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THE ECONOMIC CONSEQUENCES OF BEING DENIED AN ABORTION

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ABSTRACT

Restrictions on abortion are pervasive, yet relatively little is known about the financial and economic impact of being denied an abortion on pregnant women who seek one. This paper evaluates the economic consequences of being denied an abortion on the basis of the gestational age of the pregnancy. Our analysis relies on new linkages to administrative credit report data for participants in the Turnaway Study, the first study to collect high-quality, longitudinal data on women receiving or being denied a wanted abortion in the United States. Some women had pregnancies close to the facility's gestational age limit, but below it, and received a wanted abortion (Near Limit Group). A second group of women had pregnancies just over the facility's gestational age limit and were turned away without receiving an abortion (Turnaway Group). We link study participants to ten years of credit report data including several years prior to their recruitment into the study. Using these data, we compare differences in credit report outcomes for the two groups of women over time using an event study design. We find that the trajectories for these outcomes are similar for the two groups of women prior to the abortion encounter. However, following their visit to the abortion provider, we find evidence of a large and persistent increase in financial distress for the women who were denied an abortion that is sustained for several years. Being denied an abortion increases the amount of debt 30 days or more past due by 78 percent and increases negative public records, such as bankruptcies and evictions, by 81 percent. We conduct additional analyses that use a regression discontinuity design to compare outcomes for women just above and just below the gestation limit at each clinic and find results that are consistent with the event study analyses. We explore the mechanisms behind these findings by taking advantage of existing survey data collected for the study participants and compare the effects sizes we document to those experienced by similar women following a "typical" birth. Our results highlight important financial and economic consequences of restrictions on abortion access.

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In the United States, restrictions on access to abortion are pervasive. States enforce a variety of regulations such as banning abortions after a certain gestational age of the pregnancy, requiring women to receive state-written information about abortion, imposing a waiting period to receive an abortion, and requiring parental consent for minors seeking abortion. In addition, some states have detailed requirements on the manner and setting in which an abortion is permitted. These include bans of the "telemedicine" prescribing of abortion medication, requirements that providers have admitting privileges to local hospitals, and detailed structural standards that must be met by the facility providing the abortion (Nash et al., 2013).

These regulations effectively reduce access to abortions by limiting the definition of qualifying pregnancies and reducing the number of available providers. In addition, the prevalence of these restrictions is increasing over time: between 2011 and 2017, 401 such restrictions were enacted, more than double the 189 that had been enacted in the entire preceding decade (Nash et al., 2013, 2018). Recent research shows that these regulations appear to substantially decrease access to and use of abortions.[1]

Despite the prevalence of such restrictions, the impact of these laws on the women for whom these restrictions bind is not well understood. A small number of analyses have used aggregated state-level data to analyze the impact of abortion restrictions on outcomes other than the direct effects on abortions or births, but these analyses tend to focus on child outcomes (e.g. Bitler and Zavodny, 2002, 2004). In addition, the data used in these papers does not include information on which women actually sought, and were denied, an abortion. Since these women represent only a small fraction of the total population in any given year, such studies are limited in their power to detect the effects of abortion restrictions.[2]

This paper uses a novel data source on individual women who sought, but did not all obtain, abortions at facilities across the U.S. to provide new information on the consequences of an abortion denial. It builds on the Turnaway Study, which is a unique source of longitudinal data on women's experiences when denied an abortion due to gestational limits. The study recruited women seeking abortions at 30 different clinics in 21 states. Each of these clinics had the latest abortion gestation

[1] See, e.g. Colman et al. (2011); Fischer et al. (2018); Grossman et al. (2014); Lindo et al. (2019); Quast et al. (2017); Venator and Fletcher (2019) and Section 1 for further discussion.

[2] According to the Centers for Disease Control and Prevention surveillance data, the abortion rate for 2015 was 11.8 abortions per 1,000 women aged 15-44, or 1.18 percent of women of reproductive age. Even when focusing on effects among birth cohorts, the frequency of abortions relative to births is low (188 abortions per 1,000 live births, see Jatlaoui et al., 2018). In addition, it is likely that a much smaller number of women (and births) are affected by abortion restrictions of the type described above.

limit within 150 miles, making it difficult for women to receive abortions elsewhere if denied based on a gestational limit. The study collaborated with participating clinics to enroll women who were above the gestation limit (by up to 3 weeks) for each clinic, and were turned away without receiving the abortion they sought (we refer to these women as the "Turnaway" group). The majority of these women, 68 percent, ended up giving birth; 32 percent either obtained abortions elsewhere or experienced a miscarriage or stillbirth.[3] Other women who were within the gestation limit, but only by up to two weeks, and received a wanted abortion were recruited as a comparison group (the "Near Limit" group). Additionally, the study also recruited women who sought and received abortions early in their pregnancies (during the first 14 weeks) for a point of comparison.

By linking this unique data source to administrative data, this paper provides new evidence on the economic and financial consequences of being denied an abortion. We link women in the Turnaway Study to ten years of credit report data that contain high-quality administratively collected information that may be used to study financial health. These data allow us to observe measures of financial stress such as lateness in paying bills, having bills sent to collection agencies, and serious adverse financial events like evictions and bankruptcies. They also allow us to observe markers of financial self-sufficiency and resiliency, such as taking out a mortgage or having access to a reserve of credit. By using administrative records rather than self-reported information to study these outcomes, we avoid concerns of non-random misreporting of economic outcomes often observed in survey data.[4] At the same time, we benefit from the pathbreaking efforts of the Turnaway Study to identify and recruit this hard to reach population (described in Dobkin et al., 2014).

This new linkage to administrative data offers other important advancements over previous studies using the Turnaway Study data. First, we are able to link the study participants to credit report data that predate the abortion encounter when they were recruited into the study. This allows us to observe the women's trajectories of financial outcomes both before and after the pregnancy, which was not possible in the original study. Using these data, we are able to test the validity of the Near Limit group as an appropriate comparison group for women who were denied an abortion by comparing the evolution of pre-study outcomes for the two groups of women. In addition, having both pre- and post-data on financial well-being allows us to document the *change* in individual well-being resulting from the abortion denial for women in the Turnaway group by including individual fixed

[3]These percentages are based on all women in the Turnaway Study for whom we have follow-up information about the outcome of the pregnancy. For some women, no such follow-up information is available.

[4]For example, in a Turnaway Study of socioeconomic outcomes, Foster et al. (2018) found that women living at home with their parents were less likely to know their household income.

effects in our regression models.

Second, because we only require information collected when women were initially recruited into the study to perform the linkage, we are able to observe outcomes for women who later opted not to participate in survey interviews. While 85 percent of those enrolled in the study completed the first survey, by the end of the 5-year study period only 58 percent responded (Foster et al., 2018). This increases our sample size substantially relative to the original Turnaway Study, and also allows us to document how outcomes change over time across the two groups without the selection concerns that result from survey non-response.

Using an event study design that accounts for any non-time varying differences across the Near Limit and Turnaway groups, we find that, prior to the pregnancy, financial outcomes in the Near Limit and Turnaway groups evolved very similarly. However, around the time of the birth, women in the Turnaway group experienced much higher rates of financial problems, increasing a summary measure of financial distress by approximately one tenth of a standard deviation. We find that abortion denial resulted in increases in the amount of debt 30 days or more past due of $1,750, an increase of 78 percent relative to their pre-birth mean, and in negative "public records" on the credit report such as bankruptcy, evictions, and tax liens, of about 0.07 additional records, or an increase of 81 percent. These effects are persistent over time, with elevated rates of financial distress observed the year of the birth and for the entire 5 subsequent years for which we observe the women. Our point estimates also suggest that being denied an abortion may reduce credit access and self-sufficiency, particularly in the years immediately following the birth, although these estimates are not always statistically significant.

In additional analyses, we take advantage of detailed information on gestational age at the time of the abortion encounter to implement a regression discontinuity (RD) design that compares outcomes for women just above and below the abortion gestation limit at each clinic. As described later, complicating factors affect the timing of when women seek abortions; for instance, the rules around gestation limits are often opaque and women may be unaware of both the cutoffs and the precise dating of their pregnancy. This suggests that women who seek abortions just above and below gestation limits might be otherwise very similar. In support of this, we find no evidence of discontinuities in observable characteristics, or for our outcomes of interest prior to the abortion encounter, at the gestational cutoff. We do, however, find results consistent with the event study design: we observe an increase in financial distress following the birth for those women who just met the criteria for an abortion denial. These effects are largest during the year of the birth and the following three years, although the confidence

intervals for these estimates tend to be large due to their reliance on a small number of data points around the gestational cutoff.

We also conduct exploratory analyses that use the longitudinal Turnaway Study survey data to better understand the mechanisms underlying these financial effects, drawing on survey outcomes originally published in Foster et al. (2018). The Turnaway Study recruited participants at the time of the abortion encounter, so pre-encounter survey data are unavailable. However, we are able to examine changes in survey outcomes over time from the initial survey interview, which occurred approximately one week after the abortion encounter. We implement a difference-in-differences analysis using these data for our specific study sample. We find that women in the Turnaway group often cared for an additional child without experiencing an increase in personal or household income. As a result, their income as a percent of the Federal Poverty Level (FPL) fell over time relative to the Near Limit group. Women in the Turnaway group did not appear to move in with male partners and were less likely to live with adult family (such as parents or grandparents) or roommates after the initial interview compared to the Near Limit group; instead, they experienced increases in the incidence of living alone with a child or children. While we find evidence that public support (via the WIC program) increased for the Turnaway relative to Near Limit group in the first year following the birth, we do not find significant increases in receipt of other public benefits and only marginally significant, modest increases in the amount of child support received. In sum, our analysis of the survey data is consistent with abortion denials resulting in greater parental obligations for women without obvious corresponding increases in support from the government, family members, or male partners, providing some context for the higher financial stress documented in our analysis.

We also explore how the financial stress for women in the Turnaway group who give birth compares to the experience following a "typical" birth among women of similar socioeconomic status. We do this by examining how financial outcomes change among women in the Near Limit group who obtained an abortion but went on to give birth later in the study period. We find evidence suggesting that the financial distress associated with a new baby is greater for women who were denied an abortion and carried an unwanted pregnancy to term than what may generally be experienced by women with similar socioeconomic characteristics after a birth. This analysis suggests that births occurring after an abortion denial carry additional economic penalties over and above what is typically experienced by disadvantaged women when they have a new child, and that greater access to abortion services and more optimally timed births may result in fewer adverse economic consequences.

Together these analyses offer strong evidence that being denied an abortion has large and persistent negative effects on measures of financial well-being. These results highlight important financial and economic consequences of restrictions on abortion access.

1 Background

While abortions are infrequent events when compared to births (e.g. there were 188 abortions per 1,000 live births in 2015, see Jatlaoui et al., 2018), they are not infrequent when observed over the full length of a woman's reproductive years. Projections based on the current abortion rate estimate that nearly 1 in 4 women in the U.S. is expected to have an abortion during her reproductive years (Jones and Jerman, 2017).[5] The majority of abortions are surgical procedures (73.1 percent in 2015), although just over one-quarter (26.8 percent) are nonsurgical abortions that occur at up to 10 weeks of gestation (Jatlaoui et al., 2018). Nonsurgical abortions use medications to terminate the pregnancy, but require that a woman know that she is pregnant and receive abortion services early in her pregnancy.[6]

1.1 Abortion Restrictions and the Women Affected

Women seeking abortions, and abortion providers, face a variety of restrictions in most states. Until recently, most restrictions involved who could receive an abortion or introduced additional requirements for women seeking abortion, such as mandatory waiting periods, counseling, or parental notification and consent laws for minors.[7] While there have been a number of studies examining the effects of mandatory waiting periods or additional barriers to receipt for minors,[8] there has been less attention paid to the consequences of gestational limits for the provision of abortion services.

Yet, gestational limits are among the most prevalent restriction for women seeking abortion ser-

[5] Based on data on receipt at abortion clinics, this is likely an underestimate since it does not include abortions obtained outside of the formal medical system (Foster, 2017).

[6] For context, gestation length is measured from the date of women's last menstrual period with an average menstrual cycle length of 28 days, but ranging from 21 to 45 days. Therefore, women may be unaware that they are pregnant until the 6th week of pregnancy or much later if they do not experience symptoms.

[7] As policies regulating providers have become more common (i.e."supply-side" regulations), a new literature examines the consequences for access to abortion providers and abortion receipt. These papers have primarily focused on the introduction of a series of stringent regulations for abortion providers in Texas and document sizeable decreases in abortion rates (see Colman et al., 2011; Fischer et al., 2018; Grossman et al., 2014; Lindo et al., 2019; Quast et al., 2017). More recently, Venator and Fletcher (2019) examine the closure of clinics in Wisconsin under increased provider regulation and find fewer abortions and increased birth rates. Kelly (2019) studies reduced clinic capacity following new regulations for abortion providers in Pennsylvania and finds evidence of delays in abortion receipt as a result. In some cases, the law changes being studied also include restrictions on the patients seeking abortions (i.e. "demand-side" regulations) but the papers in this literature often focus on the supply-side components.

