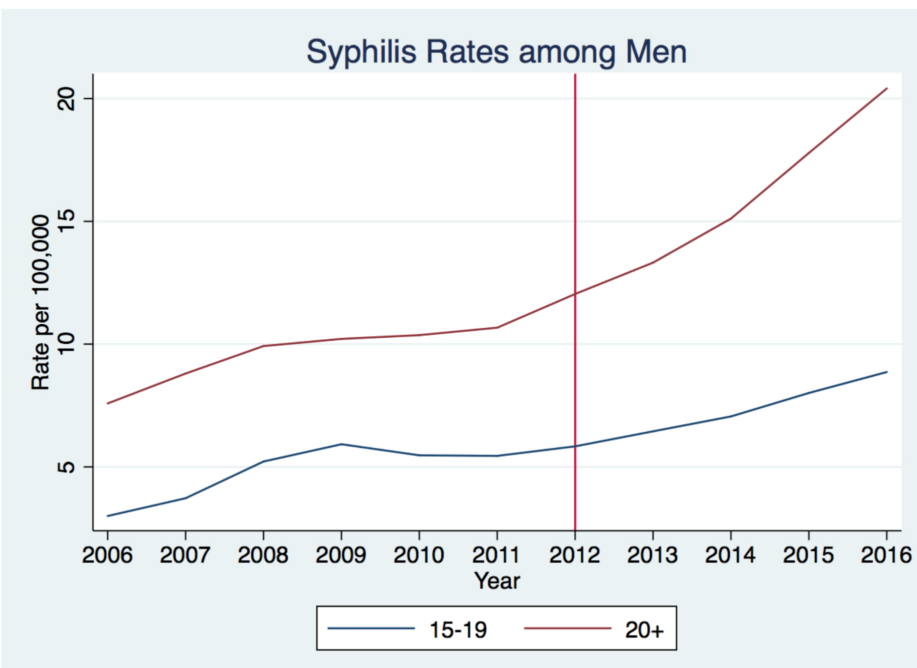
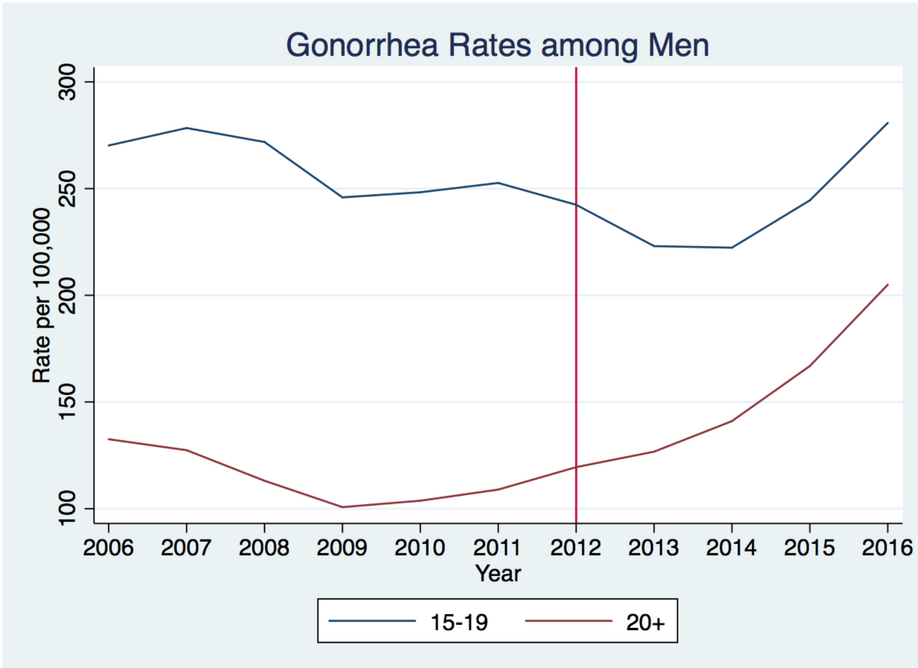


Figure 25: Gonorrhea incidence rates among men by age group.



3.8. Conclusions

I study how decreasing the risk of acquiring HIV through sex affects sexual behavior. I use the approval of Truvada for Pre-Exposure Prophylaxis (PrEP) as an exogenous source of variation. I find evidence of risk compensation around sexual activity after PrEP approval. I document that the group more likely to take PrEP, men who have sex with men (MSM), changes to risky sexual behavior after PrEP becomes available. There is an increase in the number of sexual partners as measured by the probability of having more than two sexual partners in the last year (+5 percentage points) and the probability of having a new sexual partner in the last year (+8 percentage points). However, there is no change in the extensive and intensive margin of sexual activity. PrEP is also affecting romantic relationships among the MSM population. I find a large increase of 10 percentage points in the probability of being a non-monogamous relationship and a non-statistically significant decrease in the probability of being married or cohabitating with their partner. Condom use is also affected by the availability of PrEP. After PrEP became available, MSM are 14 percentage points more likely to never use condoms. MSM who used to always use condoms and use condoms less than 50% of the times are now not using condoms at all.

This chapter also studies the effect of PrEP on STD rates. I first document a large increase of 10 percentage points in STD testing among MSM after PrEP approval. I find suggestive evidence of an increase in chlamydia and HPV infections, although not statistically significant. Using state-level data, I document an increase in chlamydia and gonorrhea incidence rates among adult men after PrEP approval. These results, however, could be reflecting a true increase in the underlying infections, or just a mechanical increase in diagnosis due to the increased testing.

Overall, my results confirm the presence of risk compensation among MSM. As they perceived a lower risk of acquiring HIV, they are more likely to engage in risky sexual behavior. This increased risky sexual behavior is causing an increase in STD infections. It is important that healthcare providers emphasize the vital role of condoms in stopping STDs and remind PrEP users that PrEP does not offer any protection against other STDs. This increased STD infection rates among MSM could also be playing a role in the increased STD rates among women since some of these MSM also have sex with women.

CITED LITERATURE

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