Title

Essays in Behavioral and Labor Economics

Author

Massenkoff, Maxim

Publication Date

2020

Peer reviewed|Thesis/dissertation

Essays in Behavioral and Labor Economics

by

Maxim Massenkoff

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Stefano DellaVigna, Chair
Professor Jesse Rothstein
Professor Christopher Walters

Spring 2020

Essays in Behavioral and Labor Economics

Abstract

Essays in Behavioral and Labor Economics

by

Maxim Massenkoff

Doctor of Philosophy in Economics

University of California, Berkeley

Professor Stefano DellaVigna, Chair

This dissertation uses tools from behavioral and labor economics to study longstanding questions about people and policies.

The first chapter, co-authored with Evan K. Rose, provides a comprehensive look at what happens to criminal offending around childbirth, marriage, and divorce for women and men. Our event study analysis suggests that pregnancy is a strong inducement for fathers and especially mothers to reduce criminal behavior. For mothers, criminal offending drops precipitously in the first few months of pregnancy, stabilizing at half of pre-pregnancy levels three years after the birth. Men show a 25 percent decline beginning at the onset pregnancy; however, domestic violence arrests spike for fathers immediately after the birth. A design using stillbirths as counterfactuals suggests a causal role for children. In contrast, marriage marks the completion of a 50 percent decline in offending for both men and women. Finally, people headed for divorce show relative increases in crime following childbirth and marriage. The patterns in drug offenses for new mothers are consistent with a Beckerian model of habit formation.

In the second chapter, I use a new dataset on unemployment insurance recipients to study the effects of benefits on search behavior. In a regression kink design, I find that monetary weekly benefits have a positive effect on unemployment duration but no impact on search behaviors. As evidence against misreporting, I show that reservation wages predict reemployment wages and decrease with unemployment duration and the local unemployment rate. These results suggest that explicitly measured search behaviors may not explain the duration response to benefits.

A common thread between these projects is to highlight the usefulness and limitations of dominant economic models in the social sciences. The first chapter argues that a version Becker's classic model of addiction does a surprisingly good job at predicting aggregate patterns of drug use. A model of habit formation predicts a distinctly gradual return to drug use for agents affected by a strong negative utility shock to use, a pattern observed among drug arrests but not economic crimes such as theft. The second chapter shows how workhorse job search models in labor economics do not closely correspond to the measured quantities from detailed survey data, suggesting that other approaches may be useful. For instance, a dominant model of work search predicts that unemployed people should increase their reservation wage when unemployment benefits are higher, but my large-scale study finds no evidence of this response.

The chapters also explore popular techniques for identifying causal effects in ecological contexts, with distinct twists due to the data sources. In the first chapter, most analyses use a straightforward event study framework, and the causal argument rests heavily on the sharpness of the response to pregnancy and a ruling out of other factors that might result in the same pattern. The analysis benefits from balance, i.e., having observations for every parent in the sample for five years before and after birth, which make the raw data presentations especially useful.

The second chapter wrestles with a thorny problem that, since unemployment benefits scale with income, it is difficult to isolate the effect of benefits on outcomes. I thus turn to a regression kink framework, exploiting the fact that benefits flatten as a function of past earnings when the statutory maximum is reached. Similar kinks in the outcomes at that point provide evidence that benefits played a causal role in determining the outcome.

Each methodological approach comes with strengths and weaknesses. While event studies can be visually compelling, there is not agreement on formal tests for pre-trends. And while regression kink discontinuities typically use benefit formulae that are not manipulable by the people under study, there is still debate on the appropriate bandwidth and functional form for estimating effects.

To Nikolai Massenkoff (Pop)

Contents

List of Figures

List of Tables

Acknowledgments

To my advisors: Stefano, through my stint as your research assistant and our many rapid-fire meetings as the job market approached, I gained a precious glimpse of the Stefano production function, and it was likely the most important lesson of my PhD. You also withstood some major skit party roasting, and the most stressful times were infused with humor. Thanks for being there starting from visit day. Jesse, you taught me a great deal about unemployment insurance, job search math, and working with governments, and embody what I think are the two most important ideals for economists: to do work that is useful and replicable. Chris, your advice on my many econometrics questions always marked a great lurch forward for my projects. All faculty showed a surprising willingness to sit down with me and work through problems, including Danny, David, Dmitry, Gabriel, Ned, Pat, and Ted.

To my collaborators: Evan, grateful for your help on the many problem sets of life. Nate, looking forward to pursuing our shared interests with blithe disregard for disciplinary boundaries. Andrew, here's hoping our experiment survives the pandemic.

To my classmates at Berkeley: Joe, my first friend in the cohort, I'll always remember our post-visit day huddle. Francis, I loved working with you on our many entertainment projects. Carla, you brightened our office. Peter J., Katja, Alex, thanks for letting me barge into your office. Caroline and Leah, you made grad school fun. Peter M., so glad I found an economic philosopher in our midst. Ingrid, your laughter fueled me through the rough patches of my economics stand-up. Yotam, Juli, Nick, thanks for the immense peer effects. Johannes, thanks for being my tech guru.

Outside of Berkeley: Jason, you are so stubbornly supportive. Hamed and Jeremy, I'm thankful for our thread and the Python support. Zoe, I loved comparing life notes with you.

Mom and dad, I ran the numbers and it really does seem as though I got some of the best parents possible. I thank you for your incredible and unconditional generosity, the many ways you have enriched my life with your talents, and the grit that resulted from the fact that both of you, by some cosmic coincidence, began your lives in orphanages halfway around the world.

Liz, I'm so lucky to have you. I'm grateful for the hike we took at Briones when we started talking over the questions that would form my job market paper. I never thought I would meet someone with my same sense of humor, but then one day your family friend said you seemed a lot happier with me and you responded, "We don't know the counterfactual." Nothing provided me more joy than zingers like these. I will never assume your love.

Chapter 1

Family formation and crime

joint with Evan K. Rose

1.1 Introduction

Researchers have long sought to understand the drivers of crime. Economists traditionally study rational models where forward-looking agents consider factors such as the certainty and severity of punishment when choosing whether to offend (Becker, 1968). In this vein, several studies focus on the impacts of expected punishments using discrete changes in sentencing regimes (Chalfin & McCrary, 2017). These efforts suggest that dissuading offenders is difficult: prominent quasi-experiments suggest small deterrence effects, consistent with extreme rates of discounting or myopia (Helland & Tabarrok, 2007; Lee & McCrary, 2005).[1]

A parallel strand of research in economics studies addictive behavior through a rational lens, where drug users choose their consumption levels fully aware that present use affects the future utility of consumption (Becker & Murphy, 1988). Tests of the rational model typically center on the responsiveness of drug users to expected changes in prices (Becker, Grossman, & Murphy, 1994; Gruber & Köszegi, 2001). However, large, anticipated shocks to the utility of drug use are rare, and even direct monetary incentives may have limited effects on consumption (Schilbach, 2019).

Sociologists have emphasized different determinants of criminal behavior and drug use, positing that "turning points" such as marriage and childbirth have the potential to spur drastic life improvements, independent of past circumstances, by strengthening social bonds (Sampson & Laub, 1992). Low-income parents often report that,

[1] California convicts with "two strikes" showed decreased offending—but in response to a massive increase in punishment, implying an elasticity of -0.06 (Helland & Tabarrok, 2007). However, a notable exception is Drago, Galbiati, and Vertova (2009), who find an elasticity of -0.5 in a large natural experiment on released Italian prisoners.

without their children or spouse, they would be in prison or on drugs (Edin & Kefalas, 2011; Edin & Nelson, 2013; Sampson & Laub, 2009). Existing empirical studies of turning points, however, have typically relied on small samples and produced conflicting results.

In this paper we use administrative data on over a million births to take an unprecedentedly close look at criminal arrests around key turning points related to family formation. We implement a novel match between Washington state records covering the universe of criminal arrests, births, marriages, and divorces, by far the largest such study ever conducted in the United States. Our comprehensive data allow us to highlight sharp changes in both the timing and types of arrests, control flexibly for key confounds such as age, and explore important differences across subgroups. The high frequency data also allow us to explore whether the timing and speed of response are consistent with anticipatory responses as in rational models of addiction (Becker, Grossman, & Murphy, 1994).

We begin our investigation with mothers. An event study analysis shows that pregnancy triggers enormous positive changes: drug, alcohol, and economic arrests decline precipitously at the start of the pregnancy, bottoming out in the months just before birth. Shortly after birth, criminal arrests recover, ultimately stabilizing at 50 percent below pre-pregnancy levels. The sharpness of the response suggests that these declines likely reflect the impact of pregnancy rather than the onset of a romantic relationship or the decision to form a family. We find similar positive long-term impacts on teen mothers, an important result given that extant studies have found zero or negative effects of teen childbearing on conventional economic outcomes such as income and education (Fletcher & Wolfe, 2009; Hotz, McElroy, & Sanders, 2005; Hotz, Mullin, & Sanders, 1997; Kearney & Levine, 2012).

We find substantial, if quantitatively smaller, impacts on fathers. Male arrests decrease sharply at the start of the pregnancy and continue at lower levels following the birth, with reductions around 25 percent for economic and drug crimes. New to our context, the timing of the fathers' response suggests that pregnancy, not childbirth, is the primary inducement to decrease criminal behavior. The results for men and women are in a sense stronger than the turning points literature anticipates; Laub and Sampson (2001) write that desistance around family formation "will be gradual and cumulative."

We next compare these responses to the impact of a conventional policy lever, the ability to purchase alcohol at age 21. We replicate the findings of Carpenter and Dobkin (2015), showing that the men and women in our sample exhibit a strong offending response to the legal availability of alcohol: alcohol-related arrests before turning 21 are 24 percent lower for men and 32 percent lower for women. However, effects on other crime categories are small and insignificant. Thus, while alcohol availability causes similar sharp responses for a subset of crimes, parenthood is associated with a broader change in criminal activity.

Throughout, we find important heterogeneity by marital status at birth. First, long-run arrest declines are much larger for unmarried parents. Second, unmarried parents are arrested at much higher rates than married couples throughout the sample period, echoing previous work on the positive correlates of marriage (e.g. Akerlof, 1998; Waite & Gallagher, 2001). This latter finding raises the question of whether marriage plays a direct role in decreasing crime. Sociological research suggests that, compared to birth, marriage may have qualitatively different effects: according to interviews with low-income mothers, marriage is "reserved for couples who have already 'made it' " (Edin & Kefalas, 2011).

Our analysis supports this view. To study arrests around marriage, we augment our data with the state marriage index, matching over two hundred thousand marriages to the parents in our sample and applying a similar event study methodology that controls flexibly for age. We find that marriage is preceded by a substantial multi-year period of desistance: both men and women exhibit a 50 percent decrease in criminal arrests across categories in the 3 years prior to the marriage. After marriage, arrest rates are flat or increasing. This suggests that, while romantic partnership may be a turning point, marriage itself does not promote additional desistance.

Theoretical accounts in economics and sociology argue that the patterns should be different for marriages ending in divorce (Becker, Landes, & Michael, 1977; Laub & Sampson, 2001). We combine our data with statewide divorce records to study effects for unsuccessful marriages. Despite showing similar trends prior to the birth, couples and especially fathers headed toward divorce show a relative increase in arrests afterwards. While not dispositive, these findings are consistent with predictions from two prominent theories of marital quality: An economic theory that divorces result from negative surprises about the expected gains from the match (Becker, Landes, & Michael, 1977), and Laub and Sampson's turning points argument that desistance is more likely in the presence of strong social bonds.

Finally, while the data show that family formation events cause sharp decreases in most categories of arrests, these same turning points also clearly mark the onset of a new and particularly costly type of criminal event. Men exhibit a large spike in domestic violence arrests at birth and marriage, an effect that, in the case of birth, is almost large enough to undo an overall decrease in arrests for some groups. Some of this increase is likely due to an increase in cohabitation. However, these offenses are strongly related to our administrative information on divorces. Within married parents, domestic violence is much more common among those who eventually divorce, and, using the exact divorce date from our data, we show that divorce filings clearly coincide with increases in arrests for these offenses.

These empirical findings help clarify a large literature based primarily on small, selected (i.e., at-risk) samples with conflicting findings, which we review in Table A.1. Most papers find no or minimal effects of motherhood on crime, and results for fathers have been similarly mixed.[2] Further, the marriage results qualify a large literature that argues for a causal negative effect of marriage on crime.[3] Also novel to our context is the ability to separate out key types of offenses and study the precise timing of the arrest reductions, which helps rule out the possibility for long-term coincident changes that may have also played a role in desistance. The two most comparable studies, on the effects of marriage and childbirth on arrest for men and women (Skarðhamar & Lyngstad, 2009; Skardhamar, Monsbakken, & Lyngstad, 2014), use Norwegian register data and find broadly similar trends at an annual level but lack these important advantages.

We next turn to robustness. An important concern is whether sample attrition may be responsible for some of the observed decreases in arrests around turning point events, as we observe administrative outcomes only within the state of Washington. One piece of evidence against such sample attrition for fathers is the earlier observation that despite the declines in other crime categories, domestic violence arrests increase substantially. We also address this important concern explicitly in two ways. First, we use traffic arrests as a proxy for presence in the state and find that they are stable after births. Second, we find similar patterns when we re-estimate the results on a subsample for which we observe a ticket for an innocuous arrest in Washington state 4-5 years after birth.

An additional concern is that the decrease in arrests for women may reflect a decreased likelihood of apprehension among pregnant women. While all analyses use the recorded date of the alleged offense, not the date of the arrest, this channel could explain some of the decrease during pregnancy. However, it does not explain its persistence in the years following childbirth. A separate concern for women is that drug use may shift indoors following birth. Yet, we find that driving-related arrests gradually increase for mothers following birth, which is inconsistent with a broad decrease in activities outside of the home.

Finally, while much of the effects are concentrated during the pregnancy, we isolate the effect of having a child by building a control group using 3,281 stillbirth records, reported when gestation exceeds 20 weeks. The results reinforce the qualitative findings from the main analyses: fathers of liveborn children have greater

[2]For another recent review on mothers, see Giordano et al. (2011); for fathers, see Mitchell, Landers, and Morales (2018).

[3]For a critique and detailed review of the marriage effect, see Skardhamar et al. (2015).

levels of domestic violence following the birth, and mothers and fathers of liveborn children show decreased rates of drug arrests. This suggests that having a child, and not just making the decisions that produce one, decreases criminal behavior.

The pregnancy and childbirth results show strong reduced-form impacts on the levels of arrests and drug use. In addition, and novel to the literature, the detailed data allow us to study features of the transition paths to the decreased levels of arrests, such as the speed of the reduction and whether it occurs in advance of birth. These questions are especially relevant for drug-related crimes because the key prediction of economic models of addiction such as Becker, Grossman, and Murphy (1991) is that current and future drug use are complementary. Indeed, responses to future price shocks have the hallmark of studies of rational addiction (Becker, Grossman, & Murphy, 1994; Gruber & Köszegi, 2001).

We set up a version of the Beckerian rational addiction model based on O'Donoghue and Rabin (1999), building on a literature modeling the dynamic decisions of drug users and criminal offenders (Arcidiacono, Sieg, & Sloan, 2007; Arora, 2019; Lee & McCrary, 2005; Levy, 2010; McCrary et al., 2010; Sickles & Williams, 2008). The model has the two key features of rational addiction: recent use lowers the utility from any action but increases the marginal utility of drug use. We assume that childbirth is a shock to the utility of using drugs, and that mothers solve the dynamic discrete choice problem between use and abstention, knowing months in advance of the upcoming birth. While not a direct test of rational addiction, the model helps interpret the transition path around childbirth.

We fit the model to the observed drug arrest patterns for mothers using a minimum distance estimator. The model suggests that mothers respond to two utility shocks: a transitory shock at the end of pregnancy, and a permanent one following birth. Further, we find that the sharp changes observed in mothers are consistent with forward-looking behavior, as mothers are able to curb their use ahead of childbirth. The gradual adjustment into a new, lower steady state following birth is consistent with a role for habit formation, which is strikingly larger for married mothers.

Taken together, the results suggest that pregnancy is a strong inducement to reduce crime and drug use, even among groups that have not made explicit plans to have children. While the quality of marriages matters, the desistance that precedes marriage is as large if not larger than the childbirth effects. While teen pregnancy and out-of-wedlock births correlate with higher baseline levels of arrests and worse outcomes for children, policies exclusively focused on reducing these forms of childbearing may undervalue the large desistance effects for new parents. In contrast, the documented spike in domestic violence arrests may be important in informing policies targeting new parents.

1.2 Data

Our core analysis is based on two administrative data sources from Washington state: the Washington State Institute for Public Policy's criminal history database, a synthesis of data from the Administrative Office of the Courts (AOC) and the Department of Corrections (DOC); and still- and live-birth certificates from the Department of Health (DOH). We augment these the Washington marriage and divorce indexes, acquired from the Washington State Archives.

The criminal history data covers every criminal charge made from 1992 to 2015, including the date of the alleged offense, the criminal code, and the name and date of birth of the defendant.[4] We refer to a record in this data as an "arrest" for concision, although some events may not involve apprehension by a police officer and jail booking (e.g., a citation for reckless driving). The birth certificates span 1980 to 2009. We restrict to births after 1996 so that all parents are visible in the arrest data five years before and after the birth, a dataset we refer to as the "fully-balanced sample." The data includes the names and dates of birth of the mother and father, their races, the residential zip code of mother, and an indicator for whether the mother was married at birth. An average of 80 thousand births happen every year in the sample period, for about 1 million births in total.

We drop 5 percent of the birth certificates in the sample with the father missing. Washington is unusually good at recording fathers as it was one of the first states to implement in-hospital voluntary paternity establishment for unmarried mothers (Rossin-Slater, 2017). Similar data in Michigan has both parents on the birth certificate only 65 percent of the time (Almond & Rossin-Slater, 2013).

We match arrest records to birth certificates by implementing a fuzzy name match across parents and arrestees with the same date of birth. We drop parents who are strongly matched to multiple people in the arrest data, but we include parents who have no matches at all in the arrest data. The never-arrested sample is kept to help identify age controls in the regression analysis, and so that the count results presented below can be interpreted as population averages. The drops of ambiguously matched names constitute 5 percent of the birth certificates with fathers.

The crime categories in the data range from traffic infractions to murder. In most analyses, we group arrests based on categories constructed by the Washington State Institute for Public Policy. Arrests that we call economic consist primarily of 3rd degree theft, 2nd degree burglary, trespassing, and forgery. Drug crime categories include furnishing liquor to minors and possessing a controlled substance. Driving

[4]We attain similar results using a dataset covering all arrests from the Washington State Patrol Computerized Criminal History Database.

under the influence, the most common arrest in the data, is treated as its own category. Destruction includes vandalism and property damage more broadly. The most common domestic violence related arrest is for fourth degree assault, which is the least severe assault charge.

These five categories account for more than half of the arrests in the data. The bulk of the remaining arrests are either driving-related (e.g., reckless driving, driving with a suspended license), which we omit from the main analyses because they are conflated with driving activity; minor assault charges, which, because of patterns in their timing, appear to often be domestic-violence related due to inconsistent coding in the administrative data; and obstructing a police officer.

In the main analyses, we restrict to the parent's first birth as measured by matching parents within the birth records using the father's full name and date of birth and the mother's full (maiden) name and date of birth as reported on the birth certificates. Since the birth certificates begin in 1980, this means we will mislabel births as firsts if someone in our sample had their first child in 1979 or earlier.

We combine state marriage and divorce records with our sample by merging them to birth certificates using a fuzzy string match of the combined names of the spouses. This match comes with the caveat that only couples who at some point have a child together will be included. Since the marriage certificates do not contain birth dates, married couples could not be linked to the arrest data without first linking to the birth certificates.

In Table 1.1, we show how the sample characteristics change as we impose the restrictions mentioned above, starting with the entire sample of DOH births in column (1). Column (2) restricts to births where the mothers are clearly matched (or not matched) to the arrest data; column (3) adds the restriction that the birth is the mother's first child; and column (4) shows the characteristics for our sample of stillbirths, including the restrictions made in (2)-(3). Analogous descriptive statistics with the father as the focal parent are shown in Table A.2.

1.3 Event study evidence

Mothers

We start by showing the raw monthly arrest rates of mothers in the three years before and after the birth of their first child, using the main analysis sample of 480,111 mothers described above. Importantly, all of the analyses are constructed using the date of the alleged offense, not the date of arrest, which partially addresses the concern that arrest is less likely for visibly pregnant women. In this setup, t=0 marks the 30-day period beginning with the date of birth.

Figure 1.1(a) shows these arrest rates for mothers for four different categories of crimes. The plots show three consistent patterns: flat or slight positive trends leading up to the approximate date of the pregnancy (i.e., nine months before birth), large declines during pregnancy and especially in the first few months, and a sharp rebound in arrests following the birth. Property and non-DUI drug arrests are lower than the pre-pregnancy averages three years after the birth, while DUI and property destruction arrests show less of a long-term decline.

To remove age effects, we present similar plots displaying the event-time coefficients from regressions of the following form:

$$\mathbb{1}(arrest)_{it} = \alpha_i + \sum_{k \in S} \delta_k \mathbb{1}(t = k) + \mathbf{X}'_{it}\beta + \epsilon_{it} \tag{1.1}$$

where $\mathbb{1}(arrest)_{it}$ is equal to 1 if person i was arrested in month t, α_i denotes person fixed effects and $\mathbf{X}'_{it}$ includes a 4th-order polynomial in age and dummies for being above age 18 and 21. The set S runs three years in either direction from the birth, or -36 to 36. We bin up periods before -36 or after 36 into two separate dummy variables (i.e., $\mathbb{1}(t < -36)$ and $\mathbb{1}(t > 36)$), which allows us to estimate age effects, person fixed effects, and the event-time dummies without introducing collinearity. Standard errors are clustered at the person level, and in some specifications, we group event time indicators at the quarterly level to smooth out the arrest patterns.

In this event study setup, the effects of childbirth δ_k are identified by changes in arrests controlling for time-varying covariates. Effectively, the specification compares two women of the same age who have children at different times. Differences in their arrest rates are measured by the event-time indicators. These differences will capture the causal effects of pregnancy and childbirth if the onset of pregnancy does not coincide with other time varying-shocks (e.g., the beginning of a romantic relationship) that also affect arrests.

As we show below, we find limited evidence that pregnancy coincides with other arrest-reducing life changes for the mothers and fathers in our sample. Most importantly, there is no anticipation of the pregnancy. Any anticipation might reflect the impact of mothers meeting potential fathers and reducing their criminal activity as a result. Instead, decreases in arrests coincide exactly with the onset of pregnancy.

This implies that it is also unlikely that the patterns reflect the *decision* to try to become pregnant rather than pregnancy itself. If decisions were playing a role, we would expect at least some couples to fail to become pregnant quickly, generating dips in arrests before t=-9. Moreover, survey evidence suggests that the majority of births to unwed mothers, who drive our results, are unplanned (Mosher, Jones, &

Abma, 2012). Similarly, below we obtain very similar results among teen mothers, for whom 78% of pregnancies are unintended (Mosher, Jones, & Abma, 2012).

We present results for the event study specification with the outcome, $\mathbb{1}(arrest)_{it}$, equal to one in any month that the mother was arrested for any of the four crime categories. These estimates, shown in Figure 1.1(b), closely match the simple averages given in the raw figure, suggesting a sustained 50 percent decrease in arrest rates. We report a subset of the event-time coefficients for the four different crime categories in Table 1.3. The decline during pregnancy is substantial, with the four crime categories decreasing by 70-95 percent relative to pre-pregnancy levels. These effects also capture the considerable rebound following pregnancy, with, for example, DUI arrests going from practically zero in the month of birth to only 48 percent lower than pre-pregnancy levels in the third month following birth.

These event study specifications similarly show no evidence of anticipation. There are small declines in t=-8, when many mothers learn they are pregnant, and the largest decline in t=-7, by which time almost all mothers know (Branum & Ahrens, 2017). This is consistent with evidence, based on self-report, that pregnancy intention does not predict alcohol cessation (Terplan, Cheng, & Chisolm, 2014).

Alcohol offenses

Contrary to the other three categories, the raw averages of DUI arrests in Figure 1(a) show an eventual increase after birth. This appears to due to the fact that women are more likely to be driving. Partial evidence for this is that more innocuous arrests related to driving, such as driving without a license, are increasing over the sample period (Figure A.1).

For more insight into drinking behavior, we turn to two common alcohol-related arrests for people under the age of 21: alcohol possession and furnishing liquor to minors. We perform this analysis for women who become mothers at or before the age of 20 or younger and plot results until age 21 in order to remove the confounding effect of reaching the legal drinking age, which brings the sample size down to 67,899 mothers. The plot of these alcohol arrests is given in Figure A.2. Similar to the non-alcohol drug arrests in the previous plot, the figure suggests a sharp, largely sustained desistance at the beginning of pregnancy.

Teen mothers

Economists still debate the consequences of teen pregnancy: influential research using miscarriages as a control finds minor negative and even some positive effects of teen childbearing (Ashcraft, Fernández-Val, & Lang, 2013; Hotz, McElroy, & Sanders, 2005; Hotz, Mullin, & Sanders, 1997).[5] However, Fletcher and Wolfe (2009) use a similar design with different data and find strictly negative effects on education and income, leading to a recent summary that the "[n]egative consequences of teen childbearing are well documented" (Yakusheva & Fletcher, 2015).

We next turn our attention to these women, defined as those who give birth before turning 20. We plot the coefficients from the event study specification for the four main crime categories in Figure A.3, where the coefficients are normalized by the pre-pregnancy average to give the fractional change in arrest rates. Motherhood remains a large driver of desistance for this subgroup. As in the full sample, drug and property crimes show a sharp and largely sustained decreases to half of the pre-pregnancy levels. These plots are also meaningful because 78% of teen mothers report that their births resulted from unintended pregnancies (Mosher, Jones, & Abma, 2012). The results provide perhaps the clearest evidence to date that childbearing is a turning point for even very young women.