[8] See, for example Bitler and Zavodny (2001); Blank et al. (1996); Colman et al. (2008); Colman and Joyce (2009); Girma and Paton (2013); Joyce and Kaestner (1996, 2000, 2001); Joyce et al. (2006); Joyce (2010); Levine (2003), for studies of the effects of the restrictions on abortion timing and receipt and birth rates. A related strand of literature has examined the effects of changes in public funding for abortion and finds decreased abortion rates under more restrictive funding (see e.g. Blank et al., 1996; Levine et al., 1996).

vices; 43 states have gestational limits in place that ban abortions for most women after a certain point in their pregnancy (Kaiser Family Foundation, 2019). Twenty states ban abortion at the point of fetus "viability," which is typically understood to range from 22 to 24 gestation weeks and is determined on an individual basis at the discretion of the patient's physician. In contrast, another 20 states have adopted bans that specify limits from 20 to 24 weeks. An additional 13 states have attempted to ban abortion earlier in pregnancy or at any time during pregnancy, but these changes have been stopped by court order (Guttmacher Institute, 2019).[9] In addition, each abortion facility sets its own gestational limits based on a variety of factors, including physician training and staff comfort (Foster et al., 2013). This can make it difficult for a woman seeking a later abortion to locate a provider willing to perform the service.

Descriptive studies document that the women most likely to be affected by these types of bans are a particularly vulnerable population. A survey of women in Georgia who had an abortion at or after 20 weeks just prior to implementation of a 20-week abortion ban found that over one-half were black, more than three-fourths were single, and most did not have education beyond high school (Roberts et al., 2015). In the Turnaway Study, the majority of the women presenting for an abortion with gestational ages close to facility gestational limits had incomes below the poverty line and reported that they did not have enough money to make ends meet (Foster et al., 2018).

Although the reasons why women seek abortions are complex, interviews with abortion seekers often find that financial or material concerns and timing are among the most important considerations, with many women indicating that having a child would interfere with their education or livelihood (e.g. Biggs et al., 2017; Finer et al., 2005). In addition, women with lower levels of education, and perhaps more financial difficulties, are more likely to seek an abortion later in the pregnancy (Jones and Finer, 2012). While a number of factors are associated with delays in seeking abortion, the most common reasons given are later recognition of pregnancy and the amount of time needed to decide and make arrangements for the abortion (Finer et al., 2006).[10] Women receiving abortions during the second trimester report a logistical reason (such as difficulty finding an abortion provider or referral to another clinic) as the primary factor that caused their delay (Drey et al., 2006); they are also more

[9] All of these gestation lengths are in terms of weeks since the date of the women's last menstrual period. In addition, there are exceptions for the life and health of the woman, also at the discretion of the patient's physician. In addition to the state rules described here, two other states have attempted to ban abortion at 20 weeks and 22 weeks, but these changes have also been stopped. See additional details in Guttmacher Institute (2019).

[10] Nearly half of pregnancies in the U.S. are unplanned (Finer and Zolna, 2016); therefore, not knowing one is pregnant is a common experience among women with irregular periods, those who do not have pregnancy symptoms, and those who have health conditions that mask pregnancy such as having recently given birth (Drey et al., 2006).

likely to have concerns about raising the money to cover the cost of the abortion (Finer et al., 2006).

Finally, informational barriers may also contribute to delays in seeking care. Evidence indicates that women are often not aware of clinic gestational limits for abortion or may be confused about limits (e.g. Assifi et al., 2016; Lara et al., 2015). This may be exacerbated in states with laws on the books that are not currently enforced due to ongoing litigation (Tavernise, 2019). Furthermore, women often do not have accurate dating information regarding their pregnancy since ultrasound methods are used by providers to determine gestation length.

1.2 Existing Evidence on Economic Effects of Abortion Policy

Being denied a wanted abortion has the potential to impact the economic and financial security of individuals in numerous ways. If being denied an abortion results in a woman delaying an abortion she later obtains, the cost of that abortion may be higher. In some cases, the cost differential between abortions obtained earlier versus later in the pregnancy can be substantial (Lindo and Pineda-Torres, 2019).[11] If the woman is not able to obtain an abortion, she faces additional medical costs associated with prenatal care, birth, and postpartum recovery, as well as potential lost wages for time missed from work. There is also a well-documented large and persistent decline in earnings that women experience on average following the birth of a child (e.g. Adda et al., 2017; Agüero and Marks, 2008; Kleven et al., 2019; Sandler and Szembrot, 2019), in addition to the many other costs associated with child-rearing. While social supports may offset some of these expenses, it may still be the case that denying a woman access to a wanted abortion could have large, negative, and long-lived effects on her financial and economic well-being.

Very few studies have examined the economic consequences for women who are denied an abortion or who carry an unwanted pregnancy to term. An older literature focused on abortion legalization in the 1970s suggests that access to abortion may have important effects on women's human capital attainment and economic outcomes. Angrist and Evans (2000) document increased rates of high school graduation, college attendance, and employment for black women under state laws increasing abortion access during this time period. In addition, a number of studies examine changes in childhood living circumstances for the children born following abortion legalization. For instance, Gruber et al. (1999) find that the children born after legalization lived in better economic conditions; for instance,

[11] The median cost of an early medication abortion in 2011-2012 was $500 and a 10-week abortion procedure was $495 (Jerman and Jones, 2014). Later first trimester and second trimester abortion procedures can range from $500 to $3,000 or more (Cowles, 2018). In addition, later abortions require a longer period for the procedure to be performed and for recovery, which may lead to additional expenses in the form of time off of work or child care. There may also be travel-related expenses depending on the location of the provider.

they were less likely to live in poverty or receive cash welfare.[12] This finding indicates that fewer disadvantaged women were selecting into parenthood following abortion legalization, but does not provide any information on how this change affected their economic outcomes or career trajectories.[13]

A large body of evidence from studies of expanded access to contraception also indicates that there may be important consequences for women's outcomes.[14] Many of these studies examine the effects of increased legal access to the birth control pill in the 1960s and 1970s and document delayed marriage and increased educational attainment, employment, and earnings among young women as a result (e.g. Bailey, 2006; Bailey et al., 2012; Goldin and Katz, 2002; Hock, 2008). However, more recent work by Myers (2017) indicates that it might be the legalization of abortion, rather than access to the pill, driving the findings in these studies.[15] A recent survey of the evidence suggests that the mixed findings on the magnitude of the pill's effects may be due, at least in part, to difficulty defining state policies that were enforced during the period of study and differing interpretations across researchers (Bailey and Lindo, 2017).

Part of the challenge with this prior body of work and its interpretation is that it estimates the effects of changes in access to contraception or abortion services under federal or state policies among women who are expected to be affected (e.g. women of reproductive age). However, these studies are unable to identify the individual women who are actually denied an abortion or whose use of contraception changes as a result of these policies. Thus, the authors must rely on cohort-level changes in women's outcomes that are associated with the particular policy change or diffusion of birth control technology being studied. Given that different policy or access changes that affect a woman's use of reproductive health services can happen simultaneously,[16] and that only a relatively small share of

[12] Ananat et al. (2009) follow these cohorts into young adulthood and show further evidence of improved outcomes. In addition, Bitler and Zavodny (2002, 2004) find evidence of decreased child maltreatment following abortion legalization. Donohue and Levitt (2001), Joyce (2004), and Joyce (2009) examine the question of whether the change in the composition of births following abortion legalization led to lower rates of crime. More recently, Sun (2019) focuses on the older siblings born just prior to abortion legalization who then experienced smaller family sizes in areas with abortion service roll-out. He finds evidence of better living circumstances and significant improvements in the long-term outcomes for these children, with gains in both human capital attainment and economic self-sufficiency as adults.

[13] However, follow up work has examined the effects on fertility over the lifespan, indicating that the reduction in births was permanent for many women (i.e. they remained childless and did not just delay childbearing, see Ananat et al., 2007).

[14] Another relevant literature focuses on the effects of motherhood timing on women's human capital and career outcomes. A number of these papers find that fertility delay for teenagers (e.g. Ashcraft et al., 2013; Fletcher and Wolfe, 2009; Klepinger et al., 1999; Schulkind and Sandler, 2019) and women in their 20s and 30s (Miller, 2011) improves education or labor market outcomes, although the estimates are sometimes modest in size.

[15] Joyce (2013) also argues that the legalization of abortion is a potentially important confounder in this literature, although Bailey et al. (2013) discusses how findings tied to increased access to the pill in Bailey et al. (2012) and Bailey (2006) are robust to analyses that address this criticism.

[16] Examples of this include abortion legalization and state liberalization of access to the birth control pill (Joyce, 2013); changes in multiple types of state abortion restrictions including Medicaid funding restrictions, parental involvement laws and mandatory delay laws (Bitler and Zavodny, 2001); and, the enactment of new state regulations of abortion providers

the female population may actually be affected, this can make it difficult to pinpoint effects that occur at the individual level.

1.3 Evidence from the Turnaway Study

The Turnaway Study offers a unique opportunity to overcome these data challenges. Data from this study allows researchers to follow women who are actually seeking abortions, allowing a direct examination of the relevant women rather than relying on comparisons across broad groups such as cohorts or states. The focus of the study was women who were denied an abortion due to facility gestational limits (Turnaway group). In addition to successfully identifying and recruiting these women to participate, another innovation of the study was the construction of a suitable comparison group by recruiting women seeking and receiving abortions at gestational lengths just below facility limits (Near Limit group). Data on a wide range of outcomes were collected for study participants one week after the abortion encounter and then every six months over a five-year period.

Using the survey data, the study team documented important differences in the well-being of women in the Turnaway group compared to the Near Limit group, many of which persisted over the study period. This body of work finds that women who were turned away by the abortion clinics experienced worse mental health in the short-run (Biggs et al., 2017); poorer physical health among those who gave birth, including two maternal deaths (Gerdts et al., 2016; Ralph et al., 2019) and increased risk of physical violence from the man involved in the pregnancy (Roberts et al., 2014), when compared to women in the Near Limit group who received abortions. Researchers also documented worse economic outcomes following the abortion denial for women in the Turnaway group, including higher rates of poverty, lower employment, and greater use of public assistance both in the short-term (6 months following the service denial) and over a longer time horizon (4 years later) (Foster et al., 2018). We present a more detailed overview of the Turnaway Study in Miller et al. (2020).

These studies provide some of the first evidence on the potential short- and long-term economic, health, and social consequences of being denied a wanted abortion. However, without information on the relative trajectories of these outcomes prior to the abortion encounter, an inherent limitation of the Turnaway Study is the inability to rule out pre-existing differential trends across the two groups of women. There were also initial differences between the Turnaway and Near Limit study participants at the time of the abortion encounter: women in the Turnaway group were slightly younger, less likely

coupled with cuts in public funding for family planning services, as recently seen in Texas (Fischer et al., 2018) and Wisconsin (Venator and Fletcher, 2019).

to be employed, and had fewer children. They also discovered their pregnancies at later gestational ages than women in the Near Limit group (Upadhyay et al., 2014). While the analyses described above adjusted for these observed differences, such differences could be related to unobserved differences in characteristics, such as family or partner support, which affect outcomes related to health and well-being and cannot be easily controlled for. In addition, these analyses were necessarily limited to an increasingly small and selected subgroup of participants who responded to the Turnaway Study's follow-up surveys over time.

Our study builds on these initial results by linking Turnaway Study participants to high-quality administrative, longitudinal data that includes information on financial outcomes from even prior to the pregnancy. These linkages allow us to assess the validity of using women from the Near Limit group as a comparison for the Turnaway group by examining whether outcomes evolved similarly for the two groups prior to the abortion encounter, which was not possible with the original study data. In addition, we are able to trace out the impacts of an abortion denial without relying on a selected sample of survey respondents. In sum, the Turnaway Study provided an important first look at the economic consequences of an abortion denial. By linking this pathbreaking study of a difficult-to-reach population with large-scale administrative data, we are able to further strengthen our knowledge base on this understudied question.

2 Data and Outcomes

2.1 Data and Sample Construction

Our analysis relies on a new source of data that links individual-level information from the Turnaway Study to longitudinal credit report data. The Turnaway Study recruited women seeking abortions in 30 abortion facilities across the United States between 2008 and 2010 in order to survey them about their experiences. Facilities with the latest abortion gestational limit (i.e. highest number of gestation weeks at which an abortion could be performed) within 150 miles were selected to partner with the Turnaway research team to recruit women to participate in the study. The research team sought to represent many different geographic areas in the country, while prioritizing locations with earlier gestation limits (see further details in Dobkin et al., 2014). The clinics' gestational limits ranged from 10 weeks to the end of the second trimester of pregnancy (the end of week 26), with most falling during the second trimester (weeks 14-26).

The study recruited women age 15 and older whose pregnancies exhibited no known fetal anoma-

lies and who spoke either English or Spanish.[17] If the gestational age of the pregnancy, as measured by an ultrasound, was no more than two weeks below the gestational age limit of the clinic, these women were considered part of the Near Limit group. If the gestational age was up to three weeks above the gestational age limit of the clinic, such that they were not permitted to obtain the abortion, the women were considered to be part of the Turnaway group. There were 536 and 292 women in each of these groups, respectively. Figure 1 shows the distribution of the gestational ages of the pregnancy for the Turnaway group who were denied an abortion (dark blue) and at the time of the abortion for the Near Limit group (light blue).[18] The distribution of gestational ages for the Near Limit group overlaps significantly with the Turnaway group, although it is shifted to the left; i.e., on average, the Near Limit group is seeking abortions at earlier points in the pregnancy than the Turnaway group.

While all women in the Near Limit group obtained an abortion, the converse is not true for the Turnaway group. Through the Turnaway Study surveys, we observe follow-up information regarding the outcome of the pregnancy for 217 of the 292 women in our Turnaway group sample.[19] Among these 217 women, 32 percent reported either obtaining an abortion elsewhere or experiencing a miscarriage or stillbirth. The remaining 68 percent carried the pregnancy to term. In our analysis, we examine the impact of the abortion denial for all women in the Turnaway group, regardless of the outcome of the pregnancy.[20]

To conduct our analysis, we estimate how outcomes change over time for women in the Turnaway group relative to the Near Limit group. We do this by defining a variable "event time" to capture the number of years relative to the time a woman gave birth (for those in the Turnaway group who gave birth) or would have given birth (for those in the Near Limit group or those in the Turnaway group who had an abortion, miscarried, or whose pregnancy outcome is unobserved). For example, a woman in the Turnaway group who gave birth after service denial would have event time equal to -1 in the 12 months preceding the birth, event time=0 during the month of the birth and the next 11 months, event time=1 in the 12 months after that, etc. Similarly, for those in the Turnaway group

[17] One woman whose home address was outside of the United States was excluded from our analysis.

[18] This histogram shows the values for women in the main sample used in this paper, who were successfully matched to credit records and meet the sample criteria described in this Section.

[19] Women for whom we do not have information about the pregnancy outcome include 61 women who did not complete the initial Turnaway Study survey, as well as 14 women who were pregnant when they responded to the initial survey but did not respond to any subsequent surveys.

[20] In addition to presenting average effects for the Turnaway group, a previous version of this paper also presented implied treatment effects for women who carried their pregnancies to term. We have removed this analysis due to the strong underlying assumption that only women who carry their pregnancies to term may be affected by an abortion denial. We still provide some discussion of this type of interpretation in our results section, however.

who had an abortion, miscarried, or had an unknown pregnancy outcome, and for those in the Near Limit group, we define event time relative to the year in which they would have given birth on the basis of the gestational age of the pregnancy, assuming a 40 week pregnancy. That is, event time=0 in the month they would have given birth and the following 11 months, event time=1 in the 12 months following, etc.[21] For simplicity, throughout the manuscript we refer to event time=0 as the birth year.

We link study participants to annual Experian credit report data for 2006 through 2016. This range allows us to see event times up to 3 years prior to the birth (or counterfactual birth), the year of the birth, and five years after the birth year for all participants.[22] To link to the Experian database, we used a "double blind" matching method that masked actual participants in the Turnaway Study by including approximately 50,000 randomly-selected women between the ages of 15 and 44, purchased from a marketing firm, in the data file sent to Experian to be matched. This "masking" sample prevented analysts at Experian from identifying who in our data actually sought abortions, providing an additional layer of data security to Turnaway Study participants.

The Experian credit report data include records for all individuals with any credit line, public record, or third party collection reported to the credit reporting agency; however, not all Turnaway Study participants were matched to the data. There are a couple of reasons why this might occur. The first is if the individual has never opened a credit account or had a collection or debt-related court record in their name. Since this is most likely to be true for dependents, we only include study participants who were at least 20 years old in event period zero, although we show that the results are robust to including women of all ages.

A second reason that a match may not occur is if the linking variables are not sufficient to establish a match. We matched study participants based on name, year of birth, and address.[23] However, if the credit reporting agency does not have the address provided by the woman on file (e.g., if she provided a temporary address to the study that was not included in her Experian address history), we may not successfully match her to the Experian data even if she does have a credit record. About 82.0 percent of women in the Near Limit group were successfully matched to a credit record in at least one year, higher than the 76.3 percent of the Turnaway group who were matched in at least one year. Both

[21] We could also use the 40-week rule to define the birth year for those in the Turnaway group who gave birth. We choose, however, to use the actual birth year since we have this information.

[22] Since we only observe earlier event times for a small number of women who enrolled in the study at the end of the study period, and later event times for a small number of women who were first to enroll in the study, we exclude event periods outside of this range from our analysis.

[23] Note that Experian keeps records on previously used names and aliases, so name changes due to marriage should not in principle be an impediment to successfully matching a woman to Experian's database.

match rates were slightly lower than those in the random "masking" sample of women who were also age 20 and older, of whom 84.7 percent were matched. In general, these match rates are comparable or better than those in other studies that have matched to credit reports using name and address (e.g. Finkelstein et al., 2012; Humphries et al., 2019), but lower than those generally reported in studies that included social security number as a match variable (e.g. Miller and Soo, 2018; Miller et al., 2018).

We are able to use survey data collected by the original Turnaway Study to better understand who was successfully matched to the credit database. We observe that 83 percent (483) of the 581 matched participants, and 77 percent (110) of the 143 unmatched participants, completed an initial telephone survey as part of the study. These surveys were completed approximately one week after having sought the abortion. Within both the Turnaway and the Near Limit groups, those who were not matched to credit records tended to be somewhat more likely to have lower levels of educational attainment. Within the Turnaway group, those not matched were more likely to be in poverty and employed part time, were older, and were more likely to receive Food Stamps (see Table 1). Since our analyses necessarily only include individuals that are successfully linked to the credit report data, this suggests that the effects of an abortion denial that we estimate may not necessarily represent the experiences of the most disadvantaged members of the Turnaway group. For instance, it is possible that the economic consequences for the unmatched group might be even larger given their higher rates of disadvantage reported in the initial Turnaway Study survey.

Match rates for both the Turnaway and the Near Limit groups also increased over time as participants aged (see Appendix Figure A1). This is consistent with a general trend of the establishment of credit at older ages. However, when we examine differential match rates for the two groups in event time, we observe that match rates tend to increase in the Turnaway group relative to the Near Limit group in the post-period (see Appendix Figure A2). Events that could lead to a higher match rate include establishment of credit (i.e. open credit account), a creditor reporting delinquency on bills, or a public record event, such as an eviction, bankruptcy, or court judgement (e.g. being ordered to pay child support or having wages garnished by a creditor). In order to isolate the impact of the abortion denial from changes in the matched sample composition over the study period, we limit the analytic sample to women who had a record with the credit agency prior to the birth year.[24]

Our estimates of the effects of an abortion denial will therefore be limited to the effects among women who already had credit records. This may miss important effects on women who are not in this

[24]The flow chart in Appendix Figure A3 demonstrates how each of these sample inclusion criteria affects our final sample size.

sample, however. To further explore this, in sensitivity analyses, we conduct alternative analyses in which we either assume that women without credit records in the pre-period have zero delinquencies or credit cards, or we impute group-specific average values for the missing outcomes. Our results are very similar under both of these alternative approaches.

2.2 Credit Report Outcomes

The credit report data contain information on a wide range of outcomes related to a consumer's financial well-being and creditworthiness. We focus on four outcomes that indicate financial problems and four outcomes that suggest financial independence or access to credit. In the first category, we include the amount of debt sent to a third party collection agency. This debt includes unpaid medical or utility bills, or severely delinquent credit card debt that has been sold to a third party. In addition to the debt in collections, we also examine debt that is 30 days or more past due on open accounts. This is debt that is delinquent but has not yet been sold to a third party, and would include, for example, unpaid credit card bills. We next include the number of "public records" from courts as a measure of serious financial distress. These records include any incident in which a credit interaction required an intervention via the court system–including actions such as bankruptcies, tax liens, or evictions. Finally, we include an indicator that a participant has a credit score at or below 600, which is considered "subprime."[25]

In the second category, measuring access to credit, we include the total amount of credit available on all credit cards (i.e., how much credit remains usable on all of the consumer's cards before hitting the cards' limits). More credit available indicates a greater cushion in case the consumer is faced with an unexpected expense. We also include an indicator that the consumer has a mortgage and an indicator that her credit score is in the "prime" (greater than 660) category.[26] Finally, we include the credit score itself in this category.

In order to improve power, we combine these outcomes into two broad indices, a "financial distress index" and a "credit access index" by subtracting from each individual's value the mean of that outcome observed in the Near Limit group and dividing by the standard deviation of that outcome in the Near Limit group. We then average these standardized values across all non-missing outcomes. This gives us a summary measure that we can use to test whether the entire category of outcomes was affected.

[25] We use the Vantage score to measure credit score, which is similar to the FICO credit score and is used by all three major credit reporting agencies.

[26] Note that 601 to 660 is considered "near prime," see Experian (2015).

Table 2 presents summary statistics for the Near Limit and Turnaway groups at baseline, prior to the (counterfactual) birth year. We also show the same statistics for the women recruited into the study who received an abortion during the first trimester, for the purpose of comparison. We report the mean, standard deviations, and median values of these baseline characteristics. The mean values tend to be higher than the median values and in some cases they are substantially higher. This is due to the skewed distribution of financial data, in which a small number of individuals have very high levels of debt and delinquencies. Most financial characteristics are not significantly different across the Near Limit and the Turnaway groups in the pre-period. The only exception is that women in the Turnaway group are significantly less likely to have a mortgage. In contrast, both the Turnaway and Near Limit groups have higher amounts of debt in third party collections, lower credit scores, less credit available (on average), are less likely to have a mortgage (on average), and more likely to have a "subprime" credit score (on average) when compared to the First Trimester group.

3 Empirical approach

For our main analysis, we examine how outcomes change over time for women in the Turnaway group relative to the Near Limit group using the "event time" variable that measures the number of years since the birth (for those in the Turnaway group who gave birth) or the number of years since the woman would have given birth (for those in the Near Limit group, and those in the Turnaway group who obtained abortions elsewhere, miscarried, or whose pregnancy outcome is unobserved). Event time is equal to zero in the year of the birth or counterfactual birth. Since our credit data are observed from 2006 to 2016, and since Turnaway participants were mostly enrolled in 2009 and 2010, we observe at least 3 years of pre-birth outcomes, outcomes during the year of the birth, and at least 5 years of post-birth outcomes for all participants. We use this 9 year period over which we observe outcomes for all participants in our analysis.

Figure 2 plots our primary outcome variables – the financial distress index (panel a) and the credit access index (panel b) – by this event time measure. Similar plots for the components of these indices may be found in Appendix Figures A4 and A5. Prior to the birth, women in the Turnaway and Near Limit groups had very similar outcomes related to financial distress. These outcomes diverge beginning in the year of the birth, with an increase in financial problems observed among women in the Turnaway group and fairly stable outcomes in the Near Limit group. We also see similar trends in access to credit across the two groups (panel b), with a relative decrease in access to credit for the

Turnaway group occurring around the time of the birth. This difference in access to credit, however, appears to close after three years.

We formally test for the patterns presented in Figure 2 using an event study model that compares changes in financial outcomes for the Near Limit group to changes in the same outcomes in the Turnaway group before and after the birth or counterfactual birth. We estimate the regression

$$Y_{it} = \sum_{\substack{y=-3 \\ y\neq -1}}^{5} \beta_y Turnaway_i \times I(t - t_i^* = y) + \gamma_y I(t - t_i^* = y) + \delta_i + \epsilon_{it}. \quad (1)$$

In this model, we include event time indicators, $I(t - t_i^* = y)$, that denote time relative to the birth, t_i^*, for each individual i. Our estimates of interest are the coefficients on interaction terms for these event time indicators and an indicator that the participant was in the Turnaway group. These estimated coefficients, $\hat{\beta}_y$, measure the change in the relative outcome in year y for the Turnaway group compared to the Near Limit group. The year immediately preceding the birth or counterfactual birth, $y = -1$, is the reference year. We include individual fixed effects (δ_i) in the model and robust standard errors are clustered at the individual level.[27]

Ideally, the estimated coefficients $\hat{\beta}_y$ would be close to 0 for event years preceding the birth, and then diverge only after the birth if there were effects of the abortion denial. This would imply that the outcomes for the Turnaway and Near Limit groups evolved similarly prior to the birth and lend credence to the assumption that the trajectory of outcomes for the two groups would have been similar in the absence of the abortion denial. Note that any fixed (or level) differences in outcomes between the two groups are controlled for with the individual fixed effect.

We also estimate a differences-in-differences (DD) version of model (1) that replaces the event time indicators with a single post dummy for the year of birth and all years after. The effect of the abortion denial for the Turnaway group is estimated from an interaction of this post dummy and an indicator variable for the Turnaway group, providing a summary measure of the impact of abortion denial in all of the post-birth years:

$$Y_{it} = \beta_{DD} Turnaway_i \times Post_t + \beta_2 Post_t + \delta_i + \epsilon_{it}. \quad (2)$$

The estimated coefficient $\hat{\beta}_{DD}$ captures the average change in outcomes for the Turnaway group rela-

[27] In our robustness section, we also conduct inference clustering at the level of the clinic.

tive to the Near Limit group after the abortion encounter.

In addition to the main models described above, we also present results from several alternative sample definitions and specifications. First, we conduct additional analyses in which we include individuals even if they did not have a match prior to the birth year. We include these observations by re-defining our dependent variable to assume that women who do not match to credit records in the pre-period have zero delinquencies, no mortgage, and no credit cards in these unmatched years. We then estimate the impact of abortion denial for this larger sample. Second, rather than assume that individuals without credit report matches have no financial activity in these years, we impute the average value for their group (Turnaway or Near Limit) for each outcome and year. Third, we include those who were under age 20 during the birth year and re-estimate our model using women of all ages. In addition to these alternative sample and variable definitions, we also conduct inference in our main analysis in an alternative way where we cluster our robust standard errors at the level of the clinic, rather than at the individual level. Finally, we also examine whether allowing for a differential pre-trend for the Turnaway group affects the results in our event study model, following e.g. Gross et al. (2020). We implement this by including a linear term for the number of years relative to the birth/counterfactual birth for the Turnaway group that equals 0 in all years for the Near Limit group. We then estimate our event study model (1) including this term but omitting the pre-birth event study coefficients.