Fathers

We next turn to first-time fathers. Figure 1.2(a) shows the average monthly arrest rate for fathers for the same four crime categories as mothers. While less sharp than the effects for mothers, large drops are visible in these raw averages, especially for drug arrests. Between pregnancy and three years after birth, drug arrests fall from 17 to 11 for every 10,000 men.

These effects are broadly similar when measured using the event study specification. As with the analysis for mothers, we estimate the event study specification combining these four categories of arrests and plot the results in Figure 1.2(b). The results show clear evidence of a steep decline, stabilizing at 30 percent less than the arrest rates at the start of the pregnancy. Point estimates for a subset of the event-time coefficients are reported in Table 1.4.

The declines in arrests compare favorably to the deterrent effects of exceptionally harsh punishments. Under California's three-strikes law, offenders with two strikes faced almost 20 years of additional prison time and exhibited a decrease in annual felony offenses of 15 to 20 percent (Helland & Tabarrok, 2007). In Italy, Drago, Galbiati, and Vertova (2009) find that an increase in expected sentences among recently released prisoners by 25 percent would decrease re-arrests in 7 months by 18 percent. Our results on arrest rates are not directly comparable to estimates of recidivism for people recently released from prison. However, the probability of any

[5] For an overview of the causal effects of teen childbearing, see Kearney and Levine (2012), who conclude that "most rigorous studies on the topic find that teen childbearing has very little, if any, direct negative economic consequence."

arrest in a longer period shows the same large decline: among all of the first-time fathers in our sample, the share arrested for any drug offense goes from 1.7 percent in the year before pregnancy to 1.2 percent in the year after birth.

A striking feature of these plots is that, as with women, most of the decrease occurs during the pregnancy, despite the fact that men do not directly experience any of the physical effects of pregnancy. While new to the quantitative literature, this response is consistent with qualitative research asking at-risk fathers how they reacted when they learned about a partner's pregnancy. Edin and Nelson (2013) note that, "Men are drawn in—usually after the fact of conception...[and] usually work hard to forge a stronger bond around the impending birth" (Edin & Nelson, 2013, p. 203). Further, when describing a representative case, they write,

> Upon hearing the news that the woman they are "with" is expecting, men such as Byron are suddenly transformed. This part-time cab driver and sometime weed dealer almost immediately secured a city job in the sanitation department (Edin & Nelson, 2013, p. 36).

Heterogeneity

Second births

The results for childbirth are consistent with two broad explanations. First, childbirth could initiate a permanent change in preferences. For instance, having a child could cause people to derive less utility from drug use or crime, or make them more future-regarding. However, an alternative explanation is that childbearing affects crime purely through its effect on the time budget. The presence of a young child could create a temporary incapacitation effect due to childcare or housework. We attempt a comparison of these two theories by comparing the first to the second birth. The first theory predicts that most changes should be concentrated in the first birth, while the incapacitation channel suggests similar effects regardless of birth order.

In Figure 1.3, we show the same event study coefficients split by birth order. In order to use a consistent sample, the underlying data retains all mothers and fathers whose first and second children are both born in the fully-balanced sample period. The plots show that, for both mothers and fathers, the bulk of the desistance happens at the first birth. Three years after their second birth, mothers are arrested at levels similar to before the pregnancy. Fathers experience a 10 percent decrease in arrests compared to 30 percent for the first birth. That second births could still spur a sustained decrease for fathers is consistent with the fact that some men only start investing in children for later births, while this is less common for women (Edin & Nelson, 2013).

Birth effects by marital status

Next we split the fathers and mothers by marital status. Marital status at birth has long been a focal metric of policy makers, and the descriptives in Table 1.2 show clear differences in the probability of arrest and incarceration across the two samples. Unmarried fathers are twice as likely to have ever been arrested, and seven times as likely to have had an incarceration spell. Since married couples are already less prone to crime, the additional effect of childbirth may have a less stabilizing effect. On the other hand, an unmarried childbirth may present a significant income shock, leading to increased economic offenses.

Figure 1.4 presents similar event study plots by the mother's marital status as reported on the birth certificate, showing effects on the monthly arrest rate for any of the four main crime categories. In these plots, we add the omitted-period average in order to display the stark level differences in arrest rates between the two groups. Both unmarried and married mothers exhibit a large "incapacitation" effect during the pregnancy. However, childbirth presents less of a permanent change for married mothers. By the end of our sample window, they are arrested at similar levels to before the pregnancy.

Similar to the main results, there are no signs of anticipation ahead of the pregnancy for either group. This might be expected for unmarried women, where more than half of all births are unintended. However, for married women only 23 percent of births are unintended (Mosher, Jones, & Abma, 2012, Table 2), and many couples spend months trying to conceive (Keiding et al., 2002). This could be further evidence that the decision to have a child does not influence criminal activity. However, it could also be that the criminally-active married women who drive the estimates are much more likely to have unintended pregnancies.

Figure 1.5 plots the same event study estimates for married and unmarried fathers. Similar to mothers, unmarried fathers have much higher arrest rates, but this discrepancy shrinks somewhat following the birth. Unmarried fathers show some increase in arrests leading up to the birth, which could be due an increased level of activity in Washington correlated with the timing of their relationship with the mother. As a robustness check, we show in Figure A.4 that, among unmarried fathers, two groups with stronger attachment to the state display flat pre-trends leading up to the pregnancy but similar sharp declines in arrests at pregnancy: those born in Washington state and those with at least one juvenile criminal charge.

1.4 The role of marriage

Arrests around marriage

A clear finding of the previous section is that there are large level differences in criminal arrests by the parents' marital status at birth. Marriage itself is a prominent feature of the turning points framework. In qualitative studies, formerly delinquent men often attribute considerable weight to marriage: "If I hadn't met my wife at the time I did, I'd probably be dead. It just changed my whole life...that's my turning point right there" (Sampson & Laub, 2009, p. 41). Married men also earn more: in economics, a long literature debates the content of the male marriage wage premium e.g. Antonovics and Town, 2004.

To analyze criminal arrests around marriage, we produce plots of the event study coefficients in specifications analogous to Equation 1.1 in Figure 1.6, where $t = 0$ corresponds the 30 day period starting with the date of marriage. Marriage is preceded by a long decline in arrests; for male drug and economic arrests, the decrease amounts to a more than 50 percent decrease from three years before the marriage. The decline continues until the month of marriage, where all crime categories either stabilize or increase slightly. These event study plots closely match the raw averages, shown in Figure A.5.

These figures add important nuance to the qualitative literature, which has largely interpreted the marriage effect as causal.[6] For instance, in recent work, Sampson and Laub (2009) write: "Selection into marriage appears to be less systematic than many think...[m]any men cannot articulate why they got married or how they began relationships, which often just seemed to happen by chance." The plots suggest clearly that romantic partnerships are important, demarcating a large decrease in arrests, but the association could be either because of the relationship or other factors simultaneously decreasing crime and increasing the probability of marriage.

Good marriages, bad marriages

Economic models going back to Becker, Landes, and Michael (1977) posit that divorces happen in response to negative information about the expected gains from the union (for a more recent example see Charles & Stephens, 2004), and in sociology a core tenet of turning points theory is that marriage itself does not guarantee desistance—relationships are salutary to the extent that they are characterized by high attachment (Sampson & Laub, 1992). The turning points theory plainly predicts that desistance should be less pronounced for bad marriages. The model in

[6] However, see Skardhamar et al. (2015) for a critical assessment.

Becker, Landes, and Michael (1977) implies that divorce should be preceded by some negative surprise.

In order to probe these ideas, we combine our data with statewide divorce data from Washington. We plot descriptive statistics for married and eventually divorced couples in Table 1.5. This sample includes all births where the parents were married and it was a first birth for either the mother or father. Parents who get divorced are younger, reside in poorer zipcodes, and are more likely to be white or black (and less likely to be Hispanic or Asian). Perhaps most importantly, men and women who are headed for divorce are both about twice as likely to have any arrest.

We show the raw averages in Figure 1.7, but to account for these level differences we subtract and divide by the pre-pregnancy averages in the raw plots. We compare couples still married in five years to those who have divorced by that time. The outcome is an indicator for any of the four main categories of arrest (results look similar for any of these categories separately). Compared to their past levels of arrest rates, women headed for divorce have slightly higher rates of arrests post-birth, despite broadly similar trends leading up to the pregnancy. These same effects are present and much more pronounced for men.[7]

These results are consistent with the idea that "spousal attachment" is pivotal to maintaining desistance, although the parallel trends leading up to the birth suggest that preparation for a child can be just as impactful for couples who will eventually divorce (Laub & Sampson, 2001). The results are also broadly consistent with economic conceptions of marital dissolution as in Becker, Landes, and Michael (1977) arguing that divorce occurs in reaction to unexpected changes to the gains from the union. Of course, unobserved variables—for example, income—related to crime and divorce could be driving these results. Still, the figures show clearly that, relative to past levels, increases in arrests precede dissolution.

1.5 Comparison to age 21 discontinuity

Studies in criminology and economics generally focus on discrete changes in enforcement regimes in order to measure elasticities, such as California's three strikes policy (Helland & Tabarrok, 2007); the increased punishments associated with turning 18 (Lee & McCrary, 2005) or having blood alcohol above a certain level (Hansen, 2015); and the ability to purchase alcohol legally at age 21 (Carpenter & Dobkin, 2015). We can use our data to replicate the design in Carpenter and Dobkin (2015), which employs a regression discontinuity approach to measure the increase in arrests that

[7] The results are very similar using marriages as the focal event, and controlling for age effects in the event study specification.

occurs when people turn 21. This presents a unique opportunity to compare the effects of parenthood to a widely studied criminal justice policy.[8]

To maintain the same sample, we keep all men and women who are in our parents sample and also have a 21st birthday between the years 1995 and 2012, inclusive. This gives us a balanced panel of arrests in the three years before and after the birthday. Next, we take average arrest rates around age 21 in monthly bins.

Figure 1.8 shows the results with alcohol-related arrests as the outcome variable, and with the y-axis scaled by average arrest rates in the post-period. There is clear visual evidence of a discontinuity in arrest rates for alcohol-related arrests. However, the plots for all other crime categories show no response. Table 1.6 shows regressions estimated at the daily level including a quadratic in time since 21st birthday, interacted with the indicator for being above age 21; and dummies in the weeks containing birthdays to capture any birthday-related spikes, as in Carpenter and Dobkin (2015). We also report the average arrest levels in the six months after the 21st birthday.

Based on these estimates, the effects are similar in magnitude (although opposite in sign) to the childbirth estimates: alcohol arrests before turning 21 are 24 percent lower for men and 32 percent lower for women. However, these arrests are just 6 percent of total charges of the sample window, and the regression discontinuity finds small and insignificant effects on the total amount of arrests.

1.6 Domestic violence

The previous analyses on turning points leave out a critical caveat that, to our knowledge, has not received any explicit mention in the host of quantitative studies on crime and family formation. The results for men around marriage and childbirth coincide with a large increase in domestic violence arrests.

Figure 9(a) shows raw averages for domestic violence arrests among fathers in the full first birth sample. Domestic violence arrests increase up until the start of the pregnancy, decrease sharply, and then markedly spike on the month of the birth. The increase leading up to t=-9 may reflect the selection of our sample, as relationships increasingly form ahead of the pregnancy. The decrease during pregnancy appears consistent with norms against assaulting pregnant women, when violence may also harm the developing fetus (Currie, Mueller-Smith, & Rossin-Slater, 2018). Finally, the spike at birth might help explain why recent studies found ambiguous effects of fatherhood on overall arrest rates (e.g. Mitchell, Landers, & Morales, 2018). In Figure 1.9(b), we show, also using the raw averages, that a similar spike is visible around marriage.

[8] To our knowledge, this is the first large-scale replication of Carpenter and Dobkin (2015).

Our data measure arrests with a high degree of accuracy, but the connection between arrests and violent behavior over the sample period is less certain if the propensity to report domestic violence changes around pregnancy and childbirth. Victimization surveys, which may be more accurate compared to measures based on police involvement, confirm the qualitative finding that domestic violence is more likely after the pregnancy than during: in a nationally representative survey, 1.7 percent of mothers reported physical violence during the pregnancy compared to 3.1 percent in the first post-partum year (Charles & Perreira, 2007).[9]

These domestic violence arrests also give a strong indication of the likelihood of divorce. Figure A.6(a) shows father's domestic violence arrests split by divorce status five years later, normalized by pre-pregnancy means to account for large level differences between the two groups. Despite similar pre-trends, men destined for divorce show a much larger spike in domestic violence following the birth. Figure A.6(b) focuses on these divorced men, grouping them based on whether they divorced 1, 2, 3 or 4 years after the birth. (Importantly, this uses the date that the divorce was finalized, which is at least 90 days after the date of filing.) The plot shows clearly that domestic violence spikes ahead of the divorce decree.

1.7 Robustness

Outmigration

The biggest potential confound in our setting is outmigration. Defining our sample around birth imposes selection: men are most likely to be physically present in Washington at the time of conception. Since our data only cover arrests in Washington, it is possible that the arrest patterns reflect migrations out of the state—and therefore unobservable attrition—following pregnancy or birth.[10] The most immediate argument against this threat is the clear increase in domestic violence following the birth. For migration to explain the decrease in drug arrests, the men accounting for the spike in domestic violence would need to have a much lower propensity to be arrested for drug offenses. However, arrests are correlated across offense types: men with more drug arrests tend to have more domestic violence arrests.

[9] Further, in an interview, a Seattle police officer said that the presence of children would not affect the likelihood of an arrest due to Washington's strict mandatory arrest law. However, the evidence here is indirect, and a recent meta-analysis concluded that "the research community still does not know for sure whether pregnant women are at higher or lower risk of being physically abused" (DeKeseredy, Dragiewicz, & Schwartz, 2017).

[10] Incarceration poses an analogous attrition problem as men in our sample are least likely to be in prison ten months before the birth; results using only never-incarcerated fathers are identical.

To have a proxy of residence less correlated with drug use and criminal propensity, we look at the most innocuous offense in our data: traffic arrests, consisting primarily of driving with a suspended license and not displaying a license on command. Figure A.7 shows that in both the raw averages and event study specification controlling for age men do not exhibit a decreased risk of arrest for these offenses after the pregnancy or birth, so any explanation centered on outmigration would hinge on higher-risk men selectively leaving the state.

Finally, we focus on men with greater attachment to the state in the post-birth period by restricting the sample to the 69,900 fathers who commit a DUI or traffic offense in the endpoints of our sample, i.e., 4-5 years after the birth. In Figure A.8, we show that this sample, which should be much less contaminated by migration attrition, shows a similar 25 percent decrease in drug arrests. If migration were affecting the results and fathers physically present in Washington had stable levels of arrest rates, we would expect the decrease for this group to be much smaller.

These findings are reassuring that migration is not impacting the analyses around pregnancy and birth. As for the marriage findings, migration-based attrition would bias the results in the opposite direction: marriage applicants typically need to be physically present to attain a marriage license. The results, therefore, may even understate the decline ahead of marriage if people are less likely to be in Washington in the years preceding.

Stillbirths

The preceding sections provide evidence on the causal impact of a pregnancy assuming the onset of pregnancy does not coincide with other time-varying confounds. In this section we construct a sample of couples who experience a pregnancy that ends in a late-stage miscarriage. If the outcome of the pregnancy has a causal effect on arrests in line with the previous results, parents to stillborn infants should show higher rates of arrests post-pregnancy.

A stillbirth is the delivery, at some point after the 20th week of pregnancy, of a baby who has died. Hospitals are legally required to report stillbirths if the gestation period is 20 weeks or more. Importantly for our purposes, there is still comparable coverage of the fathers' name and date of birth, which are only missing from 9 percent of the stillbirths.

Existing work using miscarriages as an instrument (e.g. Hotz, McElroy, & Sanders, 2005) includes all reported miscarriages, not just those occurring after 20 weeks of gestation. This could bias estimates if some of the early miscarriage sample would have gotten an abortion, and since among pregnant teens those who receive abortions are positively selected with respect to economic outcomes (see Hoffman, 2008). An advantage of our sample is that it does not have this censoring issue since over 90 percent of abortions occur before the 13th week of gestation (Jatlaoui et al., 2018).

On the other hand, stillbirths are less commonplace than miscarriages and often have distinct causes affecting the health of the mother such as pre-eclampsia, bacterial and viral infections, other medical conditions, and possibly domestic violence (Lawn et al., 2016). Further, the experience of a stillbirth is often followed by a pronounced period of bereavement (Heazell et al., 2016). As a check on the influence of these physical or psychological consequences, we find similar effects looking at periods 6 months or more beyond birth, rather than immediately afterwards.

The last column in Table 1.1 shows descriptive statistics for the stillbirths in our sample, restricting to those having a clear match in the arrest data and that are the mother's first birth. Mothers to stillborn babies are 10 percentage points less likely to be married but are otherwise positively selected based on receipt of WIC and arrest probabilities. Also, mothers in our data who experience stillbirths exhibit greater variance in age than mothers to liveborn children, and the infants are likely to be male and twins, in line with medical studies on risk factors (Lawn et al., 2016).

Since arrests are rare and our stillbirths sample is relatively small, we shift to a simple difference-in-differences specification to reduce noise. The specification includes person fixed effects and an indicator for post-birth interacted with an indicator for live birth:

$$y_{it} = \alpha_i + \gamma * preg_{it} + \delta_1 * after\_birth_{it} + \delta_2 * after\_birth_{it} * live_i + x'_{it}\beta + \epsilon_{it} \quad (1.2)$$

where $preg_{it}$ is equal to one for $t \in \{-9, -1\}$ and $after\_birth_{it}$ is an indicator for $t \geq 0$. The pregnancy indicator is included to remove the decline in arrests observed in the earlier results from the implicit pre-period estimates. We obtain similar results interacting the pregnancy and live indicators. The vector x'_{it} includes a 4th-order polynomial in age and dummies for being above age 18 and 21.

The results, shown in Table 1.7 for men and Table 1.8 for women echo the main results. Column (1) shows the results for the four main crime categories from the event study analysis, split out separately in columns (3)-(6); column (2) shows the effects on domestic violence. Fathers to liveborn children commit more domestic violence following the birth, but less of the four main offense categories. Columns (4) and (5) suggest that this is driven by drug and economic offenses, although the latter result is not significant. Mothers similarly show a reduced rate of drug arrests following the birth, with significantly fewer drug and property destruction offenses.

1.8 A model of habit formation

The previous findings show large effects on drug arrests. How much of these responses are consistent with potentially addicted users rationally adjusting behavior in anticipation of a large change to their environment?

Economists have often employed habit-formation models in the style of Becker and Murphy (1988) in order to study addictive behavior, but most studies focus on one-time decisions or annual panels. Our context has the advantage of having a proxy for drug use at the monthly level, and is built around a clear and powerful utility shock to drug use. Building off of O'Donoghue and Rabin (1999) and Becker and Murphy (1988), we use this setting to study the implications of a dynamic discrete choice model of rational addiction. We focus on mothers because the distinct changes during pregnancy and after birth provide greater latitude for identification of the model parameters.

Setup

Following O'Donoghue and Rabin (1999), we consider a dynamic discrete choice model where addiction is based on use in the previous period. Finitely-lived agents maximize a discounted stream of utility stemming from their choices of whether to use each period $a_t \in \{0, 1\}$, and enter each period either clean or addicted $k_t \in \{0, 1\}$. Addiction is simply whether or not the agent used last period, $k_t = a_{t-1}$. When clean, the utility from using is f_t, and the utility from refraining is normalized to 0. When addicted, agents get $f_t - \rho$ from using and $-\rho - \sigma$ from refraining. These payoffs are illustrated below.

	$U_t(1, k_t)$	$U_t(0, k_t)$
Clean ($k_t = 0$)	f_t	0
Addicted ($k_t = 1$)	$f_t - \rho$	$-\rho - \sigma$

The following assumptions to capture two key features of drug addiction:

(1) **Internalities**: utility from any action is higher when clean ($\rho > 0$)

(2) **Habit formation**: the utility gain from using is higher when hooked ($\sigma > 0$)

The addiction parameters σ and ρ are static, but f_t is allowed to change after childbirth:

$$f_t = \begin{cases} f \text{ for } t < 0 \\ f - \Delta f \text{ for } t \geq 0 \end{cases}$$

Finally, agents maximize the discounted stream of utility payoffs:

$$U = \sum_{t \in S} \delta^{t+36} U_t \tag{1.3}$$

where t indexes months since childbirth and S includes all periods between -36 and 36 months around birth. We assume that the errors are distributed generalized extreme value, which allows for analytic solutions for the probability of using drugs in any given period. These are given by

$$P(t, k_t) = \frac{e^{U(1,k_t)+\delta V_{t+1}(1)}}{e^{U(1,k_t)+\delta V_{t+1}(1)} + e^{U(0,k_t)+\delta V_{t+1}(0)}} \tag{1.4}$$

where $V_t(k_t)$ is the value of entering into period t in state k_t and $P(t, k_t)$ is the probability of using in period t in state k_t. Under these assumptions, the optimal path of discrete choice probabilities can be solved using backward recursion.

Illustrative examples

Figure A.9 plots the choice probabilities around birth for a fully forward-looking agent with $\delta = 1$, a high degree of habit formation, and a large decrease in use utility starting at $t = 0$. At news of the shock at $t = -9$, the agent decreases her probability of use immediately, then spreads her adjustment to the new steady state levels into the first 12 months after birth.

In the data, mothers' arrests show a considerable rebound following the low levels reached during childbirth. In order to fit this pattern, we assume that mothers experience an additional shock to drug use utility during pregnancy,

$$f_t = \begin{cases} f \text{ for } t < p \\ f - \Delta_1 f \text{ for } t \in \{p, 0\} \\ f - \Delta_2 f \text{ for } t \geq 0 \end{cases}, \tag{1.5}$$

where $p \in \{-10, ..., -1\}$. In this parsimonious setup, the data are best fit with $p = -2$, since the presence of habit formation and some degree of patience creates an incentive for mothers to begin desisting in anticipation.

Figure A.10 illustrates the choice probabilities of the model with the added shock. Without habit formation, both adjustments are made instantaneously (Panel (a)).

With large σ, myopic agents make sudden adjustments in the later part of pregnancy, but still ease into the new steady state (Panel (b)). Finally, as agents become more future-regarding, reaction to the news of pregnancy becomes sharper (Panels (c) and (d)).

Identification of the f and two Δf terms comes from the initial level and the two level changes during the pregnancy and after. As illustrated in Figure A.10, δ and σ are identified off of the two the transition paths during pregnancy and after: The transition from pregnancy to $t-2$ identifies δ, since this captures the immediacy of the response to a future shock. The slope into the new steady state following birth identifies σ, since non-myopic agents will only ease into a new steady state given some degree of habit formation.

Estimation

We fit the model to the data using a minimum distance estimator. The estimator minimizes the distance between the moments predicted by the model and the observed moments, where the observed moments are the raw observed drug arrest rates in the data.[11] The predicted moments are direct outputs of the logit framework given above. Since σ and ρ are not separately identified, we fix $\rho = 1$ and estimate four parameters: σ, the degree of habit formation; f, the utility of using; Δf, the change in f; and δ, the discount rate.

The results of this exercise are shown for unmarried and married mothers in Table 1.9, with the corresponding figure showing the raw data along with the simulated vector the probabilities of using in Figure 1.10. The point estimates suggest that mothers in either group are not fully myopic. Although the standard errors cannot reject high levels of discounting, the steep slope leading up to birth is consistent with strongly forward-looking behavior.

Both groups experience similar utility shocks during pregnancy, but the long-run change for married mothers, as foreshadowed in the empirical section, is almost zero. Most interestingly, the estimates suggest a higher level of habit formation for married mothers due to their slow adjustment into the new steady state. The higher levels of habit formation in turn imply that in their clean state, married mothers get a greater level of utility from using than unmarried mothers.

The results are thus broadly consistent with a habit formation framework in the style of Becker and Murphy (1988) allowing for utility shocks marking key moments in childbearing. In particular, the habit formation framework helps explain the

[11] In order to better approximate actual crime rates, and following Lee and McCrary (2005), we scale the empirical moments by 10 in accordance with estimated clearance rates around 10 percent.

slow transition into the steady state levels of arrests in the years following the birth. Interestingly, and as partial support for the habit formation approach, these patterns are unique to drug offenses: economic offenses for mothers show a much sharper rebound into the post-birth steady state, as shown in Figure 1.1.

1.9 Conclusion

How does someone change when they wed or become a parent? The previous sections uncover several novel patterns in criminal arrests around childbirth and marriage, leveraging a detailed administrative sample and providing clear evidence on the size and nature of "turning points." For mothers, childbirth is transformative, even with the large rebound in arrests that occurs after pregnancy. For fathers, a smaller but still significant decrease occurs in the same offenses. Marriage, in the words of Edin and Kefalas (2011), is reserved for couples who have made it. However, the increase in domestic violence around both births and marriage is a significant qualifier.

Parenthood is not a policy, although governments take a wide range of actions in order to prevent teen pregnancy, support marriage, and encourage father involvement. Our findings on teen mothers provide some of the strongest evidence to date against the conventional wisdom around its consequences. Further, the novel findings on the timing of desistance for fathers suggest that pregnancy could be a uniquely potent time for interventions promoting additional positive changes. Finally, the stark patterns in domestic violence arrests may argue for expanding the purview of home visitation programs in the postnatal period, typically directed at child welfare (Bilukha et al., 2005).

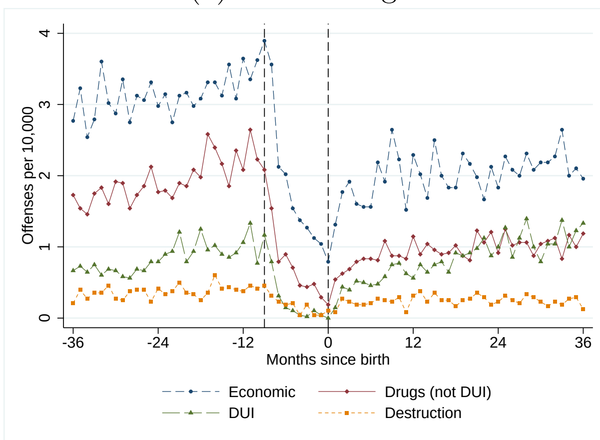
The findings on drug arrests in particular have two implications about incentive-based approaches to treatment: first, that drug use can respond to incentives; second, that incentives built around social bonds could be powerful. The first point challenges definitions of addiction which assert that drug use is the outcome of involuntary impulses.[12] And while the experience of childbearing cannot be synthesized in an intervention, addiction experts observe that some successful treatments, such as Alcoholics Anonymous, are based on promoting social cohesion and interdependence (Heyman, 2009).

[12]For example, the National Institute on Alcohol Abuse and Alcoholism (NIAAA), defines drug abuse as a disease: "Addiction is a chronic, often relapsing brain disease...[s]imilar to other chronic, relapsing diseases, such as diabetes, asthma, or heart disease"

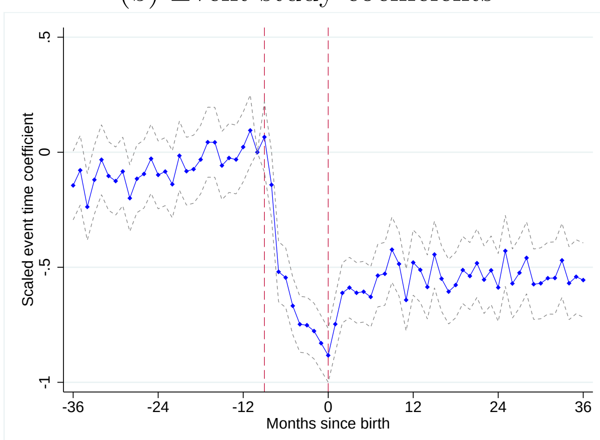
1.10 Figures

Figure 1.1: Monthly arrest rate around first birth, All mothers

(a) Raw averages



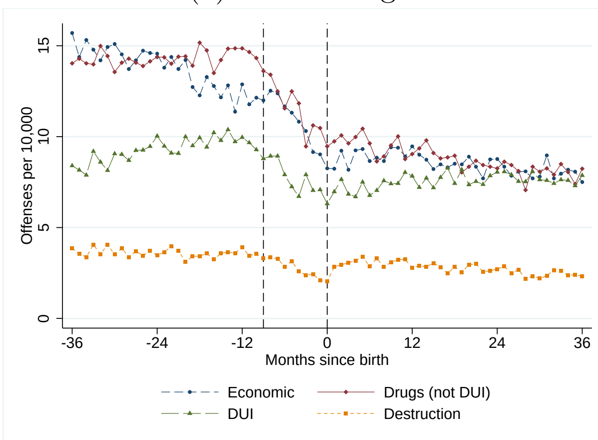
(b) Event study coefficients



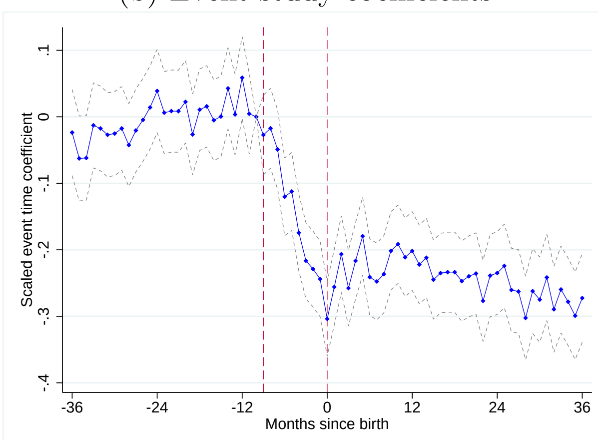
Includes fully-balanced arrest data for 480,111 first-time mothers. DUI stands for driving under the influence. In panel (b), the dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 1.1, with an indicator for any arrest in the four crime categories from panel (a) as the dependent variable. The coefficients are divided by the average arrest rate in the omitted period, 10 months before birth. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 1.2: Monthly arrest rate around childbirth, All fathers

(a) Raw averages



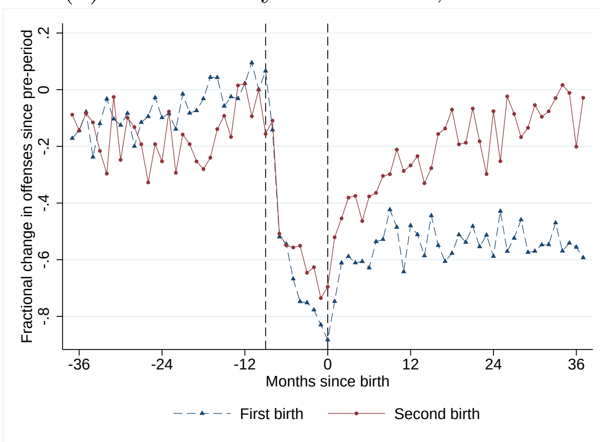
(b) Event study coefficients



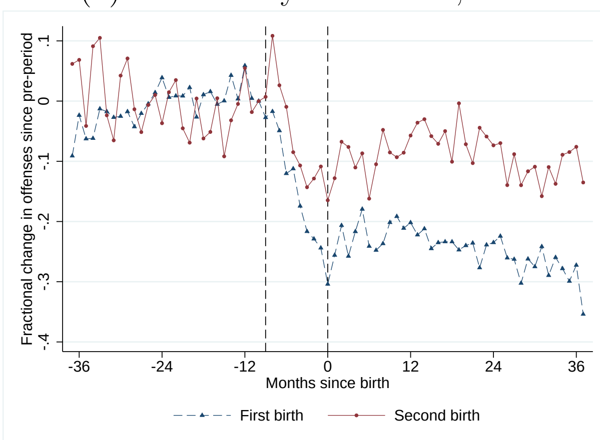
Includes fully-balanced arrest data for 545,166 first-time fathers. In panel (b), the dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 1.1, with an indicator for any arrest in the four crime categories from panel (a) as the dependent variable. The coefficients are divided by the average arrest rate in the omitted period, 10 months before birth. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 1.3: Second births

(a) Event study coefficients, women



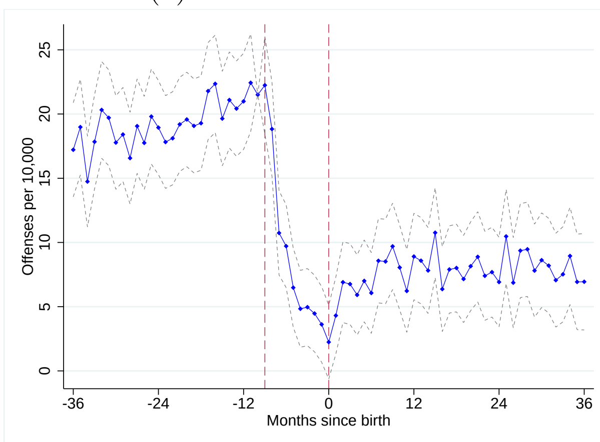
(b) Event study coefficients, men



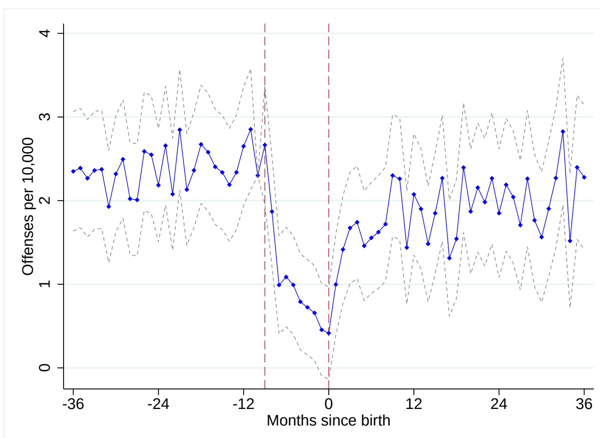
Plots show coefficients δ_k from the event study specification show in Equation 1.1 with an indicator for any drug, DUI, economic, or property destruction arrest as the dependent variable. Each line represents a separate regression run using fully-balanced arrest data on the women (panel (a), N=160,360) and men (panel (b), N=180,557) with two births in the sample window. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 1.4: Mother heterogeneity by marital status, event study coefficients

(a) Unmarried mothers



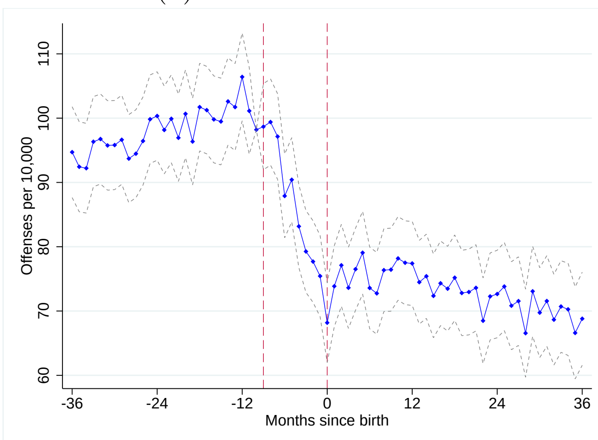
(b) Married mothers



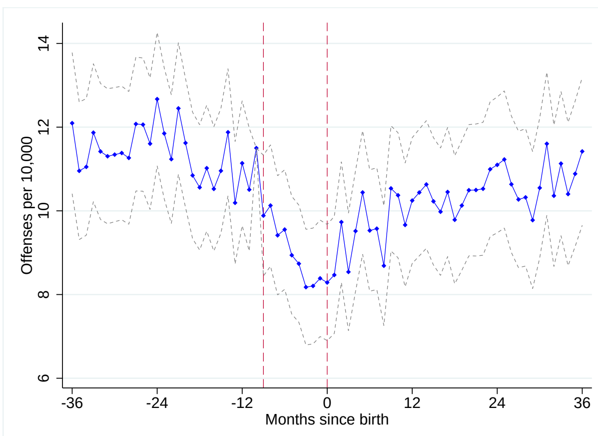
Includes fully-balanced arrest data on 112,016 unmarried and 368,095 married first-time mothers. Dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 1.1, with an indicator for a drug, DUI, economic, or property destruction arrest as the dependent variable. The omitted period is 10 months before birth and the arrest rate in the omitted period is added to the coefficients to show average arrest rates. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 1.5: Father heterogeneity by marital status, event study coefficients

(a) Unmarried fathers



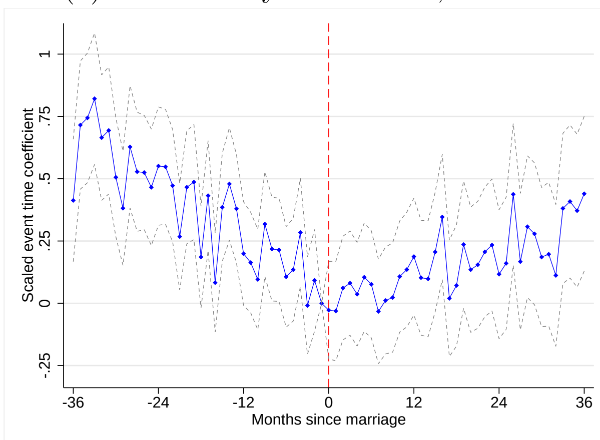
(b) Married fathers



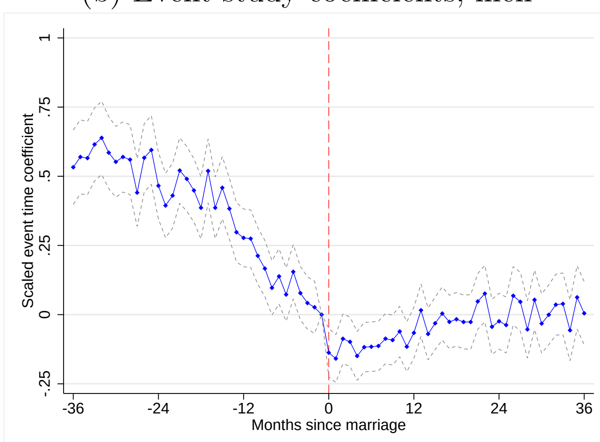
Includes fully-balanced arrest data on 160,052 unmarried and 385,114 married first-time fathers. Dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 1.1, with an indicator for a drug, DUI, economic, or property destruction arrest as the dependent variable. The omitted period is 10 months before birth and the arrest rate in the omitted period is added to the coefficients to show average arrest rates net of age effects. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 1.6: Plots of arrests around marriage

(a) Event study coefficients, women



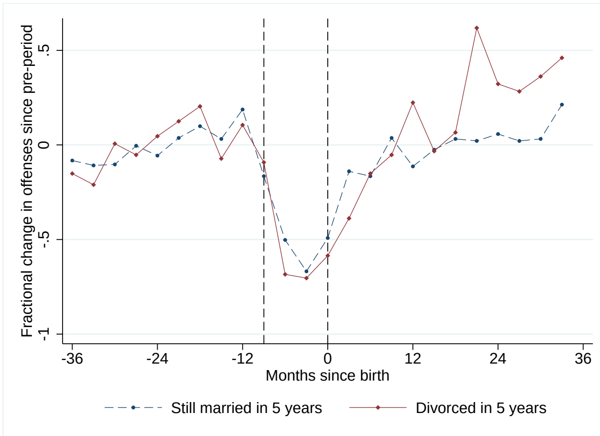
(b) Event study coefficients, men



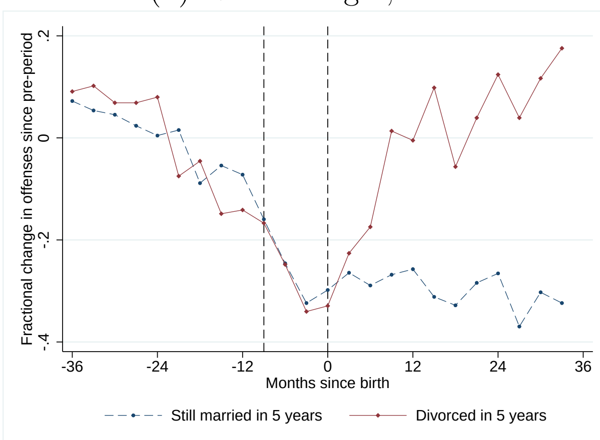
Includes all fathers (N=245,756) and mothers (N=222,392) from the birth data who are visible in the arrest data 3 years after and 3 years before their marriage. Dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 1.1, with an indicator for a drug, DUI, economic, or property destruction arrest as the dependent variable. The omitted period is one month before birth. The vertical dashed line marks the month of marriage.

Figure 1.7: Heterogeneity in the effect of childbirth between good marriages and bad marriages

(a) Raw averages, women



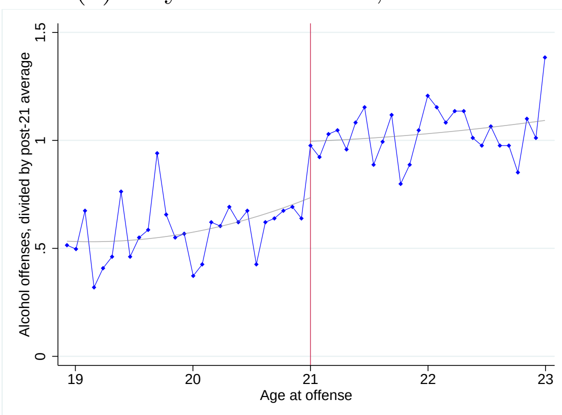
(b) Raw averages, men



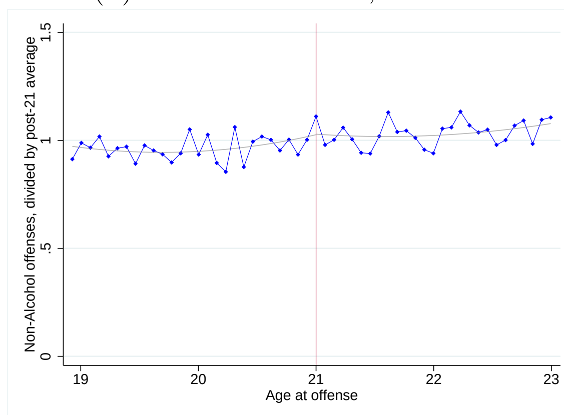
Panel (a) includes fully-balanced arrest data on 349,779 still-married women and 18,316 divorced women. Panel (b) includes fully-balanced arrest data on 364,076 still-married men and 21,038 divorced men. The outcome is any drug, DUI, economic, or property destruction arrest. Divorce classification is derived from a fuzzy match between the Washington state marriage and divorce indexes. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 1.8: Regression discontinuity evidence using the minimum legal drinking age

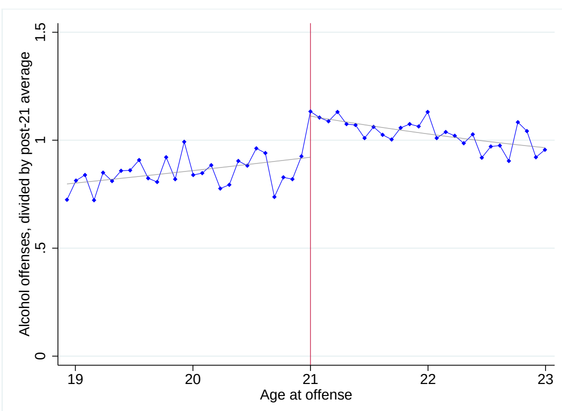
(a) Any alcohol arrest, Women



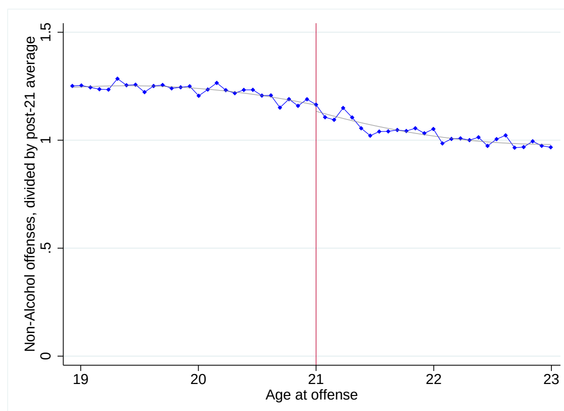
(b) All other arrests, Women



(c) Any alcohol arrest, Men



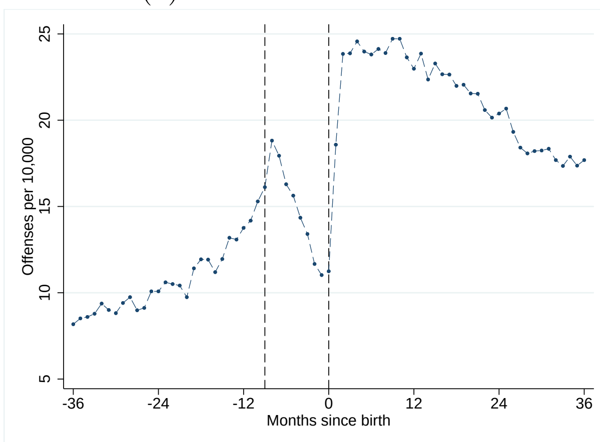
(d) All other arrests, Men



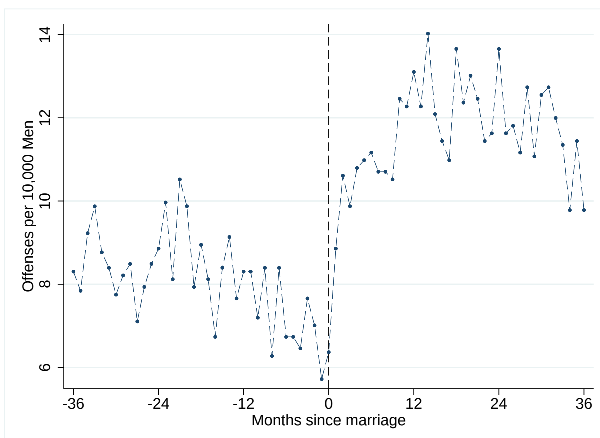
Includes fully-balanced arrest data on 422,910 men and 347,324 women with 21st birthdays within 3 years of the arrest data. Data points are scaled to give arrests relative to the post-21 average. The light grey lines show quadratic fits fully interacted with an indicator for being above 21.

Figure 1.9: Domestic violence

(a) Arrests around birth



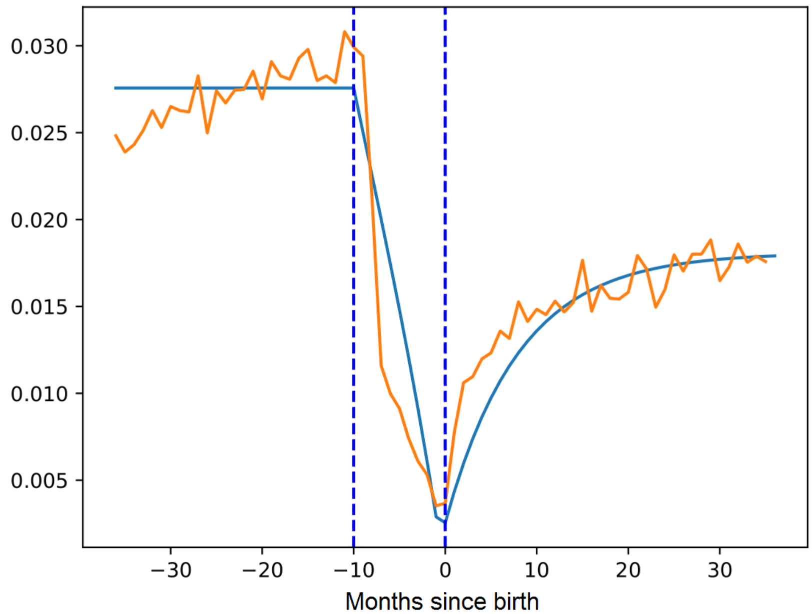
(b) Arrests around marriage



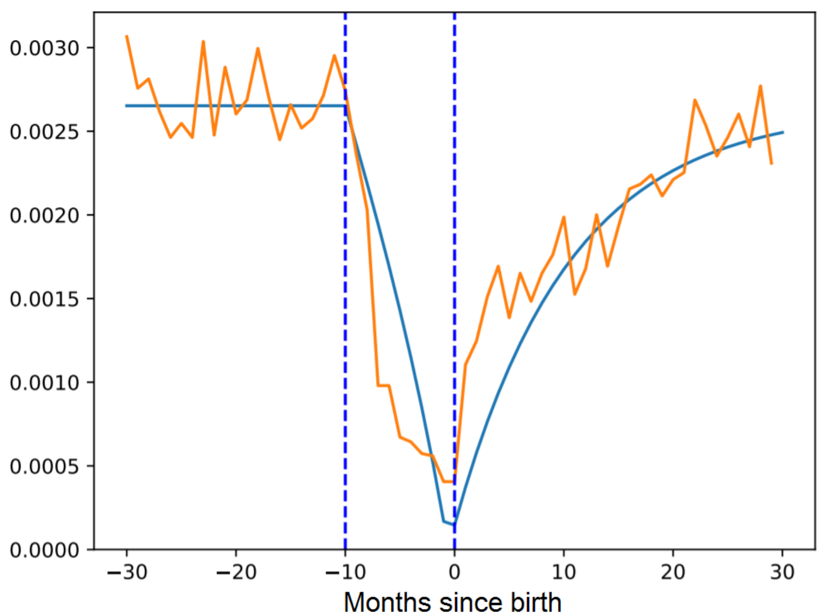
Panel (a) includes fully-balanced arrest data for 545,166 first-time fathers and the vertical dashed lines mark 9 months before the birth and the month of birth. Panel (b) includes fully-balanced arrest data for 245,756 married men and the vertical dashed line indicates the month of marriage.

Figure 1.10: Estimates from a dynamic model of addiction, mothers

(a) Unmarried mothers



(b) Married mothers



These plots show the parameter estimates from the model of habit formation described in Section 1.8 and estimated using minimum distance. The blue line gives the logit choice probabilities and the orange line gives the observed arrest rate.

1.11 Tables

Table 1.1: Descriptive statistics, Mother sample

Variable	(1) All births	(2) + Clear match	(3) +Mother's first	(4) Stillbirths
Mother age	27.91	28.50	27.55	28.04
	(6.01)	(5.91)	(6.05)	(6.66)
Father age	30.40	30.97	30.05	30.45
	(6.83)	(6.72)	(6.87)	(7.47)
Mother married at birth	0.73	0.81	0.77	0.67
	(0.44)	(0.39)	(0.42)	(0.47)
Mother on Medicaid	0.36	0.31	0.32	
	(0.48)	(0.46)	(0.47)	
WIC	0.34	0.30	0.31	0.23
	(0.47)	(0.46)	(0.46)	(0.42)
Twins+	0.02	0.02	0.02	0.05
	(0.12)	(0.13)	(0.13)	(0.22)
Male infant	0.51	0.51	0.51	0.52
	(0.50)	(0.50)	(0.50)	(0.50)
Low birth weight (<2500g)	0.05	0.05	0.06	0.60
	(0.22)	(0.21)	(0.23)	(0.49)
Any father arrest	0.41	0.35	0.34	0.31
	(0.49)	(0.48)	(0.47)	(0.46)
Any mother arrest	0.25	0.09	0.07	0.04
	(0.43)	(0.28)	(0.26)	(0.18)
Median zipcode income	59834.99	60739.80	60599.29	58650.58
	(18187.96)	(18542.80)	(18396.08)	(18073.86)
Midpregnancy marriage	0.03	0.03	0.04	0.05
	(0.18)	(0.18)	(0.21)	(0.21)
Divorce	0.22	0.21	0.21	0.36
	(0.42)	(0.41)	(0.41)	(0.48)
Father ever incarcerated	0.04	0.03	0.02	0.04
	(0.20)	(0.16)	(0.15)	(0.19)
Father ever on probation	0.09	0.06	0.05	0.07
	(0.28)	(0.23)	(0.22)	(0.25)
Observations	983,687	809,451	480,111	3,502

Standard deviations shown in parentheses. Insurance and ethnicity not recorded for stillbirths.