4 Results

We report the event study coefficients, $\hat{\beta}_y$ of equation (1), for the delinquency and access indices in Figure 3. The first graph (a) shows the effects of being denied an abortion on the financial distress index, which combines all delinquency outcomes into a single summary measure. Prior to the birth, outcomes of the Turnaway group and the Near Limit group had similar trajectories, as evidenced by the statistically insignificant coefficients on the event study indicators in years -3 and -2 (-1 is the reference period and set to zero). Beginning in the year of the birth, however, we see a significant increase in markers of financial distress in the Turnaway group relative to the Near Limit group of between 0.10 and 0.20 standard deviations. Financial distress remains significantly elevated in the Turnaway group for four years; in years 4 and 5 after the birth year, we observe positive coefficients on the event year variables, indicating that financial delinquencies remained high, but the estimates are only significant at the 10 percent level.

Graph (b) shows the effect of abortion denial on measures related to access and use of credit. The coefficients on event years immediately following the birth are negative, indicating worse access for the Turnaway group, but are not statistically significant. Starting three years after the birth year, the coefficients are close to zero, indicating no difference between the Turnaway and Near Limit group.

Similar event study figures for the components of these indices are found in Appendix Figures A6 and A7. In terms of financial distress measures (Appendix Figure A6), the post-birth coefficients are consistently positive for the subprime credit score, the amount past due, and the number of public records, although the individual year coefficients are only statistically significant for public records (years 0-4) and amount past due (year 3). The amount of debt at third party collection agencies follows a less clear pattern, with positive coefficients in the years immediately following the birth, but coefficients close to zero starting in year 3; none of these coefficients are statistically significant.

Among individual components of the access index (Appendix Figure A7), we see no evidence of an effect on the probability of having a mortgage or credit available, but a decrease in credit scores; the Turnaway group is significantly less likely to be in the "prime" credit score range one and two years following the birth. Differences in this measure of creditworthiness appear to close by the third year after the birth year.

We present the DD estimates of equation (2) in Table 3. The first row presents estimates from our main model for outcomes related to financial distress and delinquency, while row two presents results for measures related to access to credit and financial self sufficiency. In the first column of Table 3, we see that outcomes related to financial distress increase by about one tenth of a standard deviation among the Turnaway group in the post period, as compared to the Near Limit group. This effect is statistically significant at the 5 percent level.

If we assume that the effect is driven entirely by women who subsequently give birth, we can re-scale this estimate by the fraction of women in the Turnaway group who gave birth. Among those with known pregnancy outcomes, this fraction is 68 percent in our sample. This scaling would therefore imply that financial delinquencies increased by 0.15 standard deviations among those who gave birth as a result of the abortion denial (0.102/0.68). However, as discussed earlier, there are reasons to expect financial repercussions even among those women who did not give birth, such as higher expenses related to finding an abortion elsewhere, time missed from work, or productivity losses. For this reason, we focus our attention on the reduced form estimates measured for all Turnaway group members.

We examine the components of the index in the subsequent columns. We find that the Turnaway group experienced significant increases in the amount of debt 30 days or more past due of $1,749.7, a 78 percent increase relative to their pre-pregnancy mean. The number of public records, such as bankruptcies, evictions, and court judgements, significantly increases in the Turnaway group by 0.065, or 81 percent. We observe positive effects of being denied an abortion on the probability of having a subprime credit score and the amount in collections, although these effects are not statistically significant.

We present our estimates related to access to credit in the second row of Table 3. Although we observe a negative point estimate for our overall DD coefficient, suggesting that the Turnaway group experienced decreases in credit access relative to the Near Limit group following the birth, it is not statistically significant. We also do not find any statistically significant estimates of abortion denial on the components of the access index under the DD model.

4.1 Sensitivity Analyses

To assess the robustness of our results to alternative modeling, sample, and variable definitions, we present several additional estimates in Tables 4 and 5. In the first row of Table 4, we assume that women who were not matched to the credit reports have no delinquencies (i.e., have $0 past due and in collections and no public records) in our analyses of financial distress measures. This allows us to include the women with no credit report records during the pre-period but observed records in the post-period in our analysis. With this re-coding, we observe a statistically significant increase in financial distress of 0.117 standard deviations. We continue to find a significant increase in the amount of debt 30 days or more past due and an identical estimate for the increase in the number of public records.

In row 2, we impute the missing values for all participants who had at least one year matched to the credit records using the average amount observed in each year for members of their group. When we impute missing values in this way, we continue to see significant increases in indicators of financial distress of about 0.11 standard deviations, increases in the amount past due of $1,521, and in public records of 0.058. These analyses (rows 1 and 2) indicate that even with different assumptions and treatment of missing credit report records during the pre-period, the estimated financial effects of an abortion denial are robust.

In row 3, we show the estimates using women of all ages, rather than only including those who

were at least 20 years old at the time of the birth. This sample restriction was applied to avoid including the selected group of individuals who were teenagers in the pre-period and thus less likely to appear in credit report data (see Appendix Figure A1). The results using women of all ages are very similar to those reported for the main sample.

Finally, in row 4, we re-estimate our main model but cluster our estimates at the clinic, rather than individual level, to account for any correlation of the error terms between women visiting the same clinic. Our inference is essentially unchanged by this alternative level of clustering. To summarize, across multiple samples and variable definitions and using an alternative approach to inference, we find strong evidence that being denied an abortion had large effects on markers of financial distress, amount of debt past due, and adverse court records.

Table 5 presents the same robustness checks as those in Table 4 but for our measures related to access. In the first row, we assume that women unmatched in the pre-period have no mortgage and $0 in available credit during these years, and calculate the access index using these components only (i.e., we still allow credit score to be missing). Row 2 imputes missing values with group-year averages for women who do not match in the pre-period, but who match in later years. Rows 3 and 4 make similar sample and inference changes as their counterparts in Table 4. Consistent with the results in Table 3, we do not find statistically significant effects of being denied an abortion on these outcomes under these alternative sample and inference choices.

Finally, it is important to note that although we do not find evidence that the Turnaway group was on a worse financial trajectory than the Near Limit group prior to the abortion denial, it may be the case that such differential trends were present but we do not have sufficient statistical power to detect them. Following the approach outlined in Roth (2019), we estimate that we can detect a positive linear pre-trend in the financial distress index of 0.042 or larger with 80 percent power and of 0.017 or larger with 50 percent power. Even in this worst case scenario, where the largest possible undetectable trend exists, the biases resulting from such undetected trends would not reach the size of our post-period coefficient estimates until the fourth year after the birth year for the trend estimated with 80 percent power, and never during the study period for the trend estimated with 50 percent power.[28]

We further explore whether such a differential trend might be driving our results by estimating an alternative version of our event study model that explicitly allows a differential pre-trend. We estimate a version of equation (1) that includes a linear trend for the Turnaway group and omits the pre-birth

[28]We calculate the biases following the formula presented in Roth (2019), which takes into account the additional bias introduced by passing a pre-test.

event study coefficients, following Gross et al. (2020). The results are reported in Appendix Table A1. We continue to find higher rates of financial distress among the Turnaway group relative to the Near Limit group, although the point estimates are somewhat larger than in our main specification. Consistent with our main results, we do not find evidence for a change in access to credit.

5 Additional Analyses

We conduct a small number of additional analyses to provide more context for our results and to suggest possible directions for future research. Because of the limitations described along with each of these analyses, we consider these analyses to be mainly exploratory.

5.1 Regression Discontinuity Model

In addition to our primary event study specification, we take advantage of the sampling design of the Turnaway Study to implement a regression discontinuity design that compares outcomes for women just above and below the gestation limit at each clinic. The RD approach aims to compare women who sought abortions at gestational ages just above or just below the age limit at their clinic. These women sought abortions at a very similar point in their pregnancy, but were treated differently by providers because of the gestational cutoff rules. An RD design that estimates "instantaneous" changes that occur around the cutoff, which abstracts from any systematic differences related to gestational age, providing an alternative method to estimate the causal effects of an abortion denial.

There are three important empirical challenges with applying the RD approach in our setting. First, since RD analyses effectively compare outcomes for individuals on either side of a given cutoff, this type of approach performs best when large sample sizes are available, which is not the case in our setting. For this reason, we use parametric regression, which uses all data available, to estimate the discontinuity at the cutoff (Lee and Lemieux, 2010). We also, however, present estimates using nonparametric methods (i.e. local linear regression).

Second, gestational age is determined by ultrasound measurement of the fetus under the assumption that its size is consistent with its age. As such, gestational age could be subject to mismeasurement or even manipulation by the ultrasound technician, which may result in women who are just "below" the cutoff differing systematically from women just "above" the cutoff. We can evaluate this empirically by looking at differences in financial outcomes for women just above or below the cutoff in the years prior to the pregnancy and (counterfactual) birth. For survey respondents, we can also assess whether there are any differences around the cutoff in other socioeconomic characteristics as mea-

sured one week after the abortion encounter. We also implement a "donut RD" (Barreca et al., 2016) that aims to limit any bias from possible manipulation by excluding women whose gestational ages are at the cutoff or within one day of the cutoff.

Third, we do not have information on the exact gestational age cutoff used at each clinic. Between 2008 and 2010, the period during which women were recruited into the Turnaway Study, several sample clinics changed their cutoffs, and these changes were not documented. In addition, even within a clinic, the latest age at which an abortion can be performed may vary due to physician availability or changes in the clinic's internal rules and practices. Given this lack of precise information on the relevant cutoff for each woman's specific clinic encounter, we use a data-driven procedure to estimate the most common gestational age cutoff used at each site. More details on this procedure are found in Appendix Section A.

We proceed with the RD analysis addressing each of these limitations as described above. Using each clinic-specific cutoff ($cutoff_c$), we define the distance to the cutoff for each woman i presenting at clinic c as $g_{ic} = gestation\ days_i - cutoff_c$. If $g_ic \geq 0$, the woman is likely to be turned away; otherwise, she is likely to receive the abortion. Although this is a "fuzzy" RD, in the sense that some women above the cutoff received abortions while some below the cutoff were turned away (due to mismeasurement of the cutoff, changes in the cutoff over time, or physician discretion), it performs well in predicting whether a woman was turned away. Appendix Figure A8 shows the fraction of women who were turned away at each day of gestation relative to the estimated clinic-specific cutoff. There is a large increase of about 85 percentage points at the estimated cutoff.

Using this estimated cutoff, we implement the RD analysis in two ways. First, we estimate a simple parametric regression that includes a linear trend in days from the cutoff that is allowed to vary on either side of the cutoff and an indicator variable that a woman's pregnancy is at or over the estimated gestational week cutoff for the clinic. Specifically, we estimate:

$$Y_{ict} = \beta_0 + \beta_1 1(g_{ic} \geq 0) + \beta_2 g_{ic} + \beta_3 1(g_{ic} \geq 0) \times g_{ic} + \epsilon_{ict}. \quad (3)$$

Second, we estimate a local linear regression using the Fuji et al. (2009) optimal bandwidth selector. This method has the advantage that it puts more weight on data points closer to the cutoff when estimating the discontinuity. The tradeoff is reduced precision since fewer data points may be used in its estimation. In all analyses, we cluster the standard errors at the individual level.

We first present RD plots for each event year relative to the birth year. Appendix Figure A10 plots the financial distress index by days of gestation relative to the estimated clinic-specific cutoff. Note that we center the figures at zero, indicating the cutoff of the relevant clinic for each individual, but that the estimated cutoff age varies by clinic. Panels (a)-(c) show the difference at the cutoff during the three years prior to the birth. If women close to the cutoff differ systematically for reasons other than the abortion denial, we might expect to see discontinuities in these pre-pregnancy years. However, we do not see any evidence of differences at the cutoff prior to the birth year. We further probe whether there are baseline differences across the gestational age cutoff using data from the initial Turnaway Study survey for those who responded. We find no evidence of systematic variation at the cutoff on these dimensions (see Appendix Table A2 and Figure A9).[29]

Panel (d) of Appendix Figure A10 shows the difference in the financial distress index at the cutoff in the year of the birth, and panels (e)-(i) show the differences in the years following the birth. In the first three years following the birth there appears to be a discontinuity at the cutoff, with women who were turned away experiencing relatively higher rates of financial distress. This difference becomes less apparent in years 4 and 5, consistent with the patterns documented in the plot of the data by event time in Figure 2. Similar results for the access index are presented in Appendix Figure A11. The figures associated with this outcome are fairly noisy and the patterns are less clear.

When we run the regression analyses, we pool years to increase precision. Motivated by the pattern observed in Figure 2, we estimate the RD model in four time periods. In the first row of Appendix Table A3, we show the RD estimate pooling observations for one, two, or three years prior to the birth. Consistent with the figure, we do not find a statistically significant discontinuity in financial distress at the cutoff in this pre-birth period. In row 2, we present the RD estimates using observations from the year of the birth. In this year, we see a statistically significant increase in the financial distress index in the linear model, but not in the local linear regression specification. For observations one to two years after the birth year, reported in row 3, we observe significant increases in the financial distress measure in the parametric linear ($p \leq 0.05$) and local linear ($p \leq 0.10$) regression models. These estimates indicate that financial distress increased by between 0.18 and 0.19 standard deviations during these years. This increase is slightly larger to that estimated under the event study model in Figure 3 during these years. In row 4, we see a marginally significant increase in financial distress

[29] In addition, we run an alternative specification that estimates the difference in the discontinuity over the pre- and post-periods. This "RD-DID" analysis accounts for any pre-existing differences in outcomes at the cutoff. Further details on this analysis and the results, which are consistent with those presented in the main RD analysis, may be found in Appendix Section B.