Table 1.2: Descriptives for married and unmarried parents

Variable	(1) Unmarried	(2) Married
Mother age	23.58	28.60
	(5.73)	(5.51)
Father age	25.93	30.78
	(6.57)	(6.10)
Mother on Medicaid	0.65	0.22
	(0.48)	(0.42)
WIC	0.61	0.23
	(0.49)	(0.42)
Twins+	0.01	0.02
	(0.11)	(0.13)
Male infant	0.51	0.51
	(0.50)	(0.50)
Father White	0.48	0.72
	(0.50)	(0.45)
Father Black	0.07	0.04
	(0.26)	(0.19)
Father Hispanic	0.19	0.10
	(0.39)	(0.30)
Father Asian	0.05	0.10
	(0.21)	(0.30)
Father other or missing	0.21	0.04
	(0.41)	(0.19)
Low birth weight (<2500g)	0.06	0.05
	(0.24)	(0.23)
Any father arrest	0.56	0.24
	(0.50)	(0.43)
Any mother arrest	0.46	0.14
	(0.50)	(0.35)
Median zipcode income	54753.86	62025.28
	(15006.51)	(18820.73)
Father ever incarcerated	0.07	0.01
	(0.26)	(0.10)
Father ever on probation	0.14	0.03
	(0.34)	(0.16)
Observations	160,052	385,114

Standard deviations shown in parentheses. The samples restrict to clean matches and father's first birth. Median zipcode income is for the years 2006-2010 from the American Community Survey via Michigan's Population Studies Center.

Table 1.3: Event study coefficients, All mothers

	Economic	Drugs	DUI	Destruction
36 months before birth	-0.133	-0.100	0.027	-0.409
	(0.089)	(0.128)	(0.224)	(0.271)
24 months before birth	-0.107	-0.148	0.095	0.031
	(0.087)	(0.125)	(0.220)	(0.297)
12 months before birth	0.021	-0.061	0.356	-0.041
	(0.090)	(0.128)	(0.233)	(0.290)
9 months before birth	0.060	-0.082	0.494	0.090
	(0.091)	(0.128)	(0.241)	(0.300)
6 months before birth	-0.384	-0.634	-0.760	-0.525
	(0.080)	(0.108)	(0.166)	(0.250)
3 months before birth	-0.575	-0.838	-0.918	-0.537
	(0.074)	(0.101)	(0.156)	(0.251)
Month of birth	-0.694	-0.945	-0.950	-0.736
	(0.071)	(0.097)	(0.156)	(0.235)
3 months after birth	-0.450	-0.739	-0.484	-0.471
	(0.080)	(0.107)	(0.192)	(0.262)
6 months after birth	-0.542	-0.699	-0.415	-0.533
	(0.078)	(0.110)	(0.199)	(0.261)
9 months after birth	-0.303	-0.650	-0.071	-0.502
	(0.086)	(0.113)	(0.222)	(0.267)
12 months after birth	-0.406	-0.575	-0.298	-0.332
	(0.085)	(0.118)	(0.213)	(0.286)
24 months after birth	-0.576	-0.720	0.221	-0.589
	(0.086)	(0.120)	(0.256)	(0.286)
36 months after birth	-0.611	-0.626	0.636	-0.900
	(0.094)	(0.133)	(0.294)	(0.289)

Selected point estimates shown for the event study specification given in Equation 1.1 controlling for a 4th-order polynomial in age and dummies for being over age 18 and 21, and using cluster-robust standard errors. The omitted period is ten months before birth. Coefficients are divided by the omitted period mean to give the proportional change since before the pregnancy.

Table 1.4: Event study coefficients, All fathers

	Economic	Drugs	DUI	Destruction
36 months before birth	0.084	-0.111	-0.037	-0.049
	(0.031)	(0.038)	(0.057)	(0.079)
24 months before birth	0.076	-0.049	0.085	-0.078
	(0.029)	(0.037)	(0.057)	(0.073)
12 months before birth	0.027	0.018	0.059	0.061
	(0.028)	(0.037)	(0.056)	(0.074)
9 months before birth	-0.007	-0.039	-0.056	-0.057
	(0.027)	(0.036)	(0.054)	(0.071)
6 months before birth	-0.015	-0.127	-0.139	-0.142
	(0.027)	(0.035)	(0.053)	(0.069)
3 months before birth	-0.070	-0.230	-0.139	-0.232
	(0.027)	(0.033)	(0.053)	(0.067)
Month of birth	-0.157	-0.229	-0.290	-0.287
	(0.026)	(0.033)	(0.051)	(0.066)
3 months after birth	-0.161	-0.194	-0.237	-0.088
	(0.026)	(0.034)	(0.052)	(0.071)
6 months after birth	-0.141	-0.176	-0.246	-0.115
	(0.026)	(0.034)	(0.053)	(0.071)
9 months after birth	-0.112	-0.186	-0.178	-0.080
	(0.027)	(0.034)	(0.054)	(0.073)
12 months after birth	-0.113	-0.206	-0.139	-0.131
	(0.027)	(0.034)	(0.055)	(0.072)
24 months after birth	-0.160	-0.208	-0.104	-0.152
	(0.029)	(0.036)	(0.059)	(0.076)
36 months after birth	-0.239	-0.192	-0.099	-0.243
	(0.031)	(0.039)	(0.063)	(0.080)

Selected point estimates shown for the event study specification given in Equation 1.1 controlling for a 4th-order polynomial in age and dummies for being over age 18 and 21, and using cluster-robust standard errors. The omitted period is ten months before birth. Coefficients are divided by the omitted period mean to give the proportional change since before the pregnancy.

Table 1.5: Descriptives of married and divorced couples

Variable	(1) Married	(2) Divorced	(3) Difference
Mother age	28.83	26.92	-1.91***
	(5.54)	(5.64)	(0.00)
Father age	31.22	29.48	-1.74***
	(6.43)	(6.66)	(0.00)
Mother married at birth	1.00	1.00	
	(0.00)	(0.00)	
Mother on Medicaid	0.24	0.26	0.02***
	(0.42)	(0.44)	(0.00)
WIC	0.24	0.29	0.05***
	(0.43)	(0.46)	(0.00)
Twins+	0.02	0.02	-0.00***
	(0.14)	(0.12)	(0.00)
Male infant	0.51	0.51	-0.00
	(0.50)	(0.50)	(0.91)
Low birth weight (<2500g)	0.06	0.05	-0.00***
	(0.23)	(0.23)	(0.00)
Any father arrest	0.27	0.53	0.26***
	(0.45)	(0.50)	(0.00)
Any mother arrest	0.13	0.32	0.19***
	(0.34)	(0.47)	(0.00)
Median Zipcode Income (2006-2010)	61839.96	59445.59	-2394.37***
	(18851.11)	(16933.97)	(0.00)
Midpregnancy marriage	0.06	0.15	0.09***
	(0.23)	(0.36)	(0.00)
Father ever incarcerated	0.01	0.04	0.03***
	(0.11)	(0.21)	(0.00)
Father ever on probation	0.03	0.10	0.07***
	(0.17)	(0.30)	(0.00)
Observations	405,387	43,115	448,502

Standard deviations shown in parentheses. *** indicates $p < .01$. The samples restrict to clean matches and father or mother's first birth. Median zipcode income is for the years 2006-2010 from the American Community Survey via Michigan's Population Studies Center.

Table 1.6: Regression discontinuity results

	Women		Men	
	(1)	(2)	(3)	(4)
	Alcohol	Non-Alcohol	Alcohol	Non-Alcohol
Over 21	5.630**	4.789	45.89***	-50.80
	(2.330)	(11.32)	(7.938)	(39.19)
Post-mean	17.84	263.19	187.76	2219.60
r-squared	0.070	0.019	0.107	0.455
N	422,910	422,910	347,324	347,324

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports the regression discontinuity estimate using daily arrest counts for all individuals in the DOH sample observable for three years before and after their 21st birthday. Birthday indicators are included as controls, as well as a quadratic in days since 21st birthday fully interacted with the indicator.

Table 1.7: Stillbirth results, men

	(1) Four main	(2) DV	(3) DUI	(4) Drug	(5) Econ	(6) Destruct
After birth	0.589	8.366***	-2.046*	0.976	1.373	0.0805
	(2.833)	(1.709)	(1.092)	(1.480)	(1.854)	(0.953)
Live X After	-5.955**	2.948*	-0.888	-2.785*	-2.755	0.318
	(2.816)	(1.683)	(1.079)	(1.466)	(1.843)	(0.948)
Age poly	yes	yes	yes	yes	yes	yes
FEs	yes	yes	yes	yes	yes	yes
Group size	6	6	6	6	6	6
Outcome Mean	38.079	9.511	9.882	14.517	12.429	3.509
r^2	0.205	0.158	0.102	0.158	0.179	0.105
N livebirths	545,166	545,166	545,166	545,166	545,166	545,166
N stillbirths	3,831	3,831	3,831	3,831	3,831	3,831

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports estimates from the difference-in-differences specification reported in Equation 1.2 and including person fixed effects, an indicator for pregnancy, a 4th order polynomial in age, and an indicator for after birth interacted with the livebirth indicator. Outcome is scaled to give monthly arrests per 10,000. DV stands for domestic violence; DUI stands for driving under the influence.

Table 1.8: Stillbirth results, women

	(1) Four main	(2) DV	(3) DUI	(4) Drug	(5) Econ	(6) Destruct
After birth	-1.456*	-0.0368	-0.810***	-0.106	-0.750	0.162
	(0.771)	(0.401)	(0.266)	(0.348)	(0.585)	(0.108)
Live X After birth	-1.823**	0.122	-0.173	-1.129***	-0.374	-0.273***
	(0.755)	(0.391)	(0.259)	(0.340)	(0.572)	(0.103)
Age poly	yes	yes	yes	yes	yes	yes
FEs	yes	yes	yes	yes	yes	yes
Group size	6	6	6	6	6	6
Outcome Mean	6.579	1.144	0.975	2.213	3.236	0.407
r^2	0.162	0.114	0.091	0.128	0.145	0.090
N livebirths	480,111	480,111	480,111	480,111	480,111	480,111
N stillbirths	3,502	3,502	3,502	3,502	3,502	3,502

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports estimates from the difference-in-differences specification reported in Equation 1.2 and including person fixed effects, an indicator for pregnancy, a 4th order polynomial in age, and an indicator for after birth interacted with the livebirth indicator. Outcome is scaled to give monthly arrests per 10,000. DV stands for domestic violence; DUI stands for driving under the influence.

Table 1.9: Habit formation model, mothers

Parameter	Unmarried	Married
Change in utility of using during pregnancy ($\Delta_1 f$)	1.310	1.777
	(0.480)	(2.041)
Permanent change in utility of using after pregnancy ($\Delta_2 f$)	0.026	0.0004
	(0.006)	(0.0007)
Habit formation (σ)	8.202	10.643
	(0.344)	(0.156)
Utility of using (f)	4.832	8.460
	(0.652)	(2.319)
Monthly discount factor (δ)	0.993	0.974
	(0.062)	(0.134)

Standard errors shown in parentheses. This table reports the parameter estimates from a model of habit formation matched to the observed arrest rates in the data using a minimum distance estimator.

Transitory remarks

The previous chapter outlines a situation where one of the arguably most relevant models—Becker's model of habit formation—does a fairly good job of matching the results. The habit formation framework makes the simple assumption that the utility received from some action depends on the stock of addiction, or past performance of the same action. This simple insight makes predictions about how agents should respond when something in their environment changes sharply, such as a price increase in cigarettes or, in my case, the initiation of a pregnancy.

In the next chapter, I pursue a question where economic models are found to have some room for refinement. A straightforward notion for labor economists is the reservation wage, which is ostensibly a minimal requirement for any job search model unless agents are presumed to be accepting of any job offer. I use new data that asks unemployment insurance claimants for their exact reservation wage, which provides a unique opportunity to study the implications of the model. However, I find no evidence that people increase their reservation wage in response to increased benefits.

Models can help reduce a setting to its key driving forces, allowing us to better make predictions and understand the underlying causes of phenomena. The reservation wage concept in job search models may still be useful if, for instance, patterns of aggregate job finding are uniquely explained by the predictions of a model wherein reservation wages are important forces. However, my results do raise the possibility that other formulations are worth exploring so that the model might enjoy accurate predictions at multiple levels of analysis.

Chapter 2

Job Search and Unemployment Insurance: New Evidence from One Million audits

2.1 Introduction

Understanding the behavioral response to unemployment insurance (UI) is crucial for determining the optimal level of benefits (Chetty, 2008). However, due to data limitations, the key quasi-experimental evidence on this question in the United States is restricted to data from more than three decades ago (Landais, 2015). Moreover, studies quantifying behavioral responses to benefits are typically forced to focus exclusively on time spent unemployed as the outcome, with no additional information on the behavior of claimants (Card et al., 2015a; Johnston & Mas, 2018; Rothstein, 2011).

This paper analyzes close to a million audits performed by the Department of Labor as part of its Benefit Accuracy Management (BAM) program from 1987 to the present. BAM audits are meant to determine whether randomly sampled benefit payments are valid according to state eligibility criteria. Importantly, claimants are questioned on their reservation wage, the number of jobs applied to that week, and the occupation they are seeking. Further, the data include administrative information on the claimant's exact monetary benefit amount and inputs into their benefit formula. These underutilized data present a unique opportunity to study search behaviors, the duration response to UI generosity, and the relationship between the two.

Claimants are selected based on a random draw from those who received benefits each week, with each state required to select a fixed number that roughly scales with population. The interviews are primarily performed in person or over the phone, and have a response rate above 90 percent (Potter et al., 2014). BAM investigators contact employers and state employment offices in order to verify survey responses, and claimants face potential monetary penalties if their answers are found to be inaccurate.

I first use this data to measure the duration response to monetary UI benefits. I exploit features of each state's benefit schedule in a regression kink framework, combining the BAM data with newly digitized panels of benefit formulas across all states from 1987 to the present. This extends existing quasi-experimental evidence in the US, which has been limited to the five states included in the Continuous Wage and Benefit History Project (Card et al., 2015a; Landais, 2015; Meyer, 1990).

I find a positive elasticity of duration at audit (i.e., interrupted duration) to the UI monetary benefit amount, with an increase of roughly 4-9 percent for every 10 percent increase in benefit levels. When I impute expected completed duration using the distribution of interrupted spell lengths, the response shrinks to 1-3 percent. This latter finding is similar to although broadly lower than other estimates in the United States; Landais (2015) finds an elasticity between .2 and .7 and Card et al. (2015a), using administrative data from Missouri, find an elasticity between .4 and .9.

I next turn to the unique feature of the data: measures of search effort and reservation wage. While the responses are not a panel as in Krueger and Mueller (2010), the size of the sample affords a similar causal exercise using the regression kink design. I find no evidence that reservation wages, job contacts, or occupation choice respond to benefits. The null results persist using both claimants in the first week and the entire sample, where estimates could be affected by length-biased sampling. The results suggest that, although greater benefits prolong unemployment spells, this may not be due to any of the measured search behaviors.

These findings present some of the first quasi-experimental evidence in the US evaluating the effects of UI benefits on search behavior and the reservation wage. The most comparable US papers study search behaviors using aggregated online search data. Marinescu (2017), using state-level variation in potential benefit duration from the Great Recession extensions and data from CareerBuilder.com, finds that a one percent increase in potential benefit duration decreases job applications by 0.4 percent, but Baker and Fradkin (2017), using a similar methodology with Google search data, find effects closer to zero. The most similar work uses administrative data from France. Le Barbanchon, Rathelot, and Roulet (2017) find a precise zero effect of unemployment benefits on the reservation wage, concluding that the duration response to potential benefits must be due to search effort.[1]

While search behaviors in the present context do not respond to benefits, they exhibit the predicted associations across several validation exercises. I match the audits to Missouri claim and wage records, which allows for a comparison between reservation wages and eventual reemployment earnings. This important validation exercise is not possible in Le Barbanchon, Rathelot, and Roulet (2017), and just one other US study has connected reported reservation wages and accepted wages, using only survey data (Krueger & Mueller, 2016). I find that reservation wages in BAM are a meaningful proxy of eventual earnings, with a significant elasticity of .4 even controlling for past wages.

Next, I show how search behavior evolves with unemployment duration. Claimants later in their spells have lower reservation wages, increased job contacts, and increased probability of switching occupations. Only the reservation wage response survives the addition of controls and person fixed effects for a limited sample of twice-audited claimants, suggesting that differences in job contacts and occupational choice are due to dynamic selection over the spell.

Finally, I find that the search behaviors exhibit cyclical qualities: across several specifications with flexible controls, and including all or only first week claimants to account for length-biased sampling, claimants lower their reservation wages by 0.5 percent for every 1 percent increase in the state unemployment rate. Interestingly, this reservation wage response outstrips the new hire wage elasticity to the state unemployment rate found in recent work (Gertler, Huckfeldt, & Trigari, 2016). In contrast, the decision to switch occupations or make more job contacts appear unrelated to local labor market conditions.

Taken together, these findings challenge the view that UI duration depends on the measurable search activities undertaken by claimants. While UI increases unemployment duration, the behavioral measures provided in the data—the reservation wage, job choice, and number of job contacts—are unlikely to drive the response.

[1] Smaller survey-based studies have similarly had mixed results. Feldstein and Poterba (1984) find a positive association between the reservation wage ratio and the benefit replacement ratio in a relatively small sample unemployed workers, but Krueger and Mueller (2016) find no association using a longitudinal survey of UI claimants in New Jersey. Using the same survey, Krueger et al. (2011) find no effect of UI benefits on job search activity, but, using the American Time Use survey and cross-state variation in UI schedules, Krueger and Mueller (2010) find a strong negative response of search effort to unemployment benefits.

2.2 Data

The data covers 921,801 paid claims audits from 1987 to 2019 from the Department of Labor's Benefit Accuracy Management (BAM) program (USDOL, 2018). The BAM program seeks to measure the accuracy of paid and denied claims.[2] Interviews are mostly conducted in-person or over the phone. Across all audits, 25 percent uncover an erroneous payment, which half of the time results in a change to their benefit amount. Top reasons for overpayment concern work search, benefit calculations, separation issues, and availability for work.

Audits are based on random samples of benefit payments: Each week, claimants are randomly chosen from a list all claimants with positive benefit amounts in that state. The target number of audits scales loosely with state population, ranging from 6 per week in Delaware to 15 per week in California.[3] Importantly, claimants are not followed after the audit, so it is not possible from BAM data alone to know their ultimate unemployment duration or employment outcome.

The survey includes questions about job search behavior. The text of the reservation wage question is: "What is the lowest rate of pay you will accept for a job?" Claimants can give any time period in their response ($X per Y). It is then converted to an hourly wage by the state (the original response is not available). In Figure B.1, I plot the distribution of the reservation wage ratio, the reservation wage over the claimant's previous hourly wage. Following Krueger and Mueller (2016), I drop respondents with ratios below .3 or above 3. Compared to Krueger and Mueller and Le Barbanchon, Rathelot, and Roulet (2017), the reservation wage ratio in this context is less dispersed and much less likely to exceed one.

Contacts are recorded in a worksheet with spaces for the employer name and address, the contact date and method of contact, the type of work applied for, and whether a job was offered; some states also accept electronic proof of applications. The distribution of the contacts variable is shown in Figure B.2, split by whether the claimant was required to search for a job. The variation in this figure assuages one concern about the contacts measure—that claimants exclusively report the number of job contacts required to maintain eligibility.[4]

Finally, claimants are asked to report which occupation they had previously and which occupation they are now seeking, recorded using 3-digit O*NET codes. I use these two variables together to create the switching occupations variable, equal to one

[2]The most recent BAM Annual report is available here.

[3]See the BAM operations guide (US Dept of Labor, 2009) for more details.

[4]In another check on this issue, I find that more than 20 percent of claimants report more contacts that the state-year mode for claimants required to search, which I presume to be the statutory requirement.

if the two do not match. Means for these search variables, benefit parameters, and other recorded demographics are reported in the descriptive statistics in Table B.1.

An advantage of the audit context is that answers to these questions may be investigated by the examiner. The state UI office investigates almost all contacts reported by the claimant, and some auditees are cited for refusal of acceptable work, albeit rarely.

Matched Missouri sample

In order to partially validate the reservation wage measure, I also match BAM claimants from Missouri to UI claims data and quarterly wages from Missouri using unique combinations of highest quarter earnings, base period earnings, and week of claim.[5] This matched sample is useful because it allows me to observe the total number of weeks claimed and eventual reemployment wages for claimants. As evidence that the match is successful, I find that claimants who reported that they were expecting to be recalled in the BAM data have significantly shorter durations in the Missouri claims data (Appendix Table B.5)

Duration measures

The data provide two measures of duration: weeks claimed and unemployment duration, both measured at audit. The unemployment duration variable measures the days between the audit and the date the claim was started. Duration is different from weeks claimed because of waiting periods and lapses in the submitting weekly claims, which could be due to part-time employment or temporary non-participation.[6]

These duration outcomes taken at the audit are lengths of interrupted spells, but are arguably most useful as proxies for eventual completed duration or spell. Since a claimant is equally likely to be sampled at any week in their spell, audited claimants should on average be halfway through their full completed duration at the time of the survey assuming the composition of active claimants does not change substantially across weeks (Salant, 1977).

Figure B.5 shows a histogram of the ratio of weeks claimed at audit to total weeks claimed for each of the matched Missouri claimants. The plot suggests that,

[5]This analysis was facilitated by Andrew Johnston.

[6]For more discussion of duration outcomes, see Landais (2015). In addition to the outcomes employed here, Landais uses two other measures: time to employment and initial spell duration, defined as time between filing and a two-week gap in claiming, which cannot be inferred from the present data.

as expected, claimants are equally likely to be selected at any point in their claim.[7] However, the relationship between completed and interrupted duration does not appear well approximated with a linear term and no constant. Figure B.3(a) plots the completed duration against the interrupted duration for the same sample of Missouri claimants. The slope of the trend line is 0.57, with a constant of 13.

This means that elasticites based on the duration at audit may not be equivalent to completed duration elasticities. I use a non-parametric method as in Sider (1985) for calculating the expected completed durations from the interrupted durations using the audit data. Assume some distribution of the interrupted spell lengths (in weeks), $F(w)$, and a survival function $S(w) = 1 - F(w)$. Then the expected duration D_e conditional on observing a claimant with an interrupted spell T equal to w is

$$E[D_e|T = w] = \frac{1}{S(w)} \int_{w}^{w_{max}} f(t)tdt. \tag{2.1}$$

where w_{max} is the maximum number of weeks available, usually 26. This is simply a weighted average taken across the weeks greater than or equal to w.

I validate this method using the Missouri sample. Figure B.3(b) shows that the actual completed duration lines up closely with the predicted duration based on Equation 2.1 and using the whole sample, although not perfectly. The slope of the trend line is 1.09. In the duration results below, I present effects on weeks claimed and duration at audit, as well as the expected weeks measure. This method requires a stationary assumption because $f(t)$ is assumed to be stable; in practice, restricting to smaller time periods did not affect the fit between expected and actual duration in the Missouri sample.

Inferring completed duration from interrupted duration has been a longstanding challenge in studies of unemployment (Baker & Trivedi, 1985; Heckman & Singer, 1984; Marston et al., 1975; Salant, 1977). This technique may have efficiency losses compared to a maximum likelihood approach, but it has the advantage of not imposing parametric assumptions on the distribution of completed durations, as previous research has found that parameter estimates are sensitive to the specified distribution (Kiefer, 1988; Kiefer, Lundberg, & Neumann, 1985).

[7] A more efficient way to measure the duration response could be to explicitly model the probability of selection for audit. This is complicated by the facts that many claims are interrupted and the DOL does not keep records of which claimants were at risk of being selected each week.

2.3 Regression kink evidence on the effect of UI benefits on unemployment duration

I first assess the impact of unemployment insurance benefits on unemployment duration and search behavior. In most states, the benefit amount is a fixed fraction of the claimant's highest quarterly wages from the past year, up to a maximum weekly amount. Thus a natural way to investigate the causal effects of benefits is with a regression kink design (RKD), which tests for a significant change in the slope of an outcome variable at the point at which the treatment variable sharply changes slope (Card et al., 2015b; Landais, 2015).

This analysis cannot use all claimants in the audit data because states vary in whether they employ a formula based on highest quarter earnings (which is included in BAM) or some other function based on multiple quarters of earnings (which are not). For the states using only highest quarter earnings, I hand-coded each benefit schedule in order to identify where benefits hit their maximum as a function of previous earnings.[8] These schedules were collected from the Department of Labor and are updated every six months. In several cases I manually inputted more exact start dates for changes in the benefit schedules based on a visual inspection of the first stage figure.

To restrict to states and periods where benefits closely track the reported schedule, I drop schedules where a simple regression of benefits on highest quarter earnings interacted with a post-kink indicator yields an r-squared lower than 0.50.[9] Finally, as in Landais (2015), I drop claimants who have below the maximum number of weeks of eligibility, as this kink in duration confounds the kink in benefits. In Figure B.8, I show the sample sizes for each state employed in the RKD analysis.

Regression kink results

Figure 2.1 shows the first stage figure combining all claimants in the RKD sample. The figure shows that, after the sample restrictions, the benefits in the analysis sample closely match the benefit schedule. Figure 2.2 shows the reduced-form pictures, plotting the three duration outcomes against highest quarter earnings, centered on the kink point. The plots show evidence of a discontinuous change in the slope at the kink point, indicated by the red line. The two grey lines show linear fits of the data on either side of the kink point.

[8] I digitized PDFs of state UI laws from 1978 to the present, collected from the Department of Labor's website. They are available at this http://maximmassenkoff.com/data.html.

[9] All analyses are robust to the choice of r-squared threshold.

Since the first stage in Figure 2.1 shows that not all claimants get the exact benefit amount predicted by their schedule, I employ a fuzzy RKD as in Card et al. (2015b), which can be estimated using 2SLS and a uniform kernel.[10] The estimating equations are given below:

$$b_{ic} = \gamma_c + \gamma_0 * D_{ic} + \sum_{p=1}^{\overline{p}} (\sigma_p HQE_i^p + \eta_p HQE_i^p D_{ic}) + e_{ic} \tag{2.2}$$

$$y_{ic} = \alpha_c + \alpha_0 * D_{ic} + \tau \hat{b_{ic}} + \sum_{p=1}^{\overline{p}} (\delta_p HQE_i^p) + \epsilon_{ic} \tag{2.3}$$

In this setup, c denotes a benefit schedule cell, i.e., a unique combination of the state, replacement rate, and benefit maximum. Next, b_{ic} gives claimant i's weekly benefit amount, D_{ic} is an indicator for being above the kink point according to highest quarter earnings, HQE_i is claimant i's highest quarter earnings centered so that the kink point is at $HQE_i = 0$. γ_c and α_c denote cell-specific fixed effects and τ measures the causal effect of monetary benefits on the outcome y_{ic}. Standard errors are clustered at the cell level, and $\overline{p}$ denotes polynomial order. For $\overline{p} > 1$, the interaction terms $\sum_{p=2}^{\overline{p}} (\beta_p HQE_i^p D_{ic})$ are added to the second stage equation.

I use the bias-corrected estimator described in Calonico, Cattaneo, and Titiunik (2014) in order to calculate optimal bandwidths. In the inset boxes on each plot, and in columns (1) and (2) of Table B.2, I report the implied elasticities from linear ($\overline{p} = 1$) and quadratic ($\overline{p} = 2$) specifications. In Table B.2, I also show estimates for the linear specification with double the optimal bandwidth (column (3)) and $\overline{p} = 3$ (column (4)). In Figure B.4, I probe the importance of bandwidth selection by plotting the 2SLS estimates for the linear and quadratic specifications for a variety of bandwidths. While the estimates are noisy for below the optimal linear bandwidth, they are otherwise fairly stable around the reported point estimates.

The estimates suggest an elasticity between .2 and .9. This implies that for every 10 percent increase in weekly benefit levels, weeks claimed at audit increases by 2 to 9 percent. The estimates for the expected duration measure are smaller, ranging from .1 to .3. Since the latter outcome is my best proxy for completed duration, these elasticities are on the lower end of estimates using similar methodologies. Landais (2015) found elasticities between .2 and .7 using data from the early 1980s covering claimants in Idaho, Louisiana, Missouri, New Mexico, and Washington. Card et al. (2015a), using more recent data from Missouri, found an elasticity between .35 and .9.

[10] In practice, the estimates are quite similar between sharp and fuzzy.

A valid RKD estimate requires that the density and covariates evolve smoothly over the kink point in highest quarter earnings. Unlike Landais (2015) and Card et al. (2015a), these tests cannot be performed on the full data due to the nature of the sample: Claimants with longer spells are more likely to get audited. If any groups are more likely to respond to benefits, the audit sample in the present data will display a kink in covariates. For instance, if only men respond to benefit levels, the share of men in the sample will change discontinuously above the kink point as men decrease in their relative probability of being audited compared to women.

I can perform similar tests of the assumptions restricting to claimants audited in the first weeks of their spells, since these claimants are uncontaminated by the dynamic selection issue. Restricting to this sample I plot a histogram of highest quarter earnings around the kink point in Figure B.6, which shows that the density evolves smoothly around the kink point at 0. Next, as in Card et al. (2015a), I use all the pre-determined covariates in the sample to predict the two main outcomes, log weeks claimed and log duration, using ordinary least squares. I plot these against the centered highest quarter earnings measure in Figure B.7. The fitted values evolve smoothly over the kink point, suggesting that there is no discontinuous change in the derivative of covariates.

Effects on search

In Figure 2.3, I show similar reduced-form plots using the full sample for the four main search behaviors: the log reservation wage, the reservation ratio, and indicator for switching occupations, and the number of contacts. In each case, the plots suggest no obvious effects of benefits on these search outcomes. The reservation ratio gives a visual indication of a kink, but it is in the opposite direction than would be expected, suggesting that benefits decrease the reservation ratio.

As with the tests of the RKD assumptions above, these could be confounded with duration when estimated on the whole sample. For instance, it could be that people with low reservation wages are most affected by benefit levels. As highest quarter earnings increase and claimants reach the maximum level of benefits, those with low reservation wages will constitute relatively less of the sample, resulting in a spurious kink in the reservation wage. In theory, it should be feasible to counteract these effects using weights. However, due to limitations in the BAM data, the claimant's probability of having been selected is not recoverable. Thus, in the analysis below, I present estimates using the potentially contaminated full sample, and only claimants in their first week. The results are the same across the two samples.

The coefficient estimates from the same regressions for the four main search measures using only claimants in their first week are given in Table B.4. In each case, the search measures show no sign of a kink where benefit levels reach their maximum. The results for the reservation wage outcomes, for instance, flip between positive and negative and significantly detect a kink in the linear specifications, where the effect suggests that the reservation ratio *decreases* with benefits.

These findings contrast with Feldstein and Poterba (1984), who find that reservation wages increase as much as 4 percent for every 10 percent increase in benefits, but corroborate a precise zero effect found in Le Barbanchon, Rathelot, and Roulet (2017). In the present context, the implied confidence intervals encompass potentially meaningful responses. For instance, the linear RKD for the reserve ratio cannot reject an elasticity as large as .08. For comparison, Nekoei and Weber (2017) find a 0.5 percent increase in wages in response to a substantial 30 percent increase in potential benefit duration. This would imply a small elasticity that is certainly consistent with these findings.

2.4 Validation of search measures

The previous results suggest that claimants spend more time unemployed when monetary benefits are more generous. However, it finds no effects of unemployment benefits on the recorded search behaviors. While this is consistent with findings on the reservation wage in Le Barbanchon, Rathelot, and Roulet (2017), this section presents some validation exercises to show that the measures do vary in the expected ways with other economic factors.

Relationship between reservation wages and reemployment wages

First, I test whether the reported reservation wages in the BAM data are related to the ultimate reemployment wages of the claimants. This is possible using the matched Missouri sample described in Section 2.2. I regress log reemployment earnings—i.e., the log of the claimant's first positive quarterly earnings following the claim, derived from the Missouri wage data—on the log reservation wage, with fixed effects for the month the claim was started. The results are in Table 2.1, where across the columns (1)-(4) I incrementally include person-level covariates. Importantly, the log of the previous hourly wage enters beginning in column (2).

In each specification, the coefficient on the reservation wage is positive with $p < .01$. The coefficient drops by roughly half when I control for the log of the previous hourly wage and drops slightly more as I add controls for demographics and industry, indicating a .4 percent increase in reemployment earnings for every 1 percent increase in the reservation wage in the most restrictive specification. This echoes the findings in Krueger and Mueller (2016) and DellaVigna and Paserman (2005), where reservation wages are predictive of accepted job offers and reemployment earnings. However, it presents the first evidence to my knowledge connecting administrative wage records to survey responses on the reservation wage.

These results suggest that answers to the reservation wage question in the BAM data are not meaningless, containing information about the claimant's eventual employment. Despite the correlation, a comparison of the r-squared in columns (4) and (5), which removes the reservation wage from the most controlled specification, demonstrates that not much information is added once accounting for occupation, previous wage, and the other controls.

Duration dependence

Next I study how the reported search behaviors vary with weeks claimed. The results in Table 2.2 show regressions with the different search behaviors on in the columns and, along with state-by-year fixed effects, three sets of regressors across rows: weeks claimed, weeks claimed with controls, and weeks claimed with person fixed effects for a small subsample of people who were audited twice in the same claim.[11]

The results in the top row show how search behaviors associate with spell duration at audit. For ease of interpretation, the coefficients are scaled to represent the change in the outcome for every additional 10 weeks of UI duration. By four out of the five measures, search intensity is higher as the spell progresses: after 10 weeks on UI, the reservation wage is lower by 1-2 percent. Claimants also report slightly more contacts and are 2 percentage points more likely to be looking for a different occupation.

Except for the reservation wage regressions, every coefficient gets smaller with the addition of detailed controls on the claimant including past earnings, education, and demographics. In particular, the confidence interval in the number of work contacts specification rejects very small responses. This contrasts with Skandalis and Marinescu (2019), who, using online search job application data, document a spike in applications close to benefit exhaustion.

The one result that remains fairly consistent across spacifications is that the reservation wage declines moderately with duration. The best other source for this relationship comes from the longitudinal survey in Krueger and Mueller (2016), where the estimates are quite similar: they find that reservation wages decline 0.5 to 1.4 percent for every ten weeks of unemployment, compared to my estimates of 1-2

[11]These qualitative findings do not change when using share of eligible weeks claimed as the measure of duration.

percent. This consonance is interesting in part because of the differing samples: Krueger and Mueller followed all claimants from their first week, while the audit data oversamples those with longer durations.

Cyclicality

Finally, I study how the search measures vary with the state unemployment rate. Little is known about how job search behaviors vary over the business cycle, although the cyclicality of continuing and new hire wages have long been a focus in macroeconomics (Bils, 1985; Gertler, Huckfeldt, & Trigari, 2016; Keynes, 1936). The coverage of the data provides a unique opportunity to study how job search behaviors vary with local labor market conditions.

Table B.6 shows how the different search measures vary with the state unemployment rate. Each point estimate reports a β from the equation below estimated using OLS:

$$y_{its} = \alpha + \beta u_{st} + \delta_t + \Omega_s + \epsilon_{its} \tag{2.4}$$

where y_{its} is the reservation ratio, log reservation wage, switching jobs indicator, or number of job contacts for claimant i in month-year t and state s, u_{st} is the unemployment rate in percent in that month-year, δ_t indicates month-year fixed effects, and Ω_s indicates state fixed effects. Standard errors are clustered at the state level.

As discussed in Section 2.3, the sample selection inherent to the audit process could confound these estimates. Separate from this issue, any results on cyclicality will capture a mix of selection into unemployment and actual changes in search behaviors caused by the business cycle (Krueger et al., 2011). I add increasingly stringent controls across columns to partially address these potential sources of bias: Column (1) uses the full sample; column (2) adds fixed effects for weeks claimed as controls. Column (3) uses only first week claimants and column (4) adds the full set of claimant-level controls including age, education, ethnicity.

The results show a stable and strong relationship between the reservation wage measures and the state unemployment rate. The first point estimate in column (1) implies that the reservation ratio decreases by half a percent for every one percent increase in the state unemployment rate. Columns (2) and (3) suggest that this result is not simply due to claimants being further along in their spells on average when unemployment is higher, and column (4), which adds tight controls including fixed effects for state-industry-occupation, suggests that selection into unemployment is also not driving the finding.

The results show no correlation between job contacts and the unemployment rate. This contrasts with Mukoyama, Patterson, and Şahin (2018), which finds a positive association between time spent searching for a job and the state unemployment rate, but the confidence intervals do not rule out the slight (but significant) effects found in their analyses.

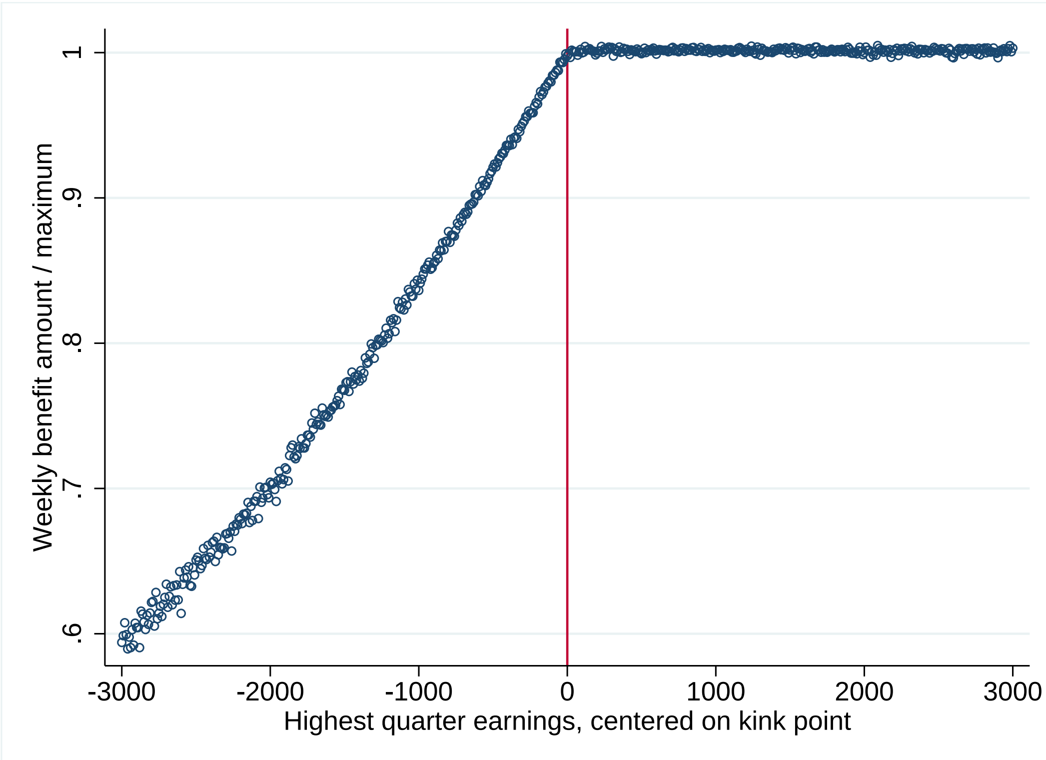
2.5 Discussion

The preceding sections find clear evidence using large-scale data and a quasi-experimental design that unemployment durations increase in the monetary level of benefits, extending existing work from Landais (2015) and Card et al. (2015a), which are based on more limited samples. The unique data allows me the probe the mechanism further by estimating the effects of benefits on search behaviors. None of the search behaviors appear to respond to benefits. This is despite the fact that the reservation wage measures appear reliable across several validation exercises comparing them to the reemployment wage, unemployment duration, and local unemployment rate.

One potential explanation is that claimants wield storable job offers that they can time to begin when their benefits elapse (Boone & van Ours, 2012). However, evidence from existing studies argues that this mechanism is not core to job search patterns. DellaVigna et al. (2020) find that job seekers do not time their start dates to coincide with UI exhaustion. Further, although more indirectly, several studies (e.g. Card, Chetty, & Weber, 2007) find that recalls, which might have more negotiable start dates given an existing relationship with the employer, do not show a markedly higher exit spike at exhaustion.

This evidence builds on Le Barbanchon, Rathelot, and Roulet, 2017, who found a precise zero effect of potential benefit duration on reservation wages using administrative data from France, but extends the results to explicitly study search intensity. These findings deepens the mystery around what exactly claimants do differently that causes them to stay unemployed longer under more generous benefit regimes.

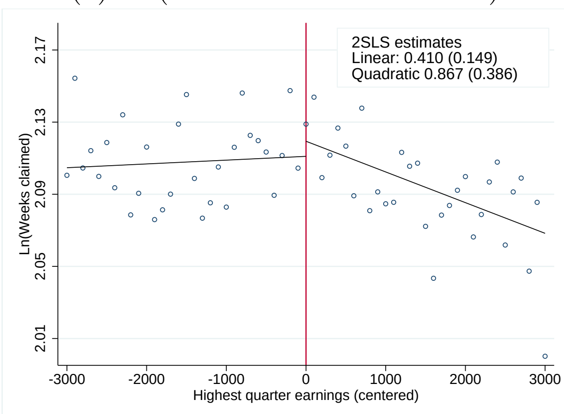
Figure 2.1: Regression kink design first stage



Notes: This plot shows the first stage relationship for all claimants in the RKD sample within \$3,000 of their benefit schedule kink (N=132,337). The y-axis gives claimant weekly benefit payments divided by the maximum amount in their benefit schedule. The x-axis gives highest quarter earnings minus the point in their schedule where the benefit amount reaches its maximum. Each dot represents an average using \$10 bins of highest quarter earnings.

Figure 2.2: RKDs showing effect of UI benefits on duration

(a) Ln(Weeks claimed at audit)



(b) Ln(unemployment duration) at audit

(c) Expected Ln(weeks claimed) at audit

Note: These are the reduced form plots from the regression kink design for the three duration outcomes. The textbox in each plot reports the 2SLS results, interpretable as elasticities, from the regression specification outlined in Section 2.3. Each dot represents a binned average, with a binsize of $50.

Figure 2.3: RKDs showing no effect of UI benefits on search behaviors

Note: These are the reduced form plots from the regression kink design for the job search outcomes. The textbox in each plot reports the 2SLS results, interpretable as elasticities, from the regression specification outlined in Section 2.3. Each dot represents a binned average, with a binsize of $50.

Table 2.1: Reemployment wage vs. reservation wage in the matched Missouri sample

	(1)	(2)	(3)	(4)	(5)
Log Reservation Wage	0.961	0.598	0.454	0.426	
	(0.0503)	(0.103)	(0.104)	(0.111)	
Log Prev Wage		0.405	0.0367	0.0401	0.342
		(0.102)	(0.117)	(0.128)	(0.0987)
Expecting recall			0.131	0.138	0.146
			(0.0557)	(0.0567)	(0.0571)
Month FEs	Yes	Yes	Yes	Yes	Yes
Weekly benefits	No	No	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	Yes
Industry	No	No	Yes	Yes	Yes
Last Occ	No	No	No	Yes	Yes
R-squared	0.269	0.278	0.335	0.386	0.379
N	1,650	1,650	1,650	1,650	1,650

Standard errors in parentheses

Table 2.2: Duration dependence results

	(1) Res ratio	(2) Ln(Res wage)	(3) Contacts	(4) Occ switch
No controls				
Weeks claimed	-0.0175	-0.0181	0.0244	0.0218
	(0.000266)	(0.000658)	(0.00240)	(0.000595)
R-squared	0.071	0.334	0.341	0.070
N	846,714	856,770	612,422	867,925
Full controls				
Weeks claimed	-0.0145	-0.0210	-0.0140	0.0134
	(0.000255)	(0.000351)	(0.00248)	(0.000623)
R-squared	0.231	0.845	0.404	0.119
N	774,019	776,972	543,293	777,274
Restricted sample with multiple audits				
Weeks claimed	-0.00759	-0.00991	-0.0229	0.00311
	(0.00343)	(0.00418)	(0.0411)	(0.00948)
R-squared	0.904	0.984	0.836	0.834
N	7,785	7,877	6,133	7,982

Standard errors in parentheses

Coefficients are multiplied by 10 for interpretability. The first two rows include state-by-month fixed effects. Full controls includes ethnicity, sex, dependents, a quadratic in age, education indicators, indicators for probable and definite recall, reason for layoff, employer tax rate, an indicator for work search required, weeks eligible, and the log of base period earnings and the previous wage.

Conclusion

In the preceding two chapters, I have provided evidence on important economic phenomena using tools from labor economics and behavioral economic theory. Both chapters wrestle with the problem of clearly establishing causality in ecological settings where there is ample reason to be suspicious of naive comparisons. In the context of the first chapter, a comparison of average crime levels between fathers and non-fathers; in the second chapter, a comparison of unemployment duration for claimants receiving high and low levels of unemployment benefits. I attempt to address these concerns using relatively recent methods in applied econometrics, and using other graphical and econometric arguments to support my conclusions.

Finally, both papers highlight the importance of measurement. The domestic violence result in the first chapter is a straightforward finding but to my knowledge has never been documented, presumably because of difficulties in merging the datasets required for such an analysis. The second chapter shows that surveying subjects on the content of economic models can provide surprising results and stimulate the development of new theory.

Bibliography

Akerlof, G. A. (1998). Men without children. *The Economic Journal*, *108*(447), 287–309.

Almond, D., & Rossin-Slater, M. (2013). Paternity acknowledgment in 2 million birth records from michigan. *PloS one*, *8*(7), e70042.

Antonovics, K., & Town, R. (2004). Are all the good men married? uncovering the sources of the marital wage premium. *American Economic Review*, *94*(2), 317–321.

Arcidiacono, P., Sieg, H., & Sloan, F. (2007). Living rationally under the volcano? an empirical analysis of heavy drinking and smoking. *International Economic Review*, *48*(1), 37–65.

Arora, A. (2019). Juvenile crime and anticipated punishment. *Available at SSRN 3095312*.

Ashcraft, A., Fernández-Val, I., & Lang, K. (2013). The consequences of teenage childbearing: Consistent estimates when abortion makes miscarriage non-random. *The Economic Journal*, *123*(571), 875–905.

Baker, G. M., & Trivedi, P. K. (1985). Estimation of unemployment duration from grouped data: A comparative study. *Journal of Labor Economics*, *3*(2), 153–174.

Baker, S. R., & Fradkin, A. (2017). The impact of unemployment insurance on job search: Evidence from google search data. *Review of Economics and Statistics*, *99*(5), 756–768.

Barnes, J., & Beaver, K. M. (2012). Marriage and desistance from crime: A consideration of gene–environment correlation. *Journal of Marriage and Family*, *74*(1), 19–33.

Barnes, J., Golden, K., Mancini, C., Boutwell, B. B., Beaver, K. M., & Diamond, B. (2014). Marriage and involvement in crime: A consideration of reciprocal effects in a nationally representative sample. *Justice Quarterly*, *31*(2), 229–256.

Beaver, K. M., Wright, J. P., DeLisi, M., & Vaughn, M. G. (2008). Desistance from delinquency: The marriage effect revisited and extended. *Social science research*, *37*(3), 736–752.

Becker, G. (1968). Crime and punishment: An economic approach. *Journal of Political Economy*, *75*(2), 169–217.

Becker, G. S., Grossman, M., & Murphy, K. M. (1991). Rational addiction and the effect of price on consumption. *The American Economic Review*, *81*(2), 237–241.

Becker, G. S., Landes, E. M., & Michael, R. T. (1977). An economic analysis of marital instability. *Journal of political Economy*, *85*(6), 1141–1187.

Becker, G. S., & Murphy, K. M. (1988). A theory of rational addiction. *Journal of political Economy*, *96*(4), 675–700.

Becker, G. S., Grossman, M., & Murphy, K. M. (1994). An empirical analysis of cigarette addiction. *The American Economic Review*, *84*(3), 396–418.

Beijers, J., Bijleveld, C., & van Poppel, F. (2012).
man's best possession': Period effects in the association between marriage and offending. *European Journal of Criminology*, *9*(4), 425–441.

Bersani, B. E., & Doherty, E. E. (2013). When the ties that bind unwind: Examining the enduring and situational processes of change behind the marriage effect. *Criminology*, *51*(2), 399–433.

Bersani, B. E., Laub, J. H., & Nieuwbeerta, P. (2009). Marriage and desistance from crime in the netherlands: Do gender and socio-historical context matter? *Journal of Quantitative Criminology*, *25*(1), 3–24.

Bils, M. J. (1985). Real wages over the business cycle: Evidence from panel data. *Journal of Political economy*, *93*(4), 666–689.

Bilukha, O., Hahn, R. A., Crosby, A., Fullilove, M. T., Liberman, A., Moscicki, E., Snyder, S., Tuma, F., Corso, P., Schofield, A., Et al. (2005). The effectiveness of early childhood home visitation in preventing violence: A systematic review. *American journal of preventive medicine*, *28*(2), 11–39.

Boone, J., & van Ours, J. C. (2012). Why is there a spike in the job finding rate at benefit exhaustion? *De Economist*, *160*(4), 413–438.

Branum, A. M., & Ahrens, K. A. (2017). Trends in timing of pregnancy awareness among us women. *Maternal and child health journal*, *21*(4), 715–726.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, *82*(6), 2295–2326.

Card, D., Chetty, R., & Weber, A. (2007). The spike at benefit exhaustion: Leaving the unemployment system or starting a new job? *American Economic Review*, *97*(2), 113–118.

Card, D., Johnston, A., Leung, P., Mas, A., & Pei, Z. (2015a). The effect of unemployment benefits on the duration of unemployment insurance receipt: New evidence from a regression kink design in missouri, 2003-2013. *American Economic Review*, *105*(5), 126–30.

Card, D., Lee, D. S., Pei, Z., & Weber, A. (2015b). Inference on causal effects in a generalized regression kink design. *Econometrica*, *83*(6), 2453–2483.

Carpenter, C., & Dobkin, C. (2015). The minimum legal drinking age and crime. *Review of economics and statistics*, *97*(2), 521–524.

Chalfin, A., & McCrary, J. (2017). Criminal deterrence: A review of the literature. *Journal of Economic Literature*, *55*(1), 5–48.

Charles, K. K., & Stephens, M., Jr. (2004). Job displacement, disability, and divorce. *Journal of Labor Economics*, *22*(2), 489–522.