($p \leq 0.10$) during the period three to five years following the birth year under the parametric model but not under the local linear model. We do not find significant differences at the cutoff in our access measure during any of the time periods. The results for both indices are similar if we drop women whose gestational age is at or close to the cutoff (i.e. a "donut" RD). See Table A4 in the Appendix.

These RD estimates are broadly consistent with the patterns presented in Figures 2 and 3. Credit report outcomes were similar for the two groups of women prior to the birth but financial outcomes worsened for the Turnaway group after the birth, with the most pronounced effects appearing in the year of the birth and the two years immediately following the birth year.

5.2 Exploration of Mechanisms from the Turnaway Study Follow-Up Surveys

To better understand the economic circumstances of these women and clarify the mechanisms underlying our findings, we turn to additional data collected in the Turnaway Study. In the initial survey interview conducted one week after being denied an abortion, the majority of women in our sample in the Turnaway group were unmarried (83 percent), already had children (61 percent), and were unemployed (48 percent). Many were living with adult family members and few with a spouse or partner. More than 43 percent reported that they did not have enough money to cover housing, transportation, and food at least "most of the time." This suggests that women in the Turnaway group were already economically vulnerable prior to any additional financial consequences experienced as a result of the abortion denial.

We conduct an exploratory analysis of how survey outcomes evolved for these women in our sample relative to the Near Limit group. The outcomes examined here were analyzed previously in Foster et al. (2018), but we conduct our own analysis in order to limit the data to respondents included in our main credit report sample (i.e. those who were matched to the credit record data for at least one year in the pre-period and who meet our age restrictions). Conducting our own analysis of the Turnaway data allows us to produce estimates that are the most relevant and comparable to our credit report results, although they differ somewhat from those presented in Foster et al. (2018), which used the entire sample of survey respondents and focused on the subset of women in the Turnaway group who carried their pregnancies to term. The analysis in Foster et al. (2018) also used different statistical methods, making their estimates not directly comparable to those presented here.

Because women were first interviewed one week after the abortion encounter, we have both pre- and post-data available for the birth (or counterfactual birth) and are able to estimate our main

difference-in-differences specification (equation 2). However, in contrast to the credit report data, we are not able to evaluate whether pre-birth trends are similar across the Near Limit and Turnaway are limited to one observation period prior to the birth. In addition, a fairly large percent of respondents in our sample (24 percent at baseline) did not provide household income information, resulting in smaller sample sizes for this outcome. Because of these limitations, we consider our analysis in this subsection to be exploratory. We present yearly estimates, analogous to the event study coefficients, in Appendix Figures A12-A13 and difference-in-differences estimates in Appendix Tables A5-A6.

Consistent with Foster et al. (2018), we do not find strong evidence that monthly household income fell for the Turnaway group relative to the Near Limit group after the birth. However, because household size increased among Turnaway group respondents, their income as a percent of the FPL fell significantly, by about 28 percentage points. By year 4 after the birth year, this difference in income relative to FPL across the Near Limit and Turnaway Group appears to have closed (panel c of Appendix Figure A12).

We do not find evidence of changes in employment (panel d), but do find an increase in the receipt of public benefits. Specifically, we see that receipt of WIC increased significantly in the Turnaway group relative to the Near Limit group (panel e), with most of the increased receipt occurring in the year of the birth. We do not find statistically significant changes in TANF or food stamp receipt (panels e and g), although the estimated coefficients are positive for both of these outcomes, suggesting that use of these public programs may have increased as well. In addition, we are unable to examine changes in benefit amounts with the data available. Our point estimates suggest that child support payments to women in the Turnaway group increased following the abortion denial by about $20 per month in the years following the birth year, but these effects are not statistically significant (panel h of Figure A12). Finally, we do not find a significant change in the share of women reporting they do not have enough money "most of the time" (panel i), although the point estimate is positive, indicating an increase in this measure.

We also see that the Turnaway group experienced significant changes in their living situation relative to the Near Limit group (Appendix Figure A13 and Table A6). Following the birth, the Turnaway group was no more likely than the Near Limit group to be living with a male partner and significantly more likely to be living alone with her child or children. The yearly estimates also suggest that the Turnaway group was somewhat less likely to be living with adult family or non-family roommates than the Near Limit group (panels b and e of Appendix Figure A12).

Taken together, these results suggest that women who were denied abortions needed to care for an additional child without experiencing increases in income or support from male partners. Women in the Turnaway group may have experienced higher housing costs, in addition to the expenses associated with a new baby, as they became less likely to live with family or roommates following the birth. Increased participation in public programs, in the form of higher levels of WIC receipt, was also short lived. Such patterns likely drive the patterns of the inability to meet financial obligations documented in our credit report analysis.

5.3 Heterogeneous Effects by State Policy Environment

Next, we examine heterogeneity in the effect of an abortion denial based on the generosity of the social safety net in the state in which the woman resided at the time she sought an abortion.[30] To characterize state safety net generosity, we examine the income threshold at which a household can gain eligibility for Temporary Aid to Needy Families (TANF). The federal government provides funding for this cash assistance program for low-income families to states who determine their own eligibility criteria for the program. We characterize states as "high generosity" if they allow TANF receipt at household incomes of 50.8 percent of the Federal Poverty Level or higher, corresponding to the average eligibility threshold observed in our data.[31] The results are presented in Appendix Table A7. Women who live in low generosity states experience significant increases in financial distress following an abortion denial. We do not find a significant effect among women residing in high generosity states, although the point estimate is still positive indicating higher financial distress. In addition, the difference in coefficients is not statistically significant (p=0.18).

This result suggests that the financial impact of abortion denial may vary according to the state policy environment. At the same time, we note that high and low generosity states differ on many dimensions other than TANF eligibility. For example, in our data, Turnaway group members who reside in high generosity states are denied abortions at significantly later gestational ages, indicating that these states permit abortions to occur later in the pregnancy. Differences in abortion restrictions and other state policies not included in this analysis may also be relevant in understanding the larger effects observed in the low generosity states.

[30]Note that although participating clinics were only in 21 states, more than 21 states are represented in this analysis because some women traveled to a different state to seek an abortion.

[31]States classified as "low generosity" are AL, AR, AZ, CO, DE, FL, GA, ID, IL, IN, KS, MA, MD, MO, MS, MT, NC, NJ, OR, PA, SC, TX and WV. States classified as "high generosity" are AK, CA, CT, IA, KY, ME, MI, ND, NM, NV, NY, OH, OK, SD, TN, VA, WA, and WI. Results are similar if we instead use the median (42 percent of the FPL) as the cutoff between "high generosity" and "low generosity" states.

5.4 Comparison to Subsequent Pregnancies in Near Limit Group

By affecting a woman's ability to receive or not receive an abortion, gestational limits may remove the option for women to have no children, in addition to changing the timing of when they have a child. Notably, only 25 percent of women in the Near Limit group who received a wanted abortion went on to have a child during the 5-year study period. In addition, prior research has documented that access to abortion has effects on completed fertility rather than just delaying childbearing; abortion legalization led to an increase in the share of women remaining childless (Ananat et al., 2007).

While acknowledging this, it may still be of interest to know whether the financial distress experienced by the Turnaway group around the time of the birth was similar to that experienced by women with similar socioeconomic status giving birth after more wanted pregnancies. It may be the case that the burden experienced by the Turnaway group is particularly high when compared to an alternative where women are able to more optimally time childbearing. Such a comparison is difficult because, to our knowledge, there is no comprehensive data linking information on socioeconomic status, birth timing and wantedness, and credit report information. Even if such data existed, it would be difficult to match the Turnaway group to "equally" disadvantaged childbearing women since women seeking abortion may vary on unobservable or difficult-to-measure dimensions such as partner, family, or community support.

To shed light on this comparison, we conduct an exploratory analysis using members of the Near Limit group who gave birth in the five years following their abortion. This analysis takes advantage of information on subsequent births collected by the Turnaway Study follow-up surveys. These births represent a mix of wanted and unwanted births that is likely more representative of a "typical" birth among women in our sample, which we are able to compare to the explicitly unwanted births observed for the Turnaway group.[32] Similar to our analysis of the Turnaway women, we restrict the sample to women in the Near Limit group who had a follow-up birth, were at least 20 years old in the year they gave birth, and had a match to the credit reporting agency database prior to the birth. We emphasize that this is exploratory as only 97 Near Limit participants had an observed birth over this period and meet our sample criteria. However, this sample does give us the opportunity to explore how financial outcomes change around childbirth for a sample of women with similar socioeconomic status as the Turnaway group, but whose birth did not necessarily result from an abortion denial.

[32]On the London Measure of Unplanned Pregnancy, for which higher values (out of a maximum score of 12) indicate more planned births, subsequent children born to the Near Limit group scored a 6.8, while births to the Turnaway group following the abortion denial scored a 2.8 (Foster et al., 2018).

Since we only observe most women for a limited time after their subsequent birth, which tended to occur between 1 and 3 years following their abortion, we define our follow up period in this analysis as the four years following the birth. We continue to define the pre-period as the three years prior to birth, to follow our approach in the main analysis for Turnaways. As a comparison group, we use Near Limit participants who did not give birth. We estimate the following model:

$$Y_{it} = \sum_{\substack{y=-3 \\ y \neq -1}}^{3} \beta_y I(t - t_i^{\dagger} = y) + \nu_t + \delta_i + \epsilon_{it}. \tag{4}$$

Here, $t_i^{\dagger}$ is the year in which the Near Limit participant gives birth, ν_t are calendar year fixed effects, and δ_i are individual fixed effects. The coefficients on the event study indicators, β_y, show how financial outcomes changed for the Near Limit group who gave birth relative to the time trend experienced by Near Limit participants who did not give birth, as captured by ν_t. Robust standard errors are clustered by individual.

The results are presented in Appendix Figure A14. To facilitate comparison, we add the event time estimates for the Turnaway group, re-scaled by the fraction of women in the Turnaway group who gave birth; these estimates are plotted with a solid green line.[33] The estimates of β_y from the Near Limit model described in equation (4) are plotted with a dashed blue line. The point estimates on the effect of financial distress for subsequent Near Limit births are smaller than those observed among the Turnaway group and are not statistically significant (panel a). In addition, the confidence intervals on the estimates for the Near Limit births do not include the coefficient estimates for the Turnaway group. We see no evidence of a change in credit access following the subsequent births of the Near Limit group with coefficient estimates very close to zero.

These results suggest that births that occurred following a subsequent pregnancy result in less financial distress than those that occur after an abortion denial. In addition, this is likely an underestimate of any difference in the financial effects between wanted and unwanted births, given that some of these subsequent births likely resulted from unplanned or unwanted pregnancies. However, we note that the confidence intervals of the two estimates do overlap, making it difficult to draw strong conclusions from this exercise.

[33]For the purpose of this re-scaling, we apply the birth rate (68 percent) for the Turnaway group calculated among those individuals with observed pregnancy outcomes.

6 Conclusion

Restrictions on abortion are common with 63 new state laws aimed at restricting abortion access implemented in 2017 alone (Nash et al., 2018). Despite the fact that such laws are pervasive, we have little data documenting how being denied an abortion affects the financial and economic well-being of women. This paper provides the first evidence on this topic using longitudinal data that allows us to observe women both before and after the abortion denial. We link high-quality administrative data from credit reports to participants in the Turnaway Study. These data allow us to compare the trajectory of outcomes for women who were denied (Turnaway group) versus received wanted abortions (Near Limit group) on the basis of state and facility gestational limits.

We find evidence that being denied an abortion has large and persistent negative effects on a woman's financial well-being. Women denied an abortion experience a significant increase in financial distress during the year that they give birth (or, in some cases, would have given birth since some of them received an abortion elsewhere or miscarried), compared to their counterparts who received a wanted abortion. Unpaid debts that are 30 or more days past due more than double in size, and the number of public records, which include negative events such as evictions and bankruptcies, increases substantially. This financial impact extends throughout our sample period, with negative effects observed up to four years after the birth year. While we do not find as strong of evidence of changes in the financial independence of these women, as measured through markers of credit access such as having a mortgage, we do find that the women who were denied an abortion were significantly less likely to have a prime credit score in the two years following the birth.

The size of the effects are substantial when compared to effects documented in other settings. While the women in our study differ from the populations analyzed in other settings, estimates based on other interventions could still provide a useful benchmark for what size of effects we might expect. For example, the impact of being denied an abortion on collections is as large as the effect of being evicted (Humphries et al., 2019) and the impact on unpaid bills is several times larger than the effect of losing health insurance (Argys et al., 2019). Although imprecisely estimated in our setting, it appears that denying a woman an abortion reduces her credit score by more than the impact of a health shock resulting in a hospitalization (Dobkin et al., 2018) or being exposed to high levels of flooding following Hurricane Harvey (Billings et al., 2019).