Charles, P., & Perreira, K. M. (2007). Intimate partner violence during pregnancy and 1-year post-partum. *Journal of Family Violence*, *22*(7), 609–619.

Chetty, R. (2008). Moral hazard versus liquidity and optimal unemployment insurance. *Journal of political Economy*, *116*(2), 173–234.

Craig, J. M. (2015). The effects of marriage and parenthood on offending levels over time among juvenile offenders across race and ethnicity. *Journal of Crime and Justice*, *38*(2), 163–182.

Craig, J., & Foster, H. (2013). Desistance in the transition to adulthood: The roles of marriage, military, and gender. *Deviant Behavior*, *34*(3), 208–223.

Currie, J., Mueller-Smith, M., & Rossin-Slater, M. (2018). *Violence while in utero: The impact of assaults during pregnancy on birth outcomes* (tech. rep.). National Bureau of Economic Research.

DeKeseredy, W. S., Dragiewicz, M., & Schwartz, M. D. (2017). *Abusive endings: Separation and divorce violence against women* (Vol. 4). Univ of California Press.

DellaVigna, S., Heining, J., Schmieder, J. F., & Trenkle, S. (2020). Evidence on job search models from a survey of unemployed workers in germany. *working paper*.

DellaVigna, S., & Paserman, M. D. (2005). Job search and impatience. *Journal of Labor Economics*, *23*(3), 527–588.

Doherty, E. E., & Ensminger, M. E. (2013). Marriage and offending among a cohort of disadvantaged african americans. *Journal of Research in Crime and Delinquency*, *50*(1), 104–131.

Drago, F., Galbiati, R., & Vertova, P. (2009). The deterrent effects of prison: Evidence from a natural experiment. *Journal of political Economy*, *117*(2), 257–280.

Edin, K., & Kefalas, M. (2011). *Promises i can keep: Why poor women put motherhood before marriage*. Univ of California Press.

Edin, K., & Nelson, T. J. (2013). *Doing the best i can: Fatherhood in the inner city*. Univ of California Press.

Feldstein, M., & Poterba, J. (1984). Unemployment insurance and reservation wages. *Journal of public Economics*, *23*(1-2), 141–167.

Fletcher, J. M., & Wolfe, B. L. (2009). Education and labor market consequences of teenage childbearing evidence using the timing of pregnancy outcomes and community fixed effects. *Journal of Human Resources*, *44*(2), 303–325.

Forrest, W., & Hay, C. (2011). Life-course transitions, self-control and desistance from crime. *Criminology & Criminal Justice*, *11*(5), 487–513.

Gertler, M., Huckfeldt, C., & Trigari, A. (2016). *Unemployment fluctuations, match quality, and the wage cyclicality of new hires* (tech. rep.). National Bureau of Economic Research.

Giordano, P. C., Seffrin, P. M., Manning, W. D., & Longmore, M. A. (2011). Parenthood and crime: The role of wantedness, relationships with partners, and ses. *Journal of Criminal Justice*, *39*(5), 405–416.

Gottlieb, A., & Sugie, N. F. (2019). Marriage, cohabitation, and crime: Differentiating associations by partnership stage. *Justice Quarterly*, *36*(3), 503–531.

Graham, J., & Bowling, B. (1995). Young people and crime.

Gruber, J., & Köszegi, B. (2001). Is addiction "rational"? theory and evidence. *The Quarterly Journal of Economics*, *116*(4), 1261–1303.

Hansen, B. (2015). Punishment and deterrence: Evidence from drunk driving. *American Economic Review*, *105*(4), 1581–1617.

Heazell, A. E., Siassakos, D., Blencowe, H., Burden, C., Bhutta, Z. A., Cacciatore, J., Dang, N., Das, J., Flenady, V., Gold, K. J., Et al. (2016). Stillbirths: Economic and psychosocial consequences. *The Lancet*, *387*(10018), 604–616.

Heckman, J., & Singer, B. (1984). A method for minimizing the impact of distributional assumptions in econometric models for duration data. *Econometrica: Journal of the Econometric Society*, 271–320.

Helland, E., & Tabarrok, A. (2007). Does three strikes deter? a nonparametric estimation. *Journal of Human Resources*, *42*, 309–330.

Herrera, V. M., Wiersma, J. D., & Cleveland, H. H. (2011). Romantic partners' contribution to the continuity of male and female delinquent and violent behavior. *Journal of Research on Adolescence*, *21*(3), 608–618.

Heyman, G. M. (2009). *Addiction: A disorder of choice*. Harvard University Press.

Hoffman, S. D. (2008). Updated estimates of the consequences of teen childbearing for mothers. *Kids having kids: Economic costs and social consequences of teen pregnancy*, 74–118.

Hope, T. L., Wilder, E. I., & Watt, T. T. (2003). The relationships among adolescent pregnancy, pregnancy resolution, and juvenile delinquency. *The Sociological Quarterly*, *44*(4), 555–576.

Hotz, V. J., McElroy, S. W., & Sanders, S. G. (2005). Teenage childbearing and its life cycle consequences exploiting a natural experiment. *Journal of Human Resources*, *40*(3), 683–715.

Hotz, V. J., Mullin, C. H., & Sanders, S. G. (1997). Bounding causal effects using data from a contaminated natural experiment: Analysing the effects of teenage childbearing. *The Review of Economic Studies*, *64*(4), 575–603.

Jaffee, S. R., Lombardi, C. M., & Coley, R. L. (2013). Using complementary methods to test whether marriage limits men's antisocial behavior. *Development and Psychopathology*, *25*(1), 65–77.

Jatlaoui, T. C., Shah, J., Mandel, M. G., Krashin, J. W., Suchdev, D. B., Jamieson, D. J., & Pazol, K. (2018). Abortion surveillance—united states, 2014. *MMWR Surveillance Summaries*, *66*(25), 1.

Johnston, A. C., & Mas, A. (2018). Potential unemployment insurance duration and labor supply: The individual and market-level response to a benefit cut. *Journal of Political Economy*, *126*(6), 2480–2522.

Kearney, M. S., & Levine, P. B. (2012). Why is the teen birth rate in the united states so high and why does it matter? *Journal of Economic Perspectives*, *26*(2), 141–63.

Keiding, N., Kvist, K., Hartvig, H., Tvede, M., & Juul, S. (2002). Estimating time to pregnancy from current durations in a cross-sectional sample. *Biostatistics*, *3*(4), 565–578.

Kerr, D. C., Capaldi, D. M., Owen, L. D., Wiesner, M., & Pears, K. C. (2011). Changes in at-risk american men's crime and substance use trajectories following fatherhood. *Journal of marriage and family*, *73*(5), 1101–1116.

Keynes, J. M. (1936). *The general theory of employment, interest and money (1936)*. Kessinger Publishing.

Kiefer, N. M. (1988). Economic duration data and hazard functions. *Journal of economic literature*, *26*(2), 646–679.

Kiefer, N. M., Lundberg, S. J., & Neumann, G. R. (1985). How long is a spell of unemployment? illusions and biases in the use of cps data. *Journal of Business & Economic Statistics*, *3*(2), 118–128.

King, R. D., Massoglia, M., & MacMillan, R. (2007). The context of marriage and crime: Gender, the propensity to marry, and offending in early adulthood. *Criminology*, *45*(1), 33–65.

Kreager, D. A., Matsueda, R. L., & Erosheva, E. A. (2010). Motherhood and criminal desistance in disadvantaged neighborhoods. *Criminology*, *48*(1), 221–258.

Krueger, A. B., & Mueller, A. (2010). Job search and unemployment insurance: New evidence from time use data. *Journal of Public Economics*, *94*(3-4), 298–307.

Krueger, A. B., & Mueller, A. I. (2016). A contribution to the empirics of reservation wages. *American Economic Journal: Economic Policy*, *8*(1), 142–79.

Krueger, A. B., Mueller, A., Davis, S. J., & Şahin, A. (2011). Job search, emotional well-being, and job finding in a period of mass unemployment: Evidence from high frequency longitudinal data [with comments and discussion]. *Brookings Papers on Economic Activity*, 1–81.

Landais, C. (2015). Assessing the welfare effects of unemployment benefits using the regression kink design. *American Economic Journal: Economic Policy*, *7*(4), 243–78.

Landers, M. D., Mitchell, O., & Coates, E. E. (2015). Teenage fatherhood as a potential turning point in the lives of delinquent youth. *Journal of Child and Family Studies*, *24*(6), 1685–1696.

Laub, J. H., & Sampson, R. J. (2001). Understanding desistance from crime. *Crime and justice*, *28*, 1–69.

Lawn, J. E., Blencowe, H., Waiswa, P., Amouzou, A., Mathers, C., Hogan, D., Flenady, V., Frøen, J. F., Qureshi, Z. U., Calderwood, C., Et al. (2016). Stillbirths: Rates, risk factors, and acceleration towards 2030. *The Lancet*, *387*(10018), 587–603.

Le Barbanchon, T., Rathelot, R., & Roulet, A. (2017). Unemployment insurance and reservation wages: Evidence from administrative data. *Journal of Public Economics*.

Lee, D. S., & McCrary, J. (2005). *Crime, punishment, and myopia* (tech. rep.). National Bureau of Economic Research.

Levy, M. (2010). An empirical analysis of biases in cigarette addiction.

Marinescu, I. (2017). The general equilibrium impacts of unemployment insurance: Evidence from a large online job board. *Journal of Public Economics*, *150*, 14–29.

Marston, S. T., Hall, R. E., Holt, C. C., & Feldstein, M. S. (1975). The impact of unemployment insurance on job search. *Brookings Papers on Economic Activity*, *1975*(1), 13–60.

Massoglia, M., & Uggen, C. (2007). Subjective desistance and the transition to adulthood. *Journal of Contemporary Criminal Justice*, *23*(1), 90–103.

Maume, M. O., Ousey, G. C., & Beaver, K. (2005). Cutting the grass: A reexamination of the link between marital attachment, delinquent peers and desistance from marijuana use. *Journal of Quantitative Criminology*, *21*(1), 27–53.

McCrary, J. Et al. (2010). Dynamic perspectives on crime. *Handbook on the Economics of Crime*, *82*.

McGloin, J. M., Sullivan, C. J., Piquero, A. R., Blokland, A., & Nieuwbeerta, P. (2011). Marriage and offending specialization: Expanding the impact of turning points and the process of desistance. *European Journal of Criminology*, *8*(5), 361–376.

Mercer, N., Zoutewelle-Terovan, M. V., & van der Geest, V. (2013). Marriage and transitions between types of serious offending for high-risk men and women. *European journal of criminology*, *10*(5), 534–554.

Meyer, B. D. (1990). Unemployment insurance and unemployment spells. *Econometrica*, *58*(4), 757–782.

Mitchell, O., Landers, M., & Morales, M. (2018). The contingent effects of fatherhood on offending. *American Journal of Criminal Justice*, *43*(3), 603–626.

Monsbakken, C. W., Lyngstad, T. H., & Skardhamar, T. (2012). Crime and the transition to parenthood: The role of sex and relationship context. *British Journal of Criminology*, *53*(1), 129–148.

Mosher, W., Jones, J., & Abma, J. C. (2012). Intended and unintended births in the united states: 1982-2010 [(link)]. *National health statistics reports*, (55), 1–28.

Mukoyama, T., Patterson, C., & Şahin, A. (2018). Job search behavior over the business cycle. *American Economic Journal: Macroeconomics*, *10*(1), 190–215.

Na, C. (2016). The consequences of fatherhood transition among disadvantaged male offenders: Does timing matter? *Journal of Developmental and Life-Course Criminology*, *2*(2), 182–208.

Nekoei, A., & Weber, A. (2017). Does extending unemployment benefits improve job quality? *American Economic Review*, *107*(2), 527–61.

O'Donoghue, T., & Rabin, M. (1999). Addiction and self-control. *Addiction: Entries and exits*, *169206*.

Petras, H., Nieuwbeerta, P., & Piquero, A. R. (2010). Participation and frequency during criminal careers across the life span. *Criminology*, *48*(2), 607–637.

Piquero, A. R., MacDonald, J. M., & Parker, K. F. (2002). Race, local life circumstances, and criminal activity. *Social Science Quarterly*, *83*(3), 654–670.

Potter, F., Ziegler, J., Roemer, G., Richman, S., Wang, S., Clarkwest, A., Bodenlos, K., Et al. (2014). *Benefit accuracy measurement methodology evaluation* (tech. rep.). Mathematica Policy Research.

Pyrooz, D. C., Mcgloin, J. M., & Decker, S. H. (2017). Parenthood as a turning point in the life course for male and female gang members: A study of within-individual changes in gang membership and criminal behavior. *Criminology*, *55*(4), 869–899.

Ragan, D. T., & Beaver, K. M. (2010). Chronic offenders: A life-course analysis of marijuana users. *Youth & Society*, *42*(2), 174–198.

Rossin-Slater, M. (2017). Signing up new fathers: Do paternity establishment initiatives increase marriage, parental investment, and child well-being? *American Economic Journal: Applied Economics*, *9*(2), 93–130.

Rothstein, J. (2011). Unemployment insurance and job search in the great recession. *Brookings Papers on Economic Activity*, 143.

Salant, S. W. (1977). Search theory and duration data: A theory of sorts. *The Quarterly Journal of Economics*, *91*(1), 39–57.

Salvatore, C., & Taniguchi, T. A. (2012). Do social bonds matter for emerging adults? *Deviant behavior*, *33*(9), 738–756.

Sampson, R. J., & Laub, J. H. (1992). Crime and deviance in the life course. *Annual review of sociology*, *18*(1), 63–84.

Sampson, R. J., & Laub, J. H. (2009). *Shared beginnings, divergent lives*. Harvard University Press.

Sampson, R. J., Laub, J. H., & Wimer, C. (2006). Does marriage reduce crime? a counterfactual approach to within-individual causal effects. *Criminology*, *44*(3), 465–508.

Savolainen, J. (2009). Work, family and criminal desistance: Adult social bonds in a nordic welfare state. *The British Journal of Criminology*, *49*(3), 285–304.

Schilbach, F. (2019). Alcohol and self-control: A field experiment in india. *American economic review*, *109*(4), 1290–1322.

Sickles, R. C., & Williams, J. (2008). Turning from crime: A dynamic perspective. *Journal of Econometrics*, *145*(1-2), 158–173.

Sider, H. (1985). Unemployment duration and incidence: 1968-82. *The American Economic Review*, *75*(3), 461–472.

Skandalis, D., & Marinescu, I. E. (2019). Unemployment insurance and job search behavior. *Working paper*.

Skarðhamar, T., & Lyngstad, T. H. (2009). Family formation, fatherhood and crime: An invitation to a broader perspectives on crime and family transitions.

Skardhamar, T., Monsbakken, C. W., & Lyngstad, T. H. (2014). Crime and the transition to marriage: The role of the spouse's criminal involvement. *British Journal of Criminology*, *54*(3), 411–427.

Skardhamar, T., Savolainen, J., Aase, K. N., & Lyngstad, T. H. (2015). Does marriage reduce crime? *Crime and justice*, *44*(1), 385–446.

Terplan, M., Cheng, D., & Chisolm, M. S. (2014). The relationship between pregnancy intention and alcohol use behavior: An analysis of prams data. *Journal of substance abuse treatment*, *46*(4), 506–510.

Theobald, D., Farrington, D. P., & Piquero, A. R. (2015). Does the birth of a first child reduce the father's offending? *Australian & New Zealand Journal of Criminology*, *48*(1), 3–23.

Thompson, M., & Petrovic, M. (2009). Gendered transitions: Within-person changes in employment, family, and illicit drug use. *Journal of research in crime and delinquency*, *46*(3), 377–408.

Tremblay, M. D., Sutherland, J. E., & Day, D. M. (2017). Fatherhood and delinquency: An examination of risk factors and offending patterns associated with fatherhood status among serious juvenile offenders. *Journal of child and family studies*, *26*(3), 677–689.

US Dept of Labor, E. T. A. (2009). Benefit accuracy measurement state operations handbook [(link)]. *Office of Unemployment Insurance, Division of Performance Management*, *1205-0245*.

USDOL. (2018). Benefit accuracy measurement state data summary improper payment information act performance year 2018 [(link)]. *Technical Report*.

Van Schellen, M., Apel, R., & Nieuwbeerta, P. (2012).
because you're mine, i walk the line"? marriage, spousal criminality, and criminal offending over the life course. *Journal of Quantitative Criminology*, *28*(4), 701–723.

Waite, L. J., & Gallagher, M. (2001). *The case for marriage: Why married people are happier, healthier, and better off financially*. Random House Digital, Inc.

Yakusheva, O., & Fletcher, J. (2015). Learning from teen childbearing experiences of close friends: Evidence using miscarriages as a natural experiment. *Review of Economics and Statistics*, *97*(1), 29–43.

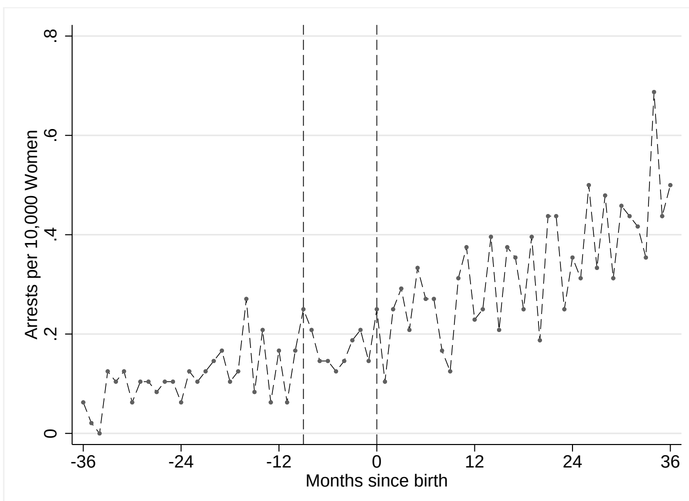
Zoutewelle-Terovan, M., & Skardhamar, T. (2016). Timing of change in criminal offending around entrance into parenthood: Gender and cross-country comparisons for at-risk individuals. *Journal of Quantitative Criminology*, *32*(4), 695–722.

Zoutewelle-Terovan, M., Van Der Geest, V., Liefbroer, A., & Bijleveld, C. (2014). Criminality and family formation: Effects of marriage and parenthood on criminal behavior for men and women. *Crime & Delinquency*, *60*(8), 1209–1234.

Appendix A

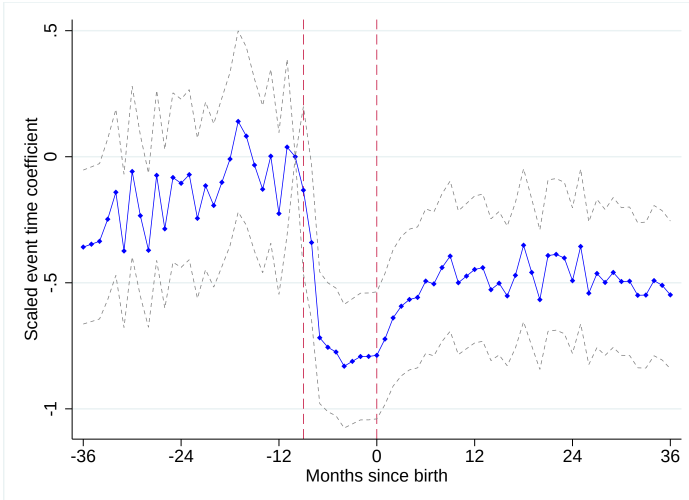
Appendix to Chapter 1

Figure A.1: Driving without a license, mothers



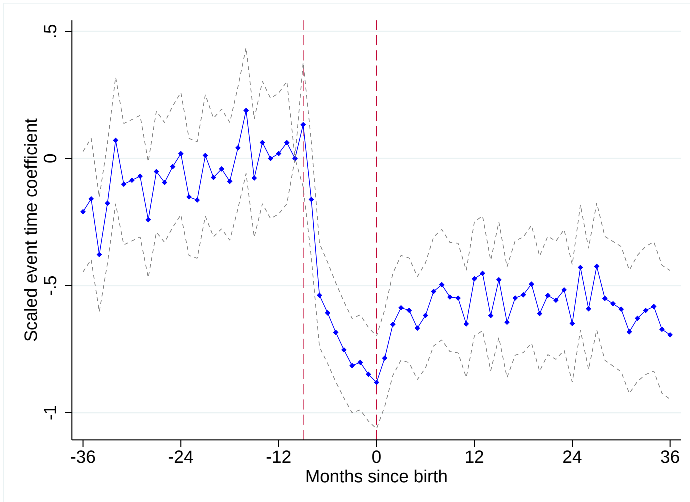
Includes fully-balanced arrest data for 480,111 first-time mothers. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure A.2: Event study coefficients for alcohol offenses, mothers under 21 years old



Includes 67,899 births. Dots show point estimates and dashed lines show 95% confidence intervals from an event study around birth shown in Equation 1.1. The coefficients are scaled by the average offense rate in the omitted period, 10 months before birth. The dashed lines marks 9 months before the birth and the month of the birth.

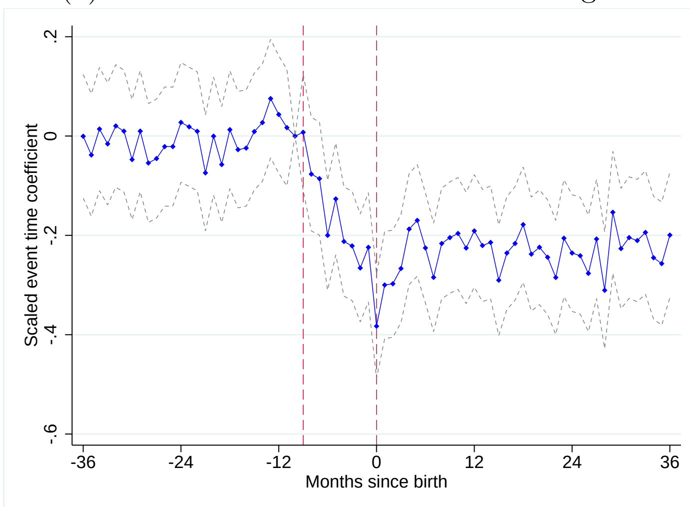
Figure A.3: Event study coefficients for teen mothers



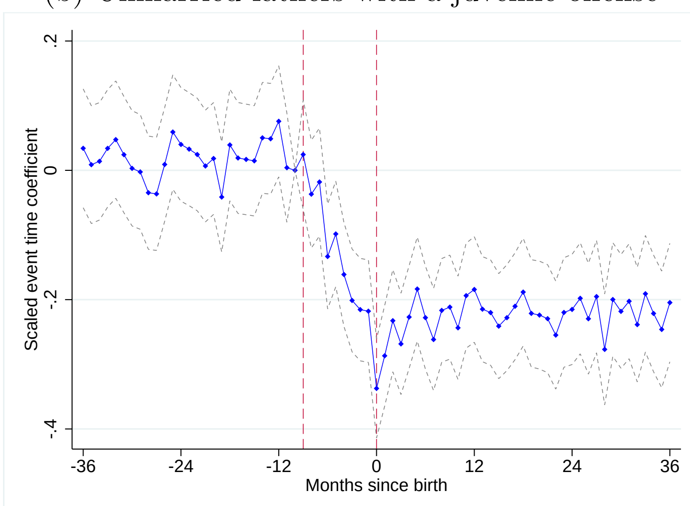
Includes a fully balanced panel of 45,759 first-time mothers who gave birth at age 19 or younger. Dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 1.1, with an indicator for any economic, drug, DUI, or property destruction offense as the dependent variable. The coefficients are divided by the average offense rate in the omitted period, 10 months before birth. The dashed lines mark 9 months before the birth and the month of birth.

Figure A.4: Event studies around childbirth, unmarried fathers

(a) Unmarried fathers born in Washington



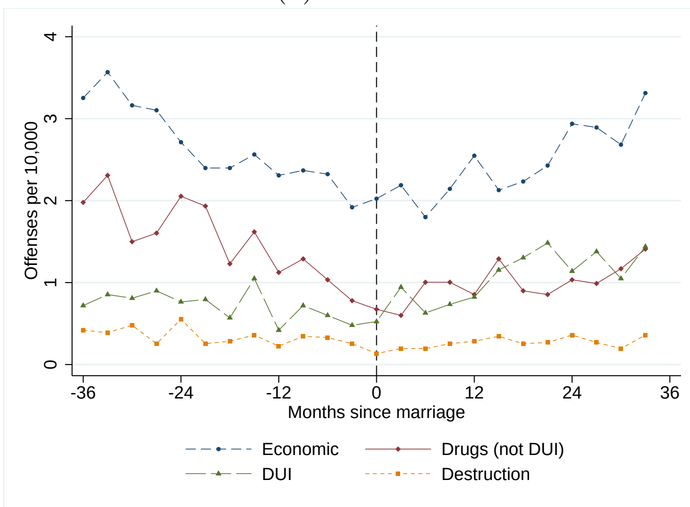
(b) Unmarried fathers with a juvenile offense



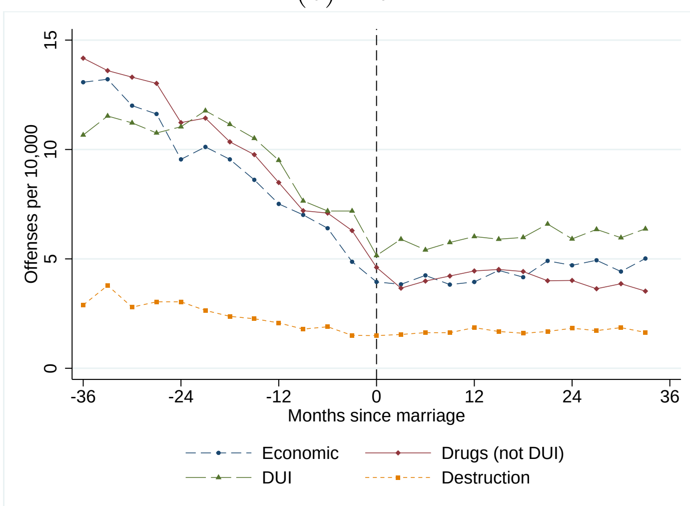
Panel (a) includes 15,600 fathers, panel (b) includes 37,014 fathers. Dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 1.1, with an indicator for a drug, DUI, economic, or property destruction offense as the dependent variable. The coefficients are divided by the average offense rate in the omitted period, 10 months before birth. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure A.5: Raw averages around marriage

(a) Women



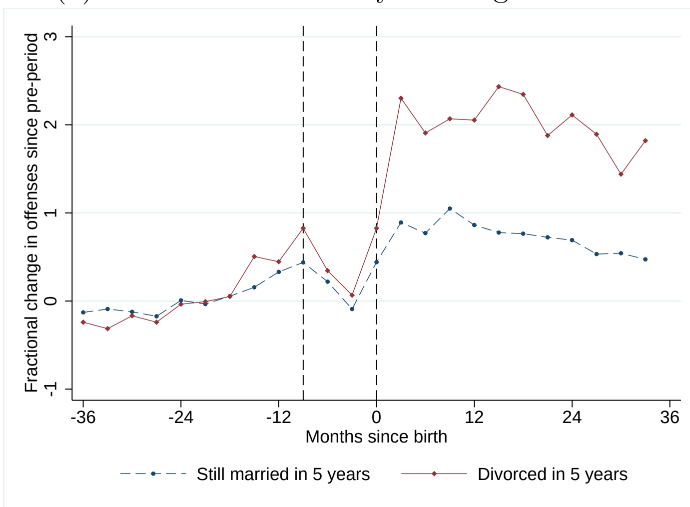
(b) Men



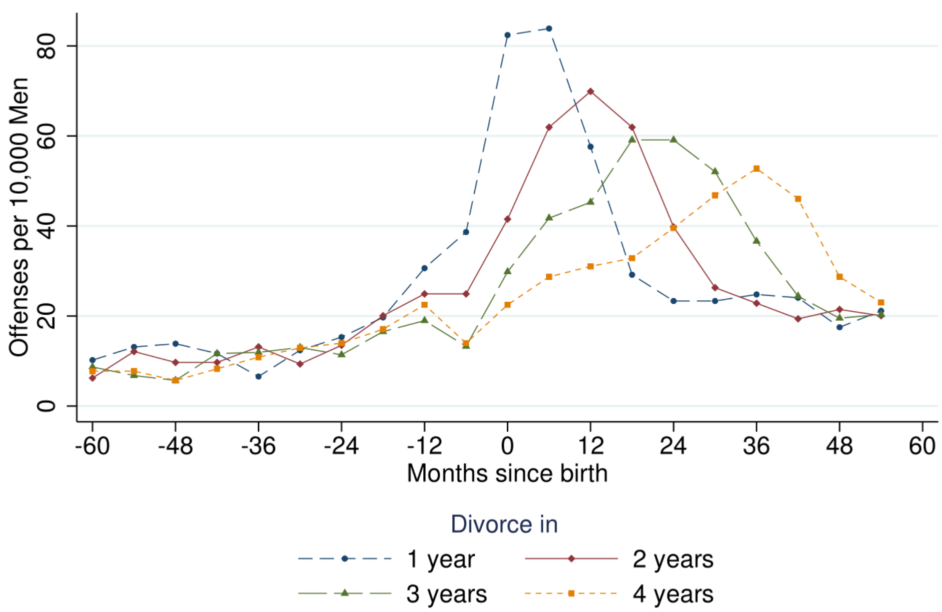
Includes all fathers (N=245,756) and mothers (N=222,392) from the birth data who are visible in the offense data 3 years after and 3 years before their marriage. The vertical dashed line marks the month of marriage.

Figure A.6: Domestic violence vs. divorce

(a) Domestic violence by marriage outcome



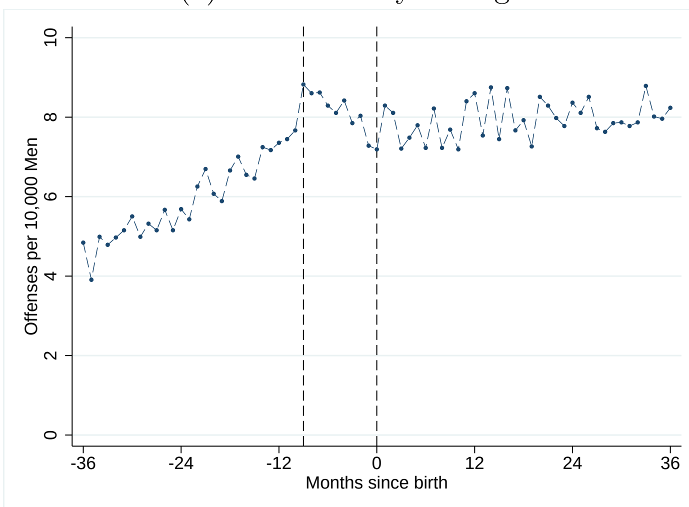
(b) Domestic violence by divorce timing



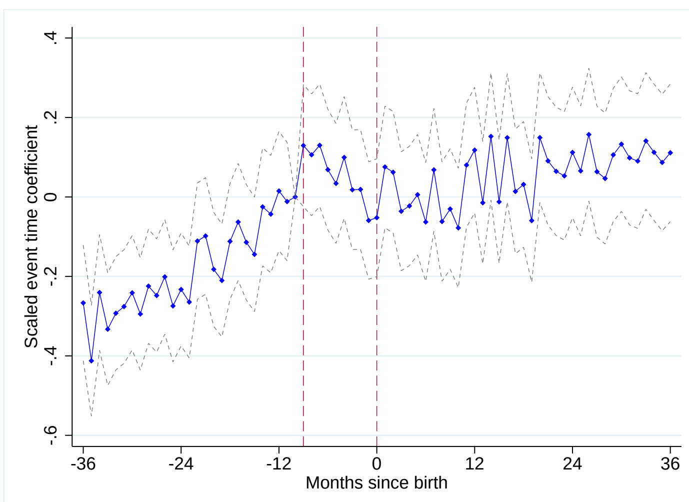
Panel (a) includes 364,076 still-married men and 21,038 divorced men. Panel (b) includes all men who were married for their first birth and then divorced 1-4 years after. Grouping is based on the rounded time in years between the child's birth date and date of the divorce decree (when the divorce is finalized). Sample sizes for the four groups are 2,285 (1 year), 4,816 (2 years), 6,147 (3 years), and 6,444 (4 years).

Figure A.7: Fathers traffic offenses

(a) Raw monthly average



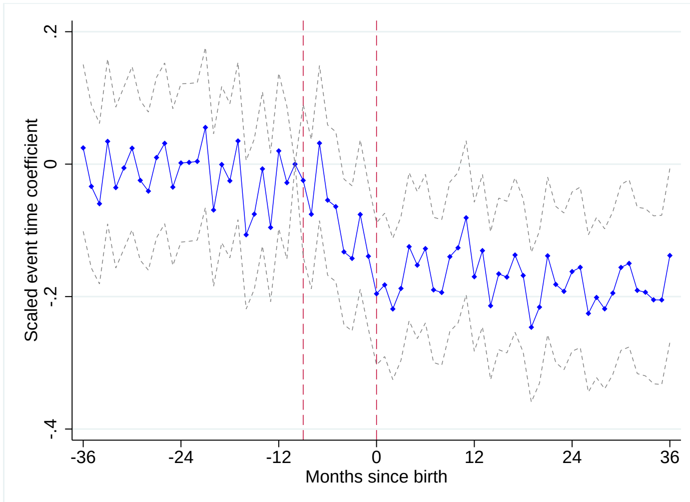
(b) Event study specification



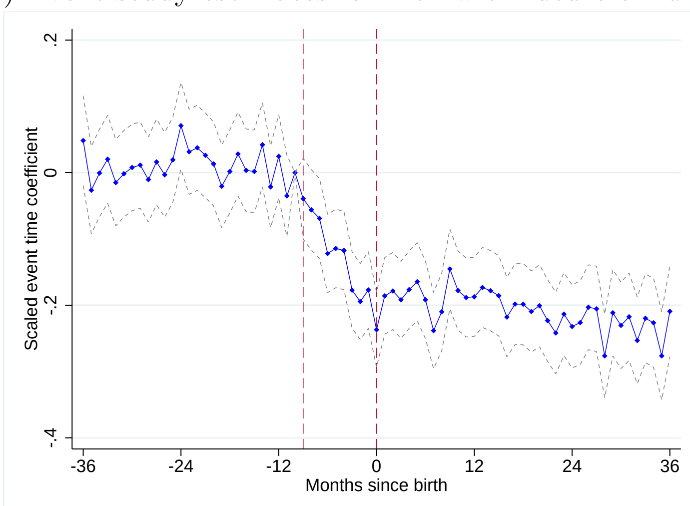
Panels show traffic offenses (mostly reckless driving and driving with an expired license) for 545,166 first-time fathers. In panel (b), dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 1.1, with an indicator for a traffic offense as the dependent variable. The coefficients are divided by the average offense rate in the omitted period, 10 months before birth. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure A.8: Outmigration

(a) Event study estimates for men with future crime

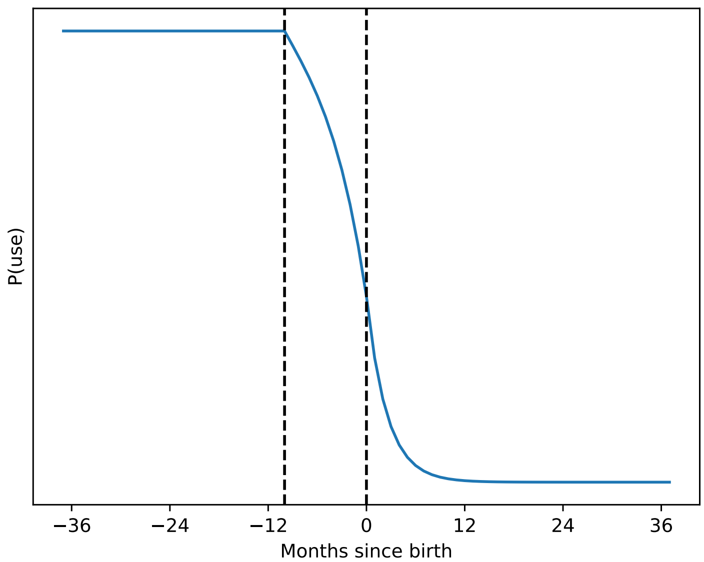


(a) Event study estimates for men with future children



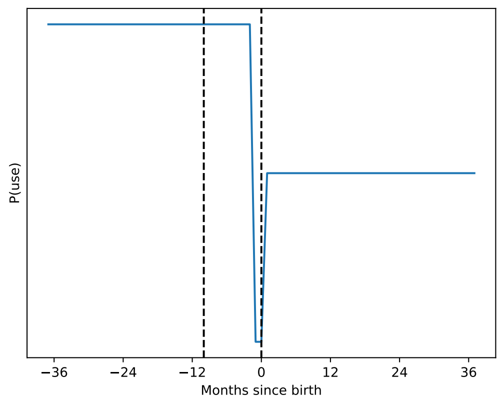
Both panels show point estimates and 95% confidence intervals from the event study specification given in Equation 1.1 for first-time fathers. Panel (a) restricts to men charged with a driving-related (including DUI) offense 4-5 years after the birth (N=14,980). The outcome for the specification underlying panel (a) is an indicator for any economic, drug, or destruction offense. Panel (b) restricts to fathers who at some point have a 2nd child in Washington (N=116,540), with an indicator for any economic, drug, DUI, or destruction offense as the outcome.

Figure A.9: Model calibration

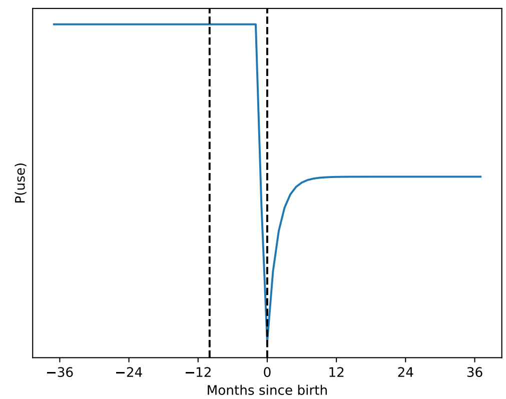


This plots the simulated choice probabilities with f changing from .8 to .2 at birth, and δ=1, $\sigma = 8$, $\rho = 1$

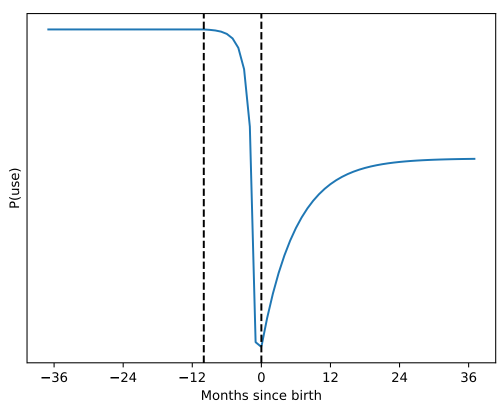
Figure A.10: Model calibration, two shocks



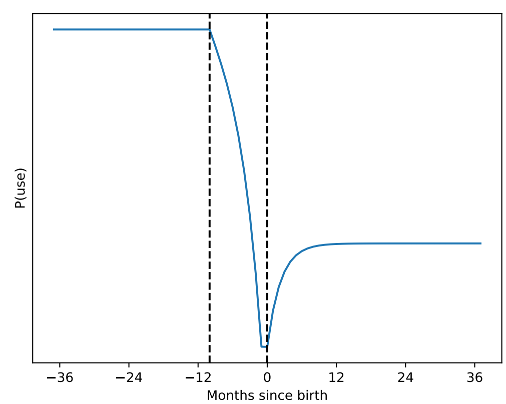
(a) Any δ, no habit formation



(b) Fully myopic, some habit formation



(c) $\delta = .75$, some habit formation



(d) $\delta = 1$, some habit formation

Table A.1: Papers on Crime and Childbearing or Marriage

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Gottlieb and Sugie (2019)	Justice Quarterly	NLSY97, N=8,496	Both cohabitation and marriage are associated with reductions in offending
Mitchell, Landers, and Morales (2018)	American Journal of Criminal Justice	NLSY97, N=2,787 non-fathers, 1,772 fathers	Fatherhood is associated with decreased substance use but not the likelihood of any arrest
Pyrooz, Mcgloin, and Decker (2017)	Criminology	NLSY97, N=629	Mothers and residential fathers have decreased likelihoods of gang membership and offending
Tremblay, Sutherland, and Day (2017)	Journal of Child and Family Studies	Pathways to Desistance Study, N=1,170	Fatherhood is associated with greater risk exposure among serious juvenile offenders
Na (2016)	Journal of Developmental and Life Course Criminology	Pathways to Desistance Study, N=864 adolescents and N=476 young adults	Teen fathers report increased offending following childbirth; older fathers experience a slight decrease
Zoutewelle-Terovan and Skardhamar (2016)	Journal of Quantitative Criminology	Statistics Norway, N=289 & Netherlands' Municipal Population Register and Judicial Documentation, N=279	For at-risk mothers and fathers, decrease leading up to birth; increase to higher levels afterwards

Table A.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Landers, Mitchell, and Coates (2015)	Journal of Child and Family Studies	NLSY 1997, N=478	Young fathers have decreased drug use controlling for individual fixed effects
Craig (2015)	Journal of Crime and Justice	Add Health, N=3,327	Marriage decreases offending among whites and Hispanics but not blacks; Parenthood only decreases whites' offending
Theobald, Farrington, and Piquero (2015)	Australian & New Zealand Journal of Criminology	Australian & New Zealand Journal of Criminology & Cambridge Study in Delinquent Development, N=411	The number of convictions decreases after childbirth for men; this effect is greater if the child is born before or within nine months of marriage
Barnes et al. (2014)	Justice Quarterly	Add Health, N=15,701	Marriage is correlated with but does not cause desistance
Zoutewelle-Terovan et al. (2014)	Crime & Delinquency	Netherlands Ministry of Justice, N=540	Marriage and parenthood both promote desistance of serious offending for men but not women
Skardhamar, Monsbakken, and Lyngstad (2014)	The British Journal of Criminology	Norwegian Register, N=80,064	Offending declines the year of before marriage followed by a slight increase after marriage; the rebound is due to those who split up

Table A.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Craig and Foster (2013)	Deviant Behavior	Add Health, N=3,082	Marriage decreases delinquent behavior for both males and females
Monsbakken, Lyngstad, and Skardhamar (2012)	The British Journal of Criminology	Statistics Norway, N=208,296 persons (101,480 women and 106,816 men)	Offending declines permanently before childbirth despite slight rebound after
Bersani and Doherty (2013)	Criminology	NLSY97, N=2,838	Marriage decreases the likelihood of arrest; Offending is higher when one is divorced than when one is married
Doherty and Ensminger (2013)	Journal of Research in Crime and Delinquency	The Woodlawn Project, N=965	Marriage reduces offending for men only
Jaffee, Lombardi, and Coley (2013)	Development and Psychopathology	Add Health, N=4,149	Marriage is associated with a lower rate of criminal activity
Mercer, Zoutewelle-Terovan, and van der Geest (2013)	European Journal of Criminology	Netherlands Ministry of Justice & Population Registration, N=540	Married males have a higher likelihood of committing violent offenses compared with non-married males; reverse is true for women
Barnes and Beaver (2012)	Journal of Marriage and Family	Add Health, N=2,284 sibling pairs	Marriage is associated with desistance; this effect decreases after controlling for genetic influences

Table A.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Beijers, Bijleveld, and van Poppel (2012)	European Journal of Criminology	Netherlands, N=971	Marriage is associated with desistance among high-risk men married after 1970 in the Netherlands
Salvatore and Taniguchi (2012)	Deviant Behavior	Add Health, N=4,880	Both marriage and parenthood reduce offending
Van Schellen, Apel, and Nieuwbeerta (2012)	Journal of Quantitative Criminology	Netherlands CCLS, N=4,615	Marriage is associated with decreased conviction frequency for women; only marriage to a non-convicted spouse is beneficial for men
Kerr et al. (2011)	Journal of Marriage and Family	US - Capaldi and Patterson (1989) Study, N=206	Men desist from crime and use alcohol and tobacco less frequently following childbirth
Giordano et al. (2011)	Journal of Criminal Justice	Toledo Adolescent Relationships Study (TARS), N=1,066	Mothers are more likely to desist from crime than fathers; parents from disadvantaged backgrounds have less desistance than those from advantaged ones
Forrest and Hay (2011)	Criminology & Criminal Justice	NLSY79, N=2,325	Unlike cohabitation, marriage is associated with reduced crime, but effects decrease once controlling for self-control measures

Table A.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Herrera, Wiersma, and Cleveland (2011)	Journal of Research on Adolescence	Add Health, N=1,267 opposite sex romantic pairs	Relationship quality and length are associated with decreased crime
McGloin et al. (2011)	European Journal of Criminology	Netherlands CCLS, N=4,612	The year of marriage and year after have the greatest effect on decreasing offending
Kreager, Matsueda, and Erosheva (2010)	Criminology	Denver Youth Survey, N=567	Teen and young adult motherhood is associated with decreased delinquency for disadvantaged women; controlling for motherhood and age, marriage is not associated with desistance
Petras, Nieuwbeerta, and Piquero (2010)	Criminology	Netherlands CCLS, N=4,615	The effects of marriage on probability and frequency of conviction are both negative
Ragan and Beaver (2010)	Youth & Society	Add Health, N=1,884	Marriage is associated with marijuana desistance
Skarðhamar and Lyngstad (2009)	Statistics Norway Discussion Papers	Norwegian Register (Marriage N=121,207; First birth=175,118)	Men desist from crime leading up to marriage/childbirth; some rebound for serious offenses
Bersani, Laub, and Nieuwbeerta (2009)	Journal of Quantitative Criminology	Netherlands CCLS, N=4,615	Marriage is associated with a decrease in the odds of a conviction; the effect for women is less than that for men

Table A.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Savolainen (2009)	The British Journal of Criminology	Statistics Finland, N=1,325	Cohabitation has a stronger effect on desistance than marriage; parenthood is associated with decreased crime
Thompson and Petrovic (2009)	Journal of Research in Crime and Delinquency	NYS, N=1,496	First childbirth increases odds of drug usage for men and women, except single mothers; marriage decreases odds of drug usage for men but women's drug usage depends on strength of relationship
Beaver et al. (2008)	Social Science Research	Add Health, N=1,555	Being married increases the odds of desisting
King, Massoglia, and MacMillan (2007)	Criminology	NYS, N=1,725	After accounting for selection into marriage, marriage has a significant but small effect on crime; the decrease is much greater for males than females
Massoglia and Uggen (2007)	Journal of Contemporary Criminal Justice	Youth Development Study, N=1,000	Relationship quality is positively correlated with desistance

Table A.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Sampson, Laub, and Wimer (2006)	Criminology	Glueck and Glueck study (1950), N=500 male delinquents and 500 male nondelinquents	Marriage is associated with a 35 percent reduction in the odds of crime for men
Maume, Ousey, and Beaver (2005)	Journal of Quantitative Criminology	NYS waves 5-6, N=593	Marriage promotes marijuana desistance only for those with high marital attachment
Hope, Wilder, and Watt (2003)	The Sociological Quarterly	Add Health, N=6,877	Adolescent girls who keep their babies reduce delinquent behavior compared to those with other pregnancy resolutions
Piquero, MacDonald, and Parker (2002)	Social Science Quarterly	California Youth Authority, N=524	Controlling for individual differences, marriage is negatively associated with violent, but not nonviolent, arrests
Graham and Bowling (1995)	Home Office Research Study	UK household survey, N=2,529	Having children is a strong predictor of desistance for females but not for males

Table A.2: Descriptive statistics, Father sample

Variable	(1) All births	(2) + Clear match	(3) +Father's first	(4) Stillbirths
Mother age	27.84	28.04	27.12	27.50
	(5.98)	(5.95)	(6.02)	(6.67)
Father age	30.21	30.40	29.36	29.61
	(6.54)	(6.50)	(6.62)	(7.19)
Mother married at birth	0.73	0.75	0.71	0.61
	(0.44)	(0.43)	(0.46)	(0.49)
Mother on Medicaid	0.36	0.34	0.36	
	(0.48)	(0.47)	(0.48)	
WIC	0.34	0.33	0.34	0.26
	(0.47)	(0.47)	(0.47)	(0.44)
Twins+	0.02	0.02	0.02	0.06
	(0.12)	(0.13)	(0.13)	(0.23)
Male infant	0.51	0.51	0.51	0.53
	(0.50)	(0.50)	(0.50)	(0.50)
Low birth weight (<2500g)	0.05	0.05	0.06	0.60
	(0.22)	(0.22)	(0.23)	(0.49)
Any father arrest	0.41	0.36	0.34	0.26
	(0.49)	(0.48)	(0.47)	(0.44)
Any mother arrest	0.25	0.23	0.23	0.21
	(0.43)	(0.42)	(0.42)	(0.41)
Median zipcode income	59820.84	60202.36	59893.14	58077.98
	(18182.44)	(18313.21)	(18092.66)	(17786.50)
Midpregnancy marriage	0.03	0.03	0.05	0.05
	(0.18)	(0.18)	(0.21)	(0.21)
Divorce	0.22	0.22	0.22	0.36
	(0.42)	(0.41)	(0.41)	(0.48)
Father ever incarcerated	0.04	0.03	0.03	0.03
	(0.20)	(0.17)	(0.16)	(0.18)
Father ever on probation	0.09	0.07	0.06	0.06
	(0.28)	(0.25)	(0.24)	(0.24)
Observations	976,581	896,459	545,166	3,831

Standard deviations shown in parentheses. Insurance and ethnicity not recorded for stillbirths. Median zipcode income is for the years 2006-2010 from the American Community Survey via Michigan's Population Studies Center.