We can draw additional insights into the mechanisms behind these changes from the Turnaway

Study follow-up surveys. Using survey data from Foster et al. (2018), but implementing our sample criteria and empirical approach, we find that women's household income as a percent of the FPL fell for the Turnaway group relative to the Near Limit group after the birth or counterfactual birth. At the same time, women in the Turnaway group did not experience significant relative increases in the probability of living with a male partner and, if anything, experienced decreases in the probability of living with adult family, such as parents, or other adult roommates. Instead, women in the Turnaway group became more likely to live alone with her child or children. The Turnaway group experienced relative increases in the use of WIC in the year of the birth and small, marginally statistically significant increases in the amount of child support received each month. In sum, while women's family obligations and need for resources increased following the abortion denial, they did not appear to experience increases in support from male partners, adult family, or the government to sufficiently offset these responsibilities, possibly driving the inability to meet financial obligations documented in our credit report analysis.

While acknowledging that abortion restrictions affect a woman's ability to decide not to have any children, in addition to the timing of such children, we also explore whether the financial consequences observed for births following an abortion denial are similar to those observed after a birth that results from a typical pregnancy. To do this, we use information for women in the Near Limit group who received a wanted abortion but who later became pregnant and gave birth. These women have similar socioeconomic characteristics to the Turnaway group. These births were a mixed of planned and unplanned births more similar to a "typical" birth experienced in this population, rather than an explicitly unwanted birth such as those experienced by the Turnaway group. Among these subsequent births, we find that the financial consequences of giving birth are less severe, although our confidence intervals are large and we cannot reject that the effects are the same as those for the Turnaway group who gave birth. However, this exploratory analysis suggests that more wanted or optimally timed births may have fewer economic consequences.

Our study indicates that laws that impose gestational limits for abortion result in worse financial and economic outcomes for the women who are denied an abortion. For the women who carry their pregnancies to term (the vast majority in our sample), there are likely to be important implications for the well-being of their offspring. There is a large literature documenting the importance of the early life environment for health and achievement over the life course. In particular, evidence indicates that human capital development under age 5 can have large long-term impacts (Currie and Almond, 2011).

Given that we observe significant financial distress for the women who were turned away during this period, there may be negative consequences for children's basic needs and other investments during this critical period. Providing some evidence for this, the Turnaway Study documented that women from the Turnaway group who carried their pregnancies to term experienced worse bonding with their child than women who received an abortion and later had a subsequent child. Children of the Turnaway mothers also lived in households with lower income levels and were less likely to have money to cover their basic living expenses than these other subsequent children (Foster et al., 2018).[34]

There are several implications for public policy. If policymakers wish to avoid the adverse economic consequences documented here, one option would be to relax laws that impose a gestational limit for abortion. At clinics for which these laws are binding, increasing gestational age limits would allow more women to be served. At the same time, several clinics choose gestational age limits that are below those legally allowed, due to clinicians' training, availability, or preference. Increasing the number of clinicians available to perform these services (for example, by reducing the regulatory burdens imposed on abortion providers) may help alleviate these supply side constraints.

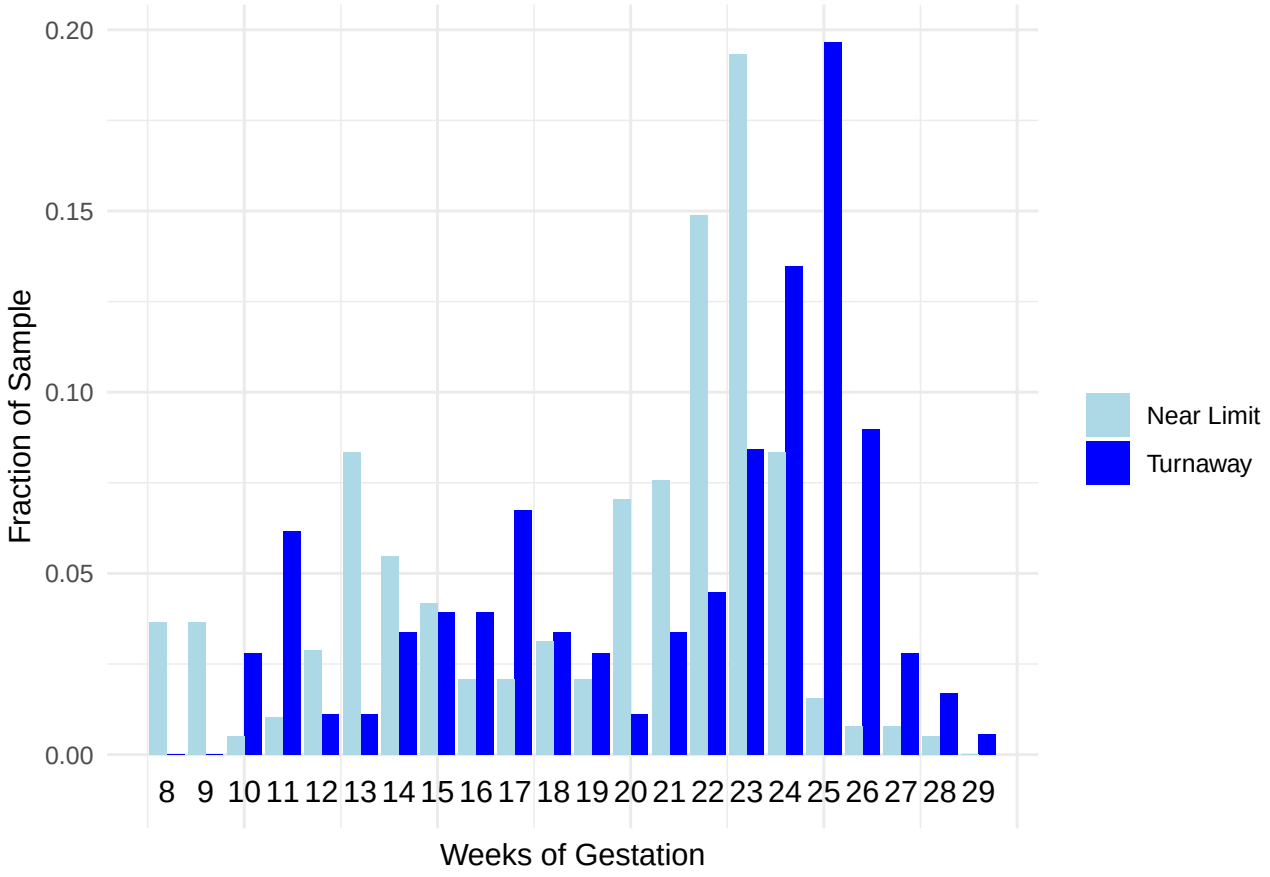
An alternative approach is to craft policies that make it less likely that women will seek abortions at later gestational ages. We can again benefit from the data collected by the Turnaway Study to identify barriers to women seeking abortions earlier in their pregnancies (Upadhyay et al., 2014). The majority of women in the Near Limit (67 percent) and Turnaway (58 percent) study groups named travel and procedure costs as a reason for their delay in seeking an abortion. Reports of other common barriers include administrative and logistical problems related to insurance coverage for the procedure, not knowing where to get care, and not knowing how to get to a provider. Public insurance does not cover abortion services in most cases and some private plans are prohibited from covering abortion under state restrictions.[35] These responses also suggest that increasing the availability of abortion providers and the affordability of the procedure may help to reduce delays in seeking care. However, other reasons that may be more difficult to address are women not recognizing their pregnancy (reported by 43 percent of women in the Near Limit group and 48 percent of women in the Turnaway

[34]The Turnaway Study also documents potential impacts for the other children of these women: the children that women already had at the time of seeking an abortion fare worse in terms of achieving developmental milestones and living in economic security when their mothers were denied, rather than receiving a wanted abortion (Foster et al., 2019); also, women were less likely to have an intended child within the next five years if they were denied an abortion (Upadhyay et al., 2019).

[35]The Hyde Amendment bans federal funding for abortion except in cases of life endangerment, rape, and incest. Some states choose to cover abortions under their Medicaid programs using their own funds (Salganicoff et al., 2020). In addition, some states regulate whether abortion services may be covered by private plans that are not self-insured, or for plans participating in the ACA marketplaces (Salganicoff et al., 2019).

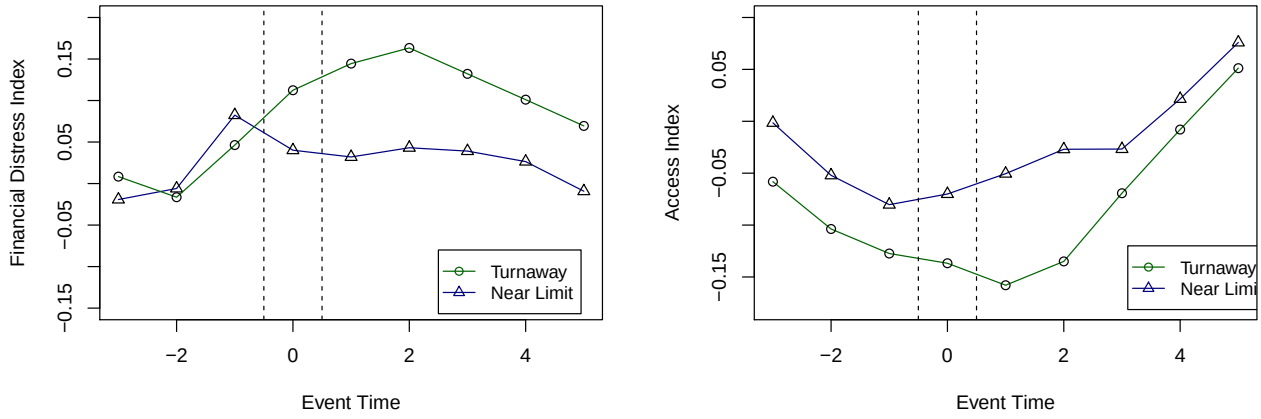
group), or difficulty deciding whether to have an abortion (44 percent and 40 percent, respectively). And, given that the current trend has been for state laws to lower gestational limits, with recent efforts to ban abortions as early as 6 weeks or even throughout the entire pregnancy (Guttmacher Institute, 2019), it seems likely that the number of women being denied a wanted abortion in the U.S. will only continue to grow over time.

Figure 1: Histogram of Gestational Age of Pregnancy at Time of Abortion Receipt or Denial



Note: These figures display histograms of the distribution of the sample for the Turnaway and Near Limit group based on the gestational age of the pregnancy at the time of abortion denial (in the case of the Turnaway group) and abortion receipt (in the case of the Near Limit group).

Figure 2: Financial Outcomes Relative to Event Time, by Group

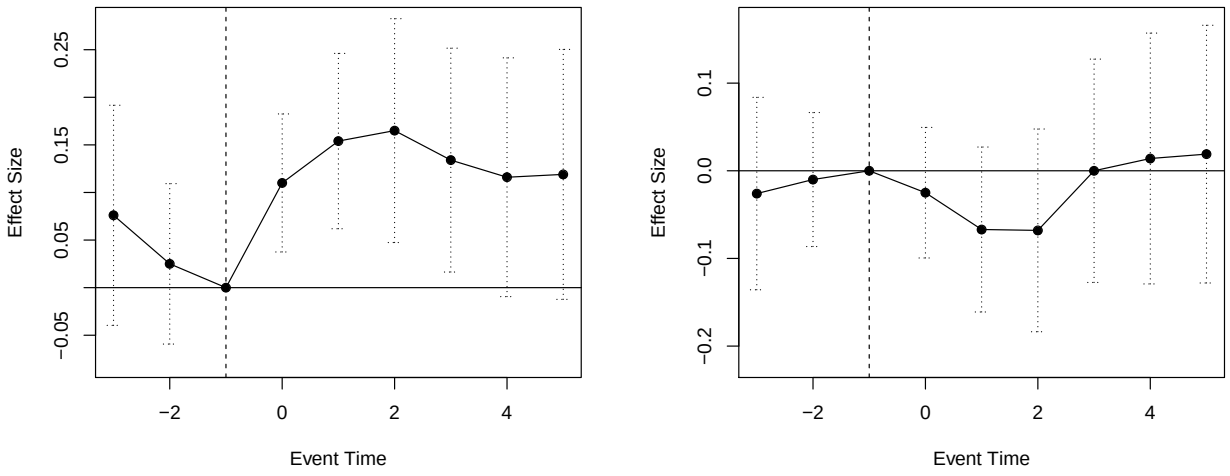


(a) Financial Distress Index

(b) Credit Access Index

Note: These figures show the average value of the financial distress index (panel a) and the credit access index (panel b) by year for the Turnaway and Near Limit groups in the main sample. See text for more information.

Figure 3: Effect of an Abortion Denial on Financial Outcomes: Event Study



(a) Financial Distress Index

(b) Credit Access Index

Note: These figure reports coefficients from the estimation of Equation (1) for the financial distress index (panel a) and the credit access index (panel b). The coefficients represent the change in each outcome for Turnaway group members relative to Near Limit group members in the three years before and six years after the time of birth or counterfactual birth, as compared to the year immediately prior to this event. See text for more information.

Table 1: Initial Survey Measures Across Matched/Unmatched (Survey Respondents Only)

Outcome	Near Limit			Turnaway		
	Matched	Unmatched	Diff. (Std. Err)	Matched	Unmatched	Diff. (Std. Err)
HS Education or Less	0.480	0.614	-0.134 (0.064)**	0.453	0.700	-0.247 (0.084)***
Single	0.775	0.729	0.046 (0.058)	0.827	0.630	0.202 (0.083)**
Full Time Employed	0.357	0.257	0.100 (0.059)*	0.267	0.275	-0.008 (0.080)
Part Time Employed	0.219	0.157	0.062 (0.049)	0.233	0.050	0.183 (0.049)***
Enough Money	0.553	0.514	0.038 (0.066)	0.570	0.450	0.120 (0.089)
In Poverty	0.555	0.620	-0.065 (0.075)	0.529	0.774	-0.245 (0.091)***
Age at Survey	25.6	26.7	-1.1 (0.8)	24.8	27.2	-2.4 (1.0)**
Received WIC	0.153	0.143	0.010 (0.046)	0.160	0.225	-0.065 (0.073)
Received TANF	0.117	0.129	-0.011 (0.044)	0.127	0.200	-0.073 (0.069)
Received Food Stamps	0.345	0.257	0.088 (0.059)	0.400	0.575	-0.175 (0.088)**
# Individuals	333	70		150	40	

Note: This table presents means for outcome variables observed in the initial survey of the Turnaway Study for both those matched and not matched to the credit records. These statistics are drawn from the complete Turnaway Study sample, including those younger than age 20 at the year of the birth. Asterisks indicate statistically significant differences between the matched and unmatched sample within each group: *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Baseline Sample Characteristics: Mean, Median, and Standard Deviation

	First Trimester			Near Limit			Turnaway			Diff b/w Turnaway and	
	Mean	Std Dev	Med	Mean	Std Dev	Med	Mean	Std Dev	Med	First Trimester	Near Limit
Delinquency Outcomes:											
Collections ($)	2,112	3,343	681	2,391	4,024	829	2,887	4,924	841	775 (388)**	496 (385)
Amount Past Due ($)	10,037	64,183	0	2,968	13,655	177	2,236	5,159	298	-7,801 (3,701)**	-732 (544)
Public Records	0.14	0.51	0.00	0.12	0.43	0.00	0.08	0.33	0.00	-0.06 (0.03)*	-0.04 (0.03)
Subprime Credit Score	0.74	0.44	1.00	0.83	0.38	1.00	0.84	0.36	1.00	0.11 (0.03)***	0.01 (0.03)
Access Outcomes:											
Prime Credit Score	0.13	0.34	0.00	0.06	0.25	0.00	0.07	0.25	0.00	-0.07 (0.02)***	0.001 (0.02)
Credit Score	560.15	86.85	547	532.18	77.94	520	529.90	77.82	519	-30.25 (7.32)***	-2.29 (6.27)
Credit Available ($)	1,980	10,649	0	798	5,215	0	699	3,607	0	-1,281 (722)*	-99 (345)
Any Mortgage	0.11	0.32	0.00	0.07	0.26	0.00	0.03	0.18	0.00	-0.08 (0.02)***	-0.039 (0.02)**
# Matched Ind x Year Obs.		2,165			3,350			1,565			
# Matched Individuals		246			383			180			

Notes: This table presents descriptive statistics for outcome variables as observed in the years prior to the birth or counterfactual birth year. These statistics are drawn from the primary analysis sample, as described in the text. The final two columns show the difference between the Turnaway group and the First Trimester group and Near Limit group respectively. Asterisks indicate statistically significant differences: *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Effect of an Abortion Denial on Financial Outcomes: Difference-in-Differences

	Financial Distress Index	Collections	Public Records	Amount Past Due	Subprime
Post × Turnaway	0.102**	202.46	0.065**	1,749.7**	0.010
	(0.045)	(529.56)	(0.026)	(702.47)	(0.023)
Pre-Period Turnaway Mean		2887	0.08	2236	0.84
	Credit Access Index	Prime Credit Score	Any Mortgage	Available Credit	Credit Score
Post × Turnaway	-0.009	-0.014	0.006	297.30	-4.857
	(0.054)	(0.018)	(0.016)	(490.03)	(5.191)
Pre-Period Turnaway Mean		0.065	0.033	698.72	529.9

Notes: N=4,914. Analyses use 2006-2016 Experian credit report files for Turnaway and Near Limit sample of women age 20 and older the year of the birth or counterfactual birth. Sample is restricted to women who had a credit report record prior to the birth or counterfactual birth. All regression models include individual fixed effects and an indicator that event time$\geq$0. Robust standard errors are clustered by individual. Significance levels: *=10%, **=5%, ***=1%. Pre-birth mean for Turnaway mothers reported in bottom row.

Table 4: Alternative Specifications: Financial Distress Measures

	Financial Distress Index	Collections	Public Records	Amount Past Due	Subprime
Pre-Period Missings as Zero	0.117**	196.58	0.064***	1,687.25**	0.010
N=5,076	(0.047)	(508.095)	(0.025)	(676.75)	(0.023)
Missings Imputed	0.110**	202.19	0.058**	1,521.36**	0.016
N=5,207	(0.048)	(485.84)	(0.024)	(645.46)	(0.022)
All Ages	0.096**	185.04	0.062**	1,667.68**	0.008
N=5,150	(0.043)	(511.13)	(0.025)	(678.28)	(0.023)
Cluster by Clinic	0.102**	202.46	0.065***	1,749.69***	0.010
N=4,914	(0.044)	(529.56)	(0.023)	(611.057)	(0.023)

Notes: Analyses use 2006-2016 Experian credit report files for Turnaway and Near Limit sample of women. Each row shows results for a different sample or model specification. All regression models include individual fixed effects and an indicator that event time$\geq$0. Robust standard errors are clustered by individual. Significance levels: *=10%, **=5%, ***=1%. Pre-birth mean for Turnaway mothers reported in bottom row.

Table 5: Alternative Specifications: Credit Access Measures

	Access Index	Prime Credit Score	Any Mortgage	Available Credit	Credit Score
Pre-Period Missings as Zero	-0.010	-0.014	0.006	283.46	-4.86
N=5,208	(0.056)	(0.018)	(0.015)	(469.1)	(5.19)
Missings Imputed	-0.031	-0.020	0.003	213.49	-6.609
N=5,076	(0.057)	(0.017)	(0.014)	(441.67)	(4.813)
All Ages	-0.009	-0.014	0.005	289.86	-4.899
N=5,151	(0.052)	(0.018)	(0.015)	(472.68)	(5.142)
Cluster by Clinic	-0.009	-0.014	0.006	297.30	-4.857
N=4,914	(0.068)	(0.021)	(0.016)	(504.1)	(4.458)

Notes: Analyses use 2006-2016 Experian credit report files for Turnaway and Near Limit sample of women. Each row shows results for a different sample or model specification. All regression models include individual fixed effects and an indicator that event time$\geq$0. Robust standard errors are clustered by individual. Significance levels: *=10%, **=5%, ***=1%. Pre-birth mean for Turnaway mothers reported in bottom row.

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The Economic Consequences of Being Denied an Abortion

Appendix

Sarah Miller Laura R. Wherry Diana Greene Foster

This appendix provides further details and additional results to supplement those presented in the main text. Details on match rates by participant age and differential match rates by study group are reported in Figures A1 and A2. Analytic sample inclusion criteria are found in Figure A3; each column represents a step in the sample inclusion criteria process and shows the number of observations that meet this and all previous inclusion criteria. Plots of the summary index components are reported in Figures A4 and A5. These plots are analogous to those presented in Figure 2 in the main text, but for component outcomes. Table A1 shows event study estimates when a differential "pre-trend" is included in the model. Tables A2-A7 and Figure A8-A14 present results from additional analyses described in the text.

Tables A2-A4 and Figures A8-A11 show the results for the regression discontinuity analysis. Figure A8 shows the change in the fraction of women turned away at each estimated clinic cutoff. Figure A9 and Table A2 report checks for discontinuities across the gestational age cutoff for women who responded to the baseline survey. Figures A10-A11 and Table A3 present the results of the RD analysis, while results for a "donut" RD that drops women with gestational ages equal to or within one day of the cutoff are in Table A4. More details on our estimation of the clinic-specific gestational age cutoffs are below in Section A.

Table A7 runs the main analysis by state groups defined using the generosity of state welfare programs. Figure A14 shows the results for the analysis that compares outcomes for subsequent births of the Near Limit group to the Turnaway births.

A Estimation of Clinic-Specific Gestational Age Cutoffs

Over the period of the Turnaway Study, several clinics changed their policies regarding the latest gestational age at which they would provide an abortion. These policy changes were not recorded. Furthermore, clinic policies could change on a day-to-day basis depending on the availability of providers. In order to estimate an RD model using gestational age, we must first estimate the most likely gestational age at each clinic. To do this, we implement a simple RD model for each site that

estimates the probability that a woman was turned away at different gestation week cutoffs. Our candidate cutoffs include the earliest cutoff at which we observe a woman being turned away (which may be a fraction of gestational weeks–e.g., 16 weeks and 5 days) and all possible cutoffs at round numbers of weeks (i.e. not fractions of weeks) within the entire distribution of gestational ages of women turned away from that given clinic. We estimate a linear RD model that identifies the change in the likelihood of being turned away among all participants in the Near Limit and Turnaway groups at the clinic at each of these cutoffs with an indicator variable for women with pregnancies of gestational age at or above the cutoff. It includes a running variable measuring distance in gestational age from the cutoff and we allow the slope to vary before and after the cutoff. This model is estimated separately for each clinic and for all possible cutoffs. We select the clinic-specific cutoff using the largest t-statistic associated with this indicator variable across all candidate cutoffs.

B Additional "RD-DD" Analysis

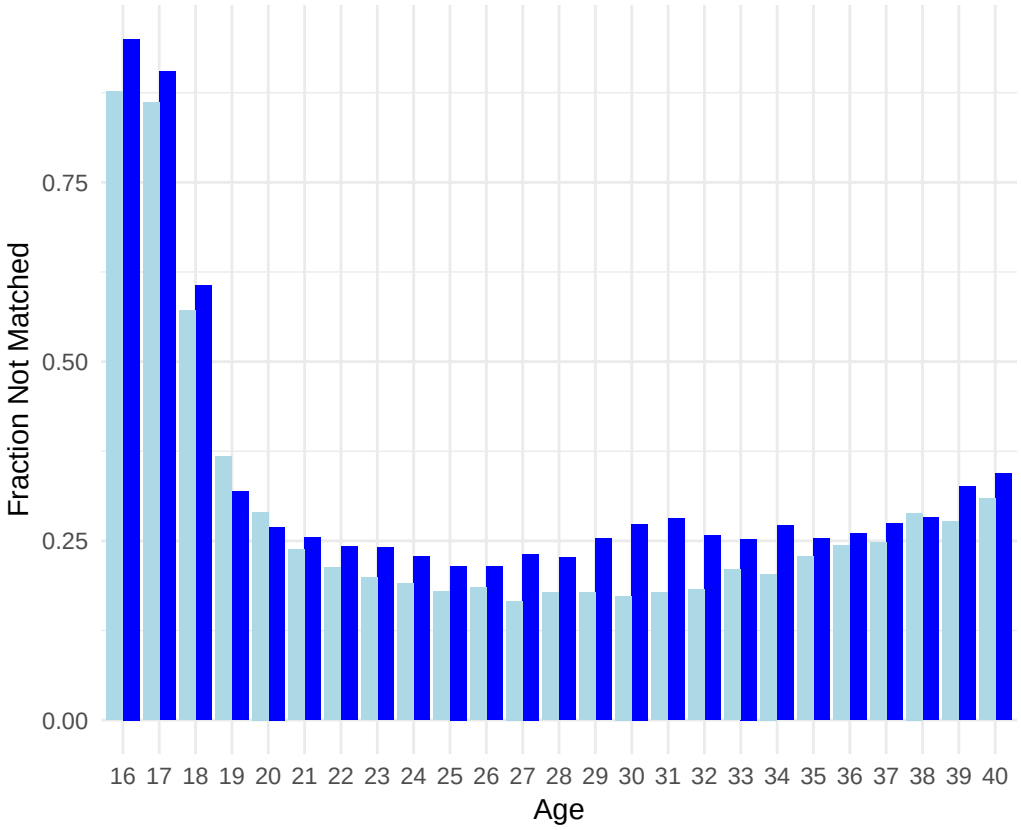
In addition to our main RD analysis, we also estimate an alternative "RD-DD" specification that differences the discontinuity observed at the gestational age cutoff before and after the birth year in the linear parametric model. To do this, we pool all years, including those prior to the birth, and estimate:

$$\begin{aligned} Y_{ict} =& \beta_{RD,DD} 1(g_{ic} \geq 0) \times Post_t + \beta_1 1(g_{ic} \geq 0) + \beta_2 g_{ic} + \beta_3 1(g_{ic} \geq 0) \times g_{ic} + \\ & \beta_4 Post_t \times g_{ic} + \beta_5 Post_t \times 1(g_{ic} \geq 0) \times g_{ic} + \epsilon_{ict}. \end{aligned} \tag{5}$$

Here, the coefficient $\beta_{RD,DD}$ provides the difference in the discontinuity estimated before the birth year ($Post_t = 0$), and in the year of the birth and later ($Post_t = 1$). In this way, the analysis uses pre-period data to control for any pre-existing differences in outcomes at the cutoff. As in all RD models, we cluster the standard errors at the individual level.