Appendix B

Appendix to Chapter 2

Figure B.1: Reservation ratio

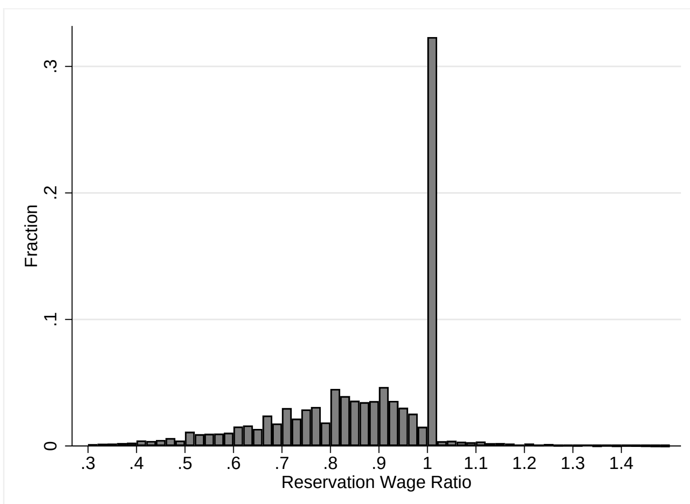


Figure B.2: Distribution of job contacts

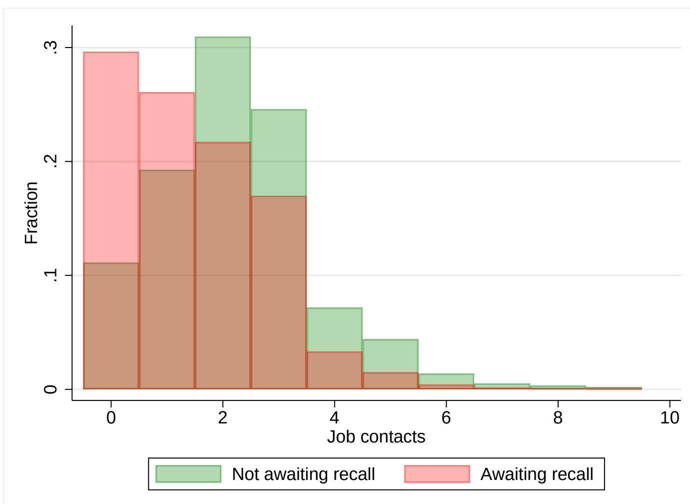
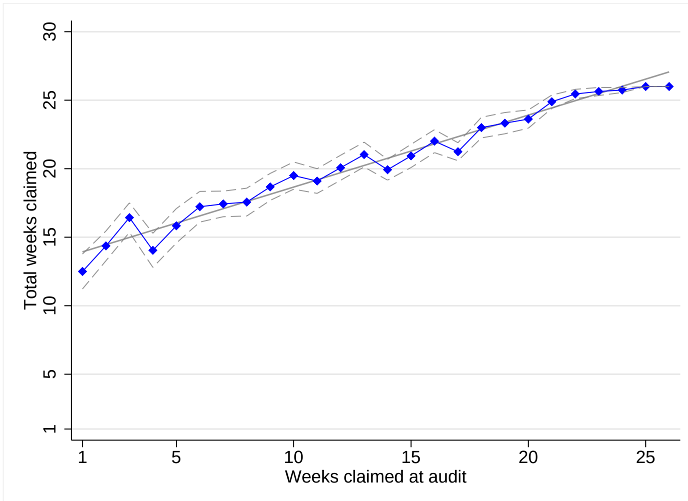
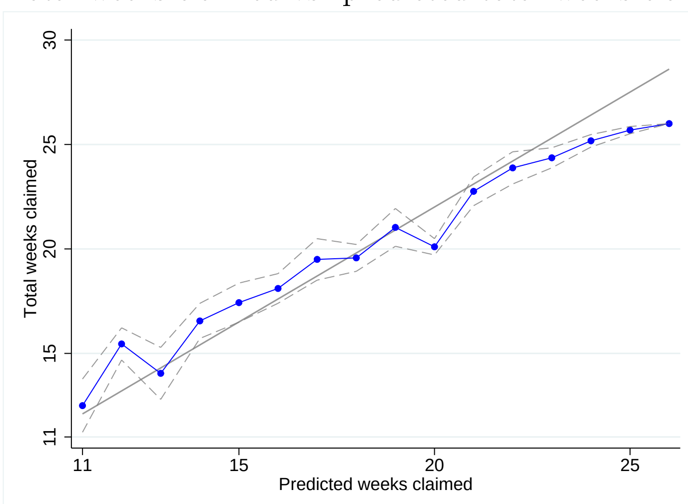


Figure B.3: Calibration using Missouri sample

(a) Total weeks claimed vs. weeks claimed at audit



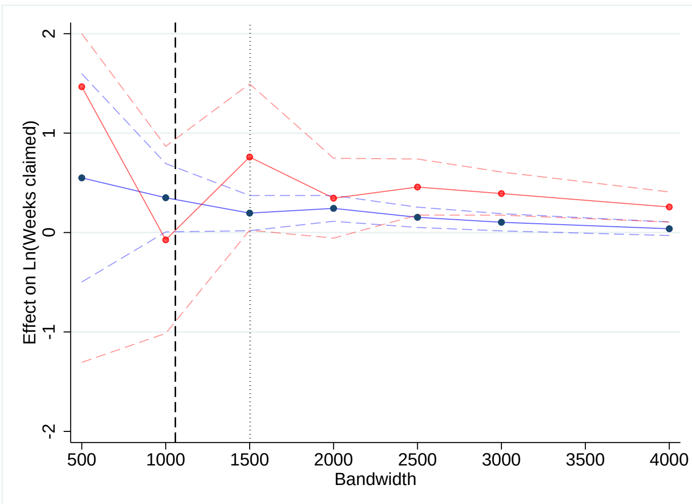
(b) Total weeks claimed vs. predicted total weeks claimed



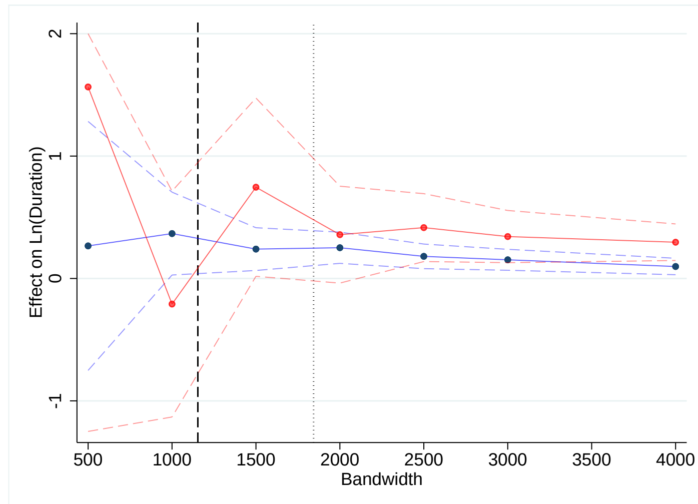
These plots show binned averages of completed duration against (a) weeks claimed at audit and (b) the expectation of completed duration using the method described in Section 2.2 for the 2,611 matched claimants from Missouri. The straight red line shows the fitted values from the estimated bivariate regression.

Figure B.4: RKD estimates varying bandwidth

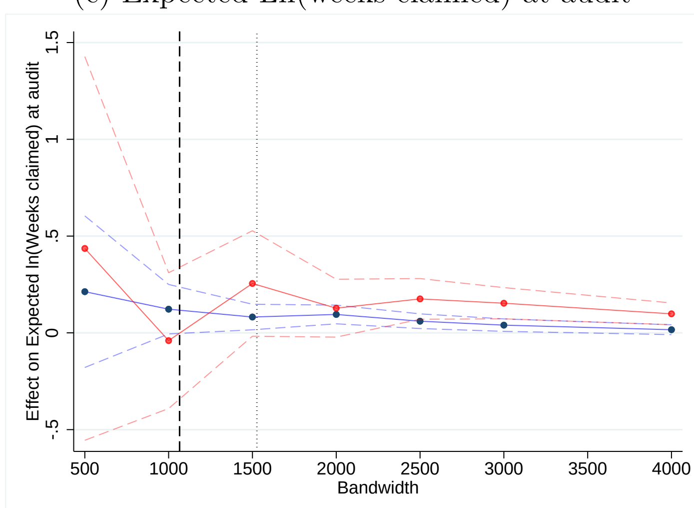
(a) Weeks claimed at audit



(b) Ln(unemployment duration) at audit



(c) Expected Ln(weeks claimed) at audit



Note: The blue series gives point estimates and 95% confidence intervals for the linear RKD specification, with the dashed verticle line indicating the optimal calonico2014robust bandwidth. The orange series and dotted vertical line show the analogous quantities for the quadratic specification.

Figure B.5: Histogram of percent spell completed at audit, Matched Missouri sample

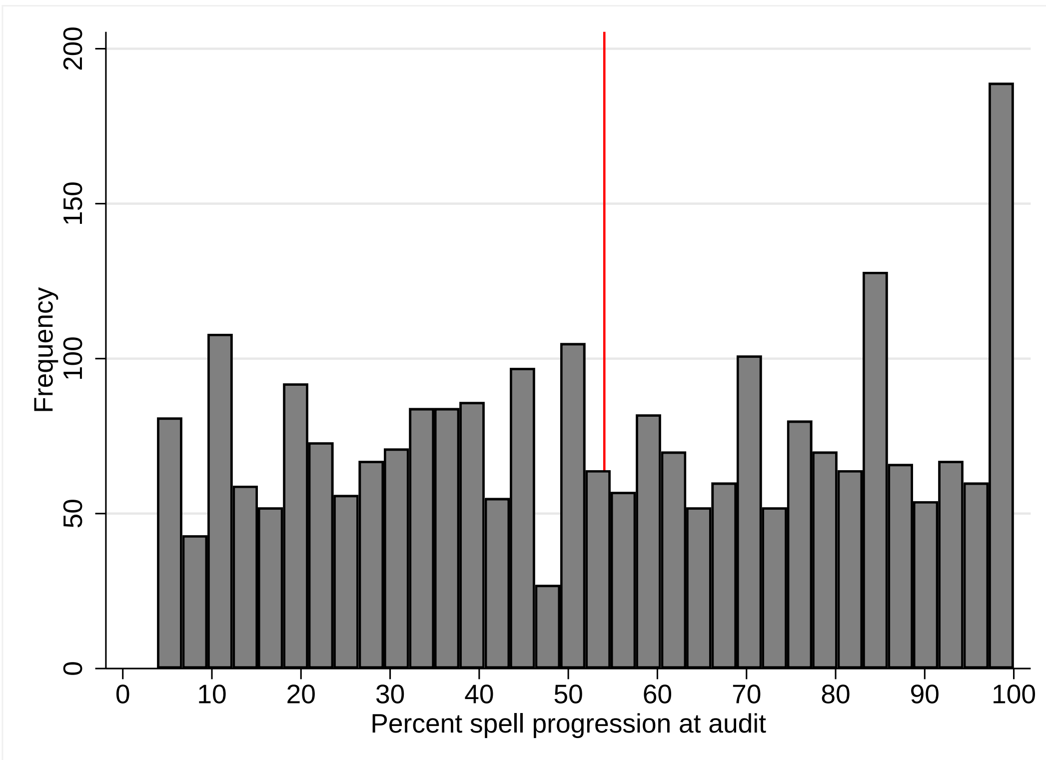


Figure B.6: Density check around kink point

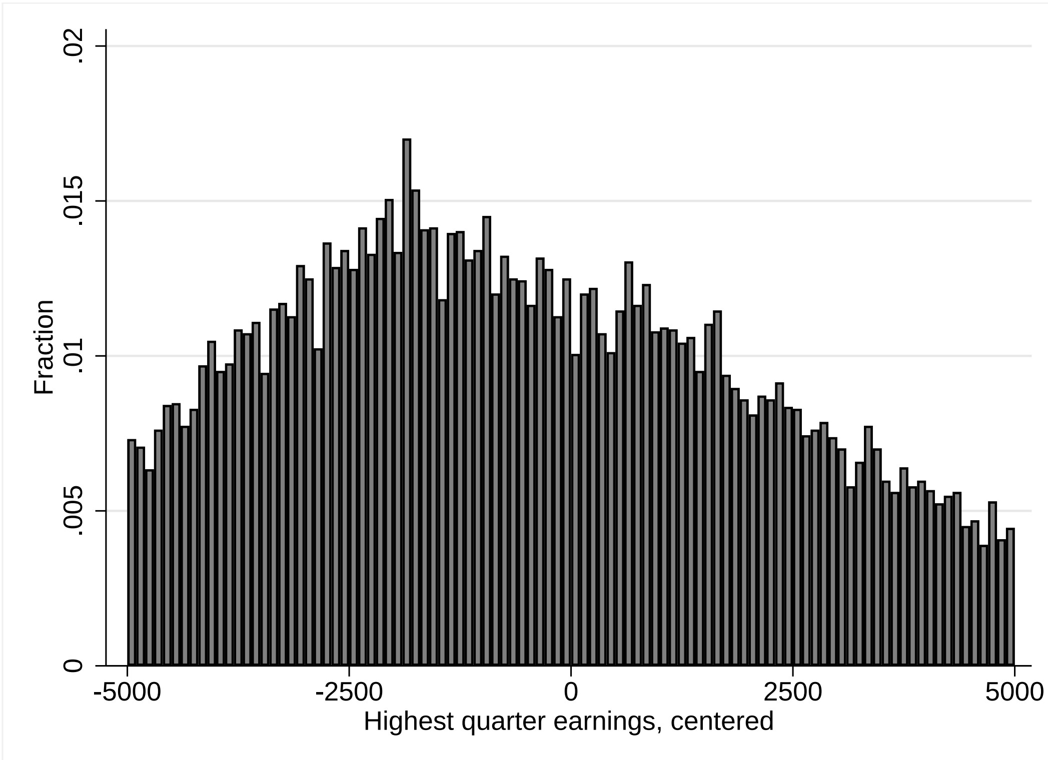
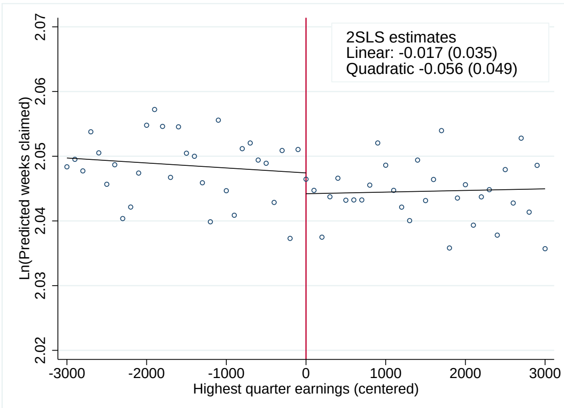
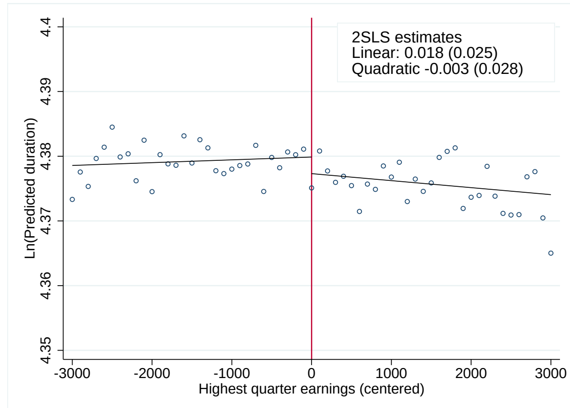


Figure B.7: RKDs showing covariate smoothness

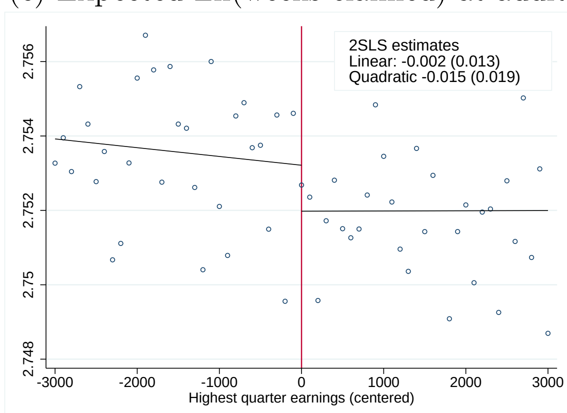
(a) Weeks claimed at audit



(b) Ln(unemployment duration) at audit



(c) Expected Ln(weeks claimed) at audit



Notes: Each plot shows the same RKD figures as Figure 2.2, except using fitted values from a regression specification that uses covariates to predict the outcomes. The full list of covariates is ethnicity, sex, age, eduction, reason for being laid off, vocational certification, and occupation-by-industry.

Figure B.8: State sample sizes in RKD

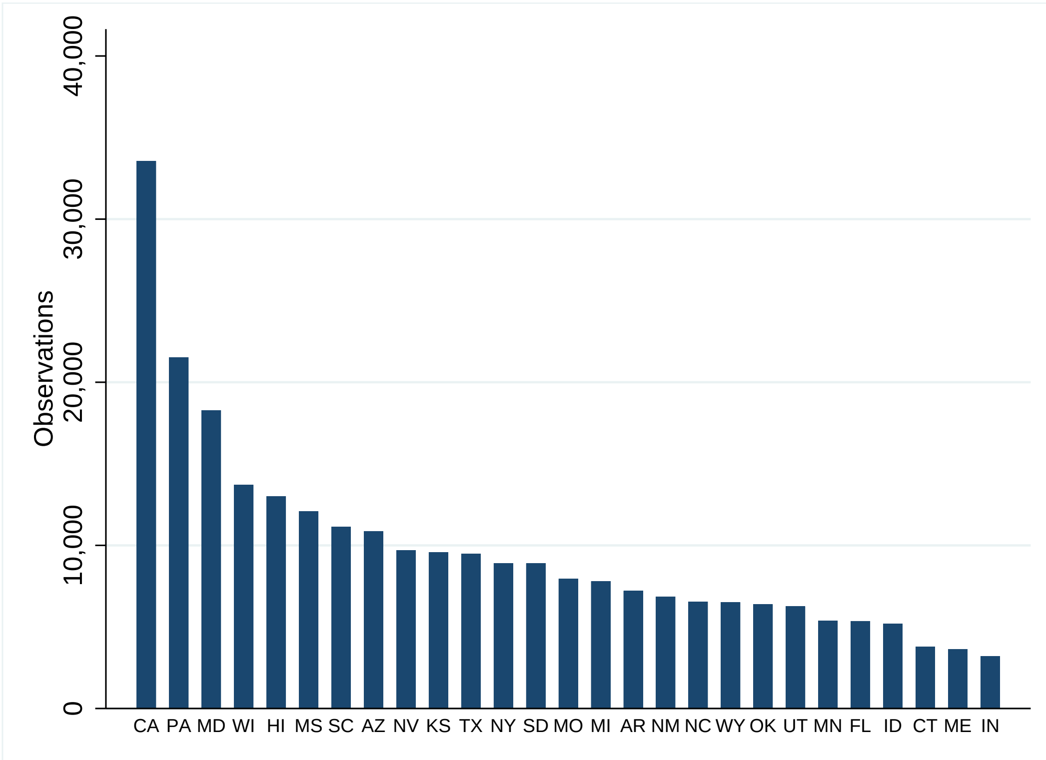


Table B.1: Descriptive statistics

Variable	(1) All	(2) First week	(3) Long term (>20 weeks)
Reserve ratio	0.859	0.875	0.840
	(0.175)	(0.168)	(0.184)
Switching occupation	0.201	0.175	0.221
	(0.401)	(0.380)	(0.415)
Job contacts	2.162	2.121	2.104
	(1.596)	(1.665)	(1.527)
Weeks claimed	10.818	1.233	23.266
	(7.349)	(0.448)	(2.586)
Weekly benefit amount	231.711	223.457	239.352
	(115.719)	(112.031)	(117.526)
Male	0.581	0.598	0.549
	(0.493)	(0.490)	(0.498)
Any dependents	0.083	0.083	0.096
	(0.276)	(0.276)	(0.294)
Black	0.158	0.142	0.165
	(0.365)	(0.349)	(0.371)
Age	40.759	39.905	41.809
	(12.539)	(12.350)	(12.720)
College	0.123	0.117	0.135
	(0.329)	(0.321)	(0.341)
Recall	0.252	0.347	0.164
	(0.434)	(0.476)	(0.370)
Definite recall date	0.166	0.255	0.097
	(0.372)	(0.436)	(0.296)
Layoff	0.697	0.710	0.657
	(0.459)	(0.454)	(0.475)
Quit	0.040	0.032	0.049
	(0.196)	(0.177)	(0.216)
Discharge	0.190	0.157	0.214
	(0.392)	(0.364)	(0.410)
Observations	921,801	81,693	145,440

Standard deviations shown in parentheses. The mean for the first week sample in column (2) is greater than one because claimants in states where two weeks are claimed at a time get the first week label in their first two weeks.

Table B.2: RKD results for duration outcomes

	(1)	(2)	(3)	(4)
Ln(Weeks claimed)	0.410	0.867	0.240	0.714
	(0.149)	(0.386)	(0.0568)	(0.310)
N	51,207	72,677	100,965	152,855
Bandwidth	1057	1503	2115	3429
Ln(Duration)	0.453	0.337	0.175	0.659
	(0.126)	(0.274)	(0.0490)	(0.303)
N	55,865	88,804	109,577	153,855
Bandwidth	1154	1843	2308	3459
Expected Ln(weeks claimed)	0.131	0.267	0.0955	0.282
	(0.0568)	(0.136)	(0.0209)	(0.111)
N	51,607	73,837	101,730	156,130
Bandwidth	1065	1527	2131	3529
Bandwidth times optimal	1	1	2	1
Polynomial order	1	2	1	3
Kernel	Uniform	Uniform	Uniform	Uniform

Standard errors in parentheses

$p < 0.10$, $p < 0.05$, $p < 0.01$

Note: Standard errors clustered at the benefit schedule level. Each coefficient gives the treatment effect of logged weekly benefit amount on the outcome indicated the leftmost column, estimated using 2SLS. MSE-optimal bandwidths are calculated using the `rdrobust` package calonico2017STATA.

Table B.3: RKD results for search outcomes, all claimants

	(1)	(2)	(3)	(4)
Ln(reservation wage)	-0.0122	-0.0772	0.0339	-0.0826
	(0.0635)	(0.106)	(0.0263)	(0.0895)
N	40,244	77,240	80,083	152,899
Bandwidth	880	1695	1759	3728
Reserve ratio	-0.0163	-0.101	-0.0402	-0.0631
	(0.0473)	(0.0553)	(0.0174)	(0.0415)
N	37,074	81,657	73,696	171,502
Bandwidth	813	1808	1627	4469
Switching occupation	-0.0505	-0.101	0.0317	0.0879
	(0.0743)	(0.144)	(0.0264)	(0.0928)
N	45,855	77,351	90,610	172,979
Bandwidth	981	1666	1962	4339
Contacts	0.217	0.683	0.0935	0.939
	(0.204)	(0.552)	(0.0802)	(0.574)
N	45,086	59,055	85,543	104,868
Bandwidth	1365	1798	2731	3580
Bandwidth times optimal	1	1	2	1
Polynomial order	1	2	1	3
Kernel	Uniform	Uniform	Uniform	Uniform

Standard errors in parentheses

$p < 0.10$, $p < 0.05$, $p < 0.01$

Note: Standard errors clustered at the benefit schedule level. Each coefficient gives the treatment effect of logged weekly benefit amount on the outcome indicated the leftmost column, estimated using 2SLS. MSE-optimal bandwidths are calculated using the `rdrobust` package calonico2017STATA.

Table B.4: RKD results for search outcomes, only first week claimants

	(1)	(2)	(3)	(4)
Ln(reservation wage)	0.193	-0.0544	-0.0138	0.0364
	(0.163)	(0.175)	(0.0418)	(0.304)
N	4,594	10,104	9,122	14,150
Bandwidth	1195	2679	2390	4090
Reserve ratio	-0.000117	0.0388	-0.0375	0.0960
	(0.0822)	(0.108)	(0.0357)	(0.195)
N	4,297	10,122	8,564	13,042
Bandwidth	1123	2703	2246	3694
Switching occupation	-0.236	-0.0236	0.0717	-0.203
	(0.178)	(0.195)	(0.0602)	(0.341)
N	4,435	10,375	8,856	13,997
Bandwidth	1127	2684	2253	3904
Contacts	0.466	0.692	0.263	1.574
	(0.290)	(0.805)	(0.148)	(2.127)
N	5,445	8,139	9,373	8,358
Bandwidth	2130	3432	4260	3552
Bandwidth times optimal	1	1	2	1
Polynomial order	1	2	1	3
Kernel	Uniform	Uniform	Uniform	Uniform

Standard errors in parentheses

$p < 0.10$, $p < 0.05$, $p < 0.01$

Note: Standard errors clustered at the benefit schedule level. Each coefficient gives the treatment effect of logged weekly benefit amount on the outcome indicated the leftmost column, estimated using 2SLS. MSE-optimal bandwidths are calculated using the `rdrobust` package calonico2017STATA.

Table B.5: Weeks claimed vs. recall status

	(1)	(2)	(3)
Expecting recall	-1.942	-2.016	-1.739
	(0.313)	(0.310)	(0.328)
Definite recall date	-5.269	-5.300	-5.315
	(0.422)	(0.430)	(0.463)
Month FEs	Yes	Yes	Yes
Demographics	No	Yes	Yes
Industry	No	No	Yes
Last Occupation	No	No	Yes
R-squared	0.266	0.286	0.325
N	2,453	2,453	2,453

Standard errors in parentheses

Note: This table provides validation for the match between BAM and the Missouri claims data. The dependent variable is total weeks of UI received as recorded in the claims data. The strong negative coefficients on indicators for the BAM-derived recall variables suggests that the matching technique is connecting the correct entries.

Table B.6: Cyclicality results

	Full sample		Just first week	
	(1)	(2)	(3)	(4)
Reserve ratio	-0.005	-0.005	-0.007	-0.008
	(0.001)	(0.001)	(0.002)	(0.001)
R-squared	0.026	0.084	0.036	0.386
N	803,200	801,872	71,477	37,868
Log reservation wage	-0.006	-0.005	-0.009	-0.006
	(0.003)	(0.002)	(0.004)	(0.002)
R-squared	0.276	0.653	0.265	0.815
N	812,546	811,199	72,207	38,338
Switching jobs	-0.001	-0.003	-0.001	-0.003
	(0.003)	(0.003)	(0.004)	(0.004)
R-squared	0.028	0.066	0.034	0.411
N	820,642	818,343	72,756	38,878
Job contacts	-0.001	-0.001	0.008	0.018
	(0.031)	(0.031)	(0.032)	(0.031)
R-squared	0.187	0.222	0.200	0.571
N	581,350	580,261	48,508	21,570
Weeks claimed	No	Yes	No	No
Full controls	No	Yes	No	Yes
Month FE	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	No
State x Industry x Occupation	No	No	No	Yes

Standard errors in parentheses

Note: Each point estimate is the coefficient on the unemployment rate (in percent) from a separately run specification. Observations vary across specifications because of missing values in the outcomes.