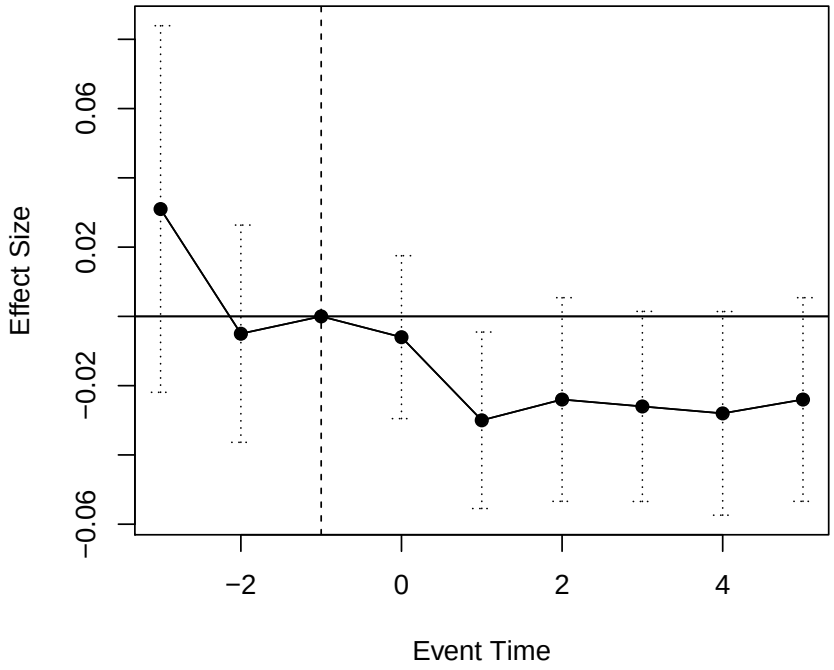
We present estimates for the RD-DD model in the last row of Table A3. This model estimates the difference in the parametric linear RD estimate before and after the birth year. Estimates generated from this model are consistent with the previous event study and RD results: we find large and statistically significant increases in financial distress associated with abortion denial but little evidence of change in the credit access index.

Figure A1: Fraction Not Matched by Age for Near Limit (Light Blue) and Turnaway Group (Dark Blue)



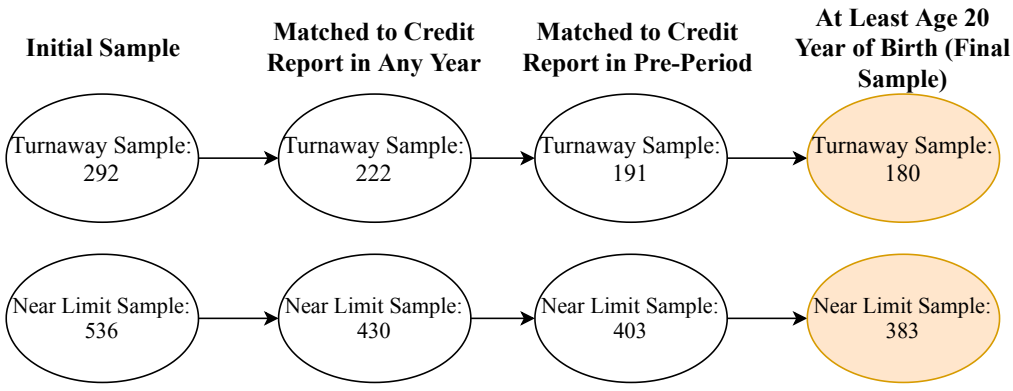
Note: This bar chart shows the fraction of the Near Limit (light blue) and Turnaway Group (dark blue) who are not matched to the credit report data at each age we observe them.

Figure A2: Changes in Probability of Not Matching to Credit Reporting Agency Data by Event Year



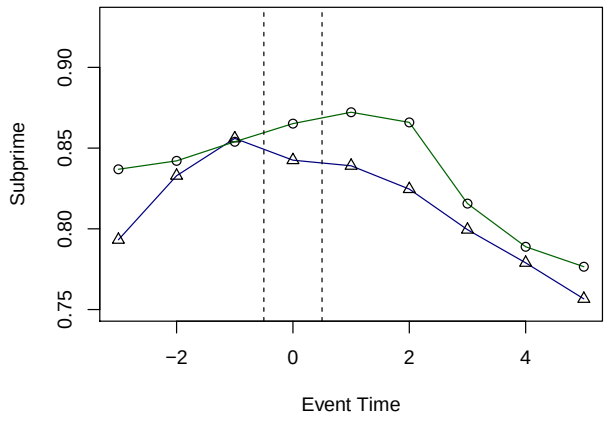
Note: This event study figures shows estimates of equation (1) where the dependent variable equals 1 if the woman did not match to the credit reporting data in that year. Note that this estimation includes those with no pre-period match to the credit reporting data.

Figure A3: Sample Size by Inclusion Criteria

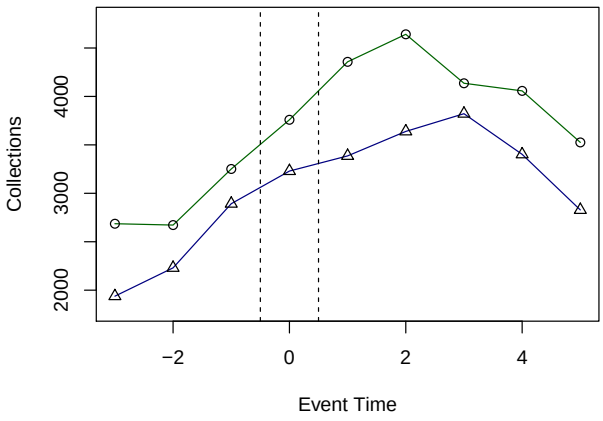


Note: This flow chart demonstrates how sample sizes change for each sample inclusion criteria for the Turnaway (top) and Near Limit (bottom) groups.

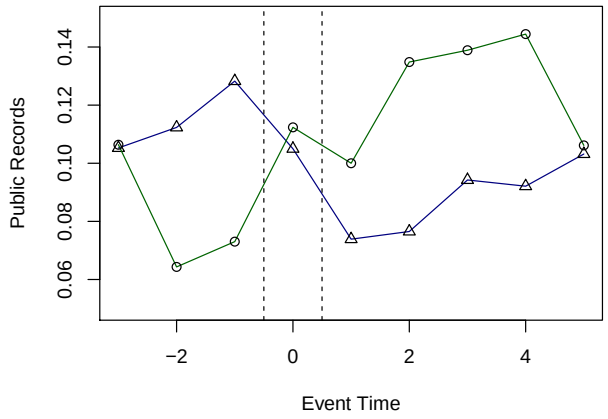
Figure A4: Financial Distress Component Outcomes Relative to Event Time, for the Turnaway Group (Green) and Near Limit Group (Blue)



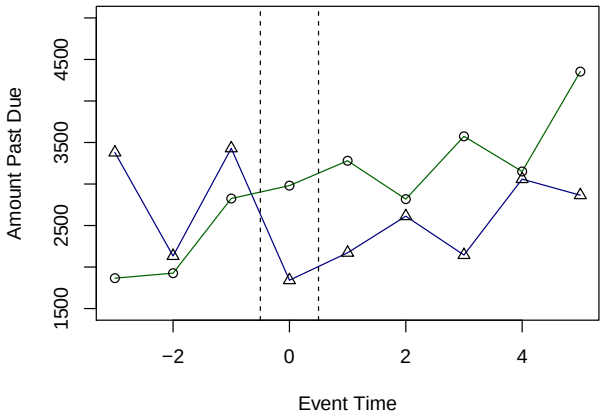
(a) Subprime



(b) Collections



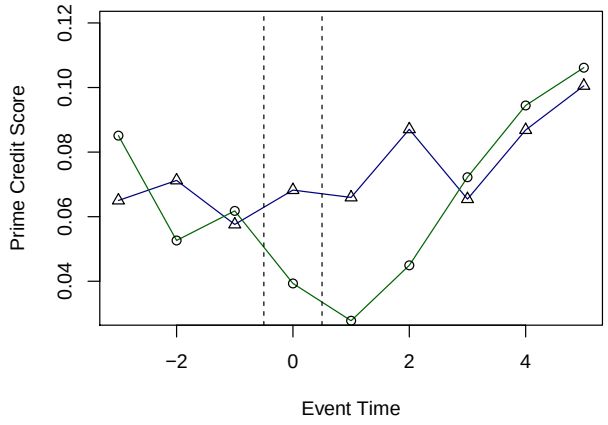
(c) Public Records



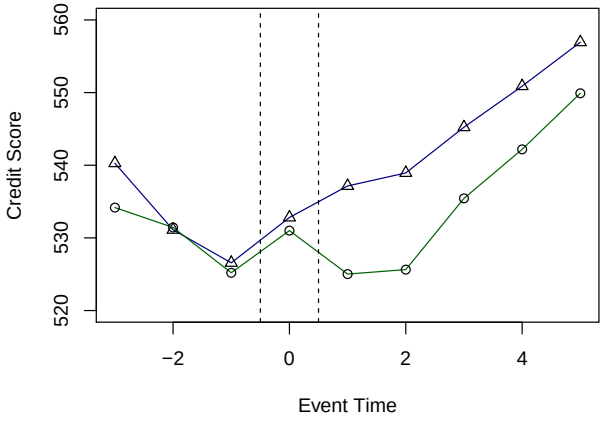
(d) Past Due

Note: This figure plots average outcomes relative to event time for the Turnaway group (green with circle points) and the Near Limit group (blue with triangle points).

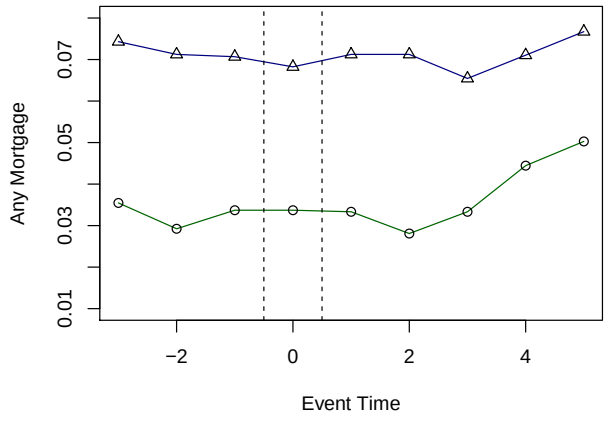
Figure A5: Access Component Outcomes Relative to Event Time, for the Turnaway Group (Green) and Near Limit Group (Blue)



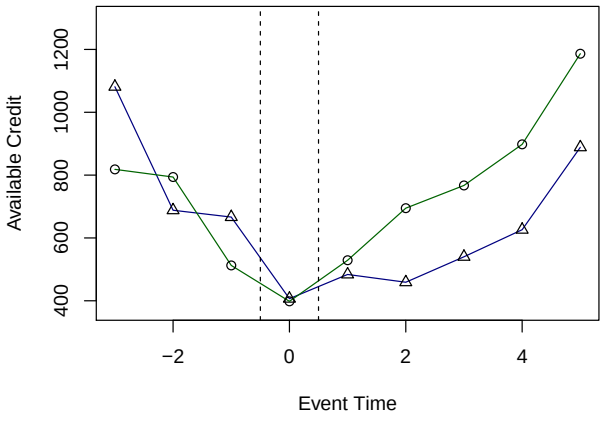
(a) Prime Credit Score



(b) Credit Score



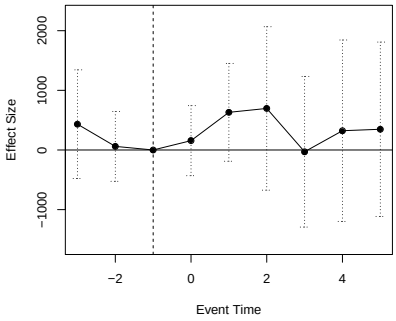
(c) Mortgage



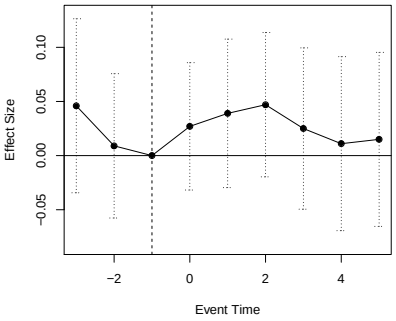
(d) Credit Card Credit Available

Note: This figure plots average outcomes relative to event time for the Turnaway group (green with circle points) and the Near Limit group (blue with triangle points).

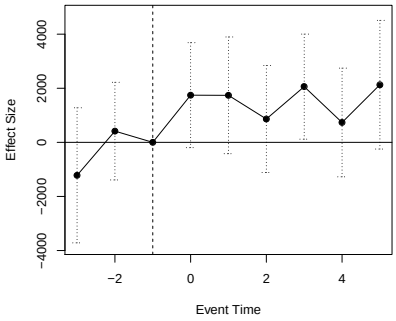
Figure A6: Event Study Coefficients: Financial Distress Component Measures



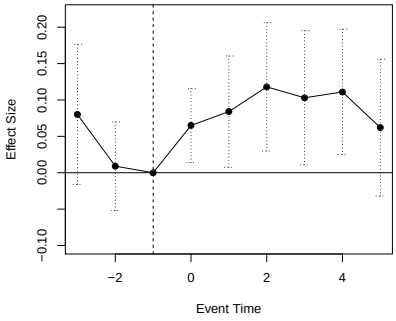
(a) Collections



(b) Subprime Credit Score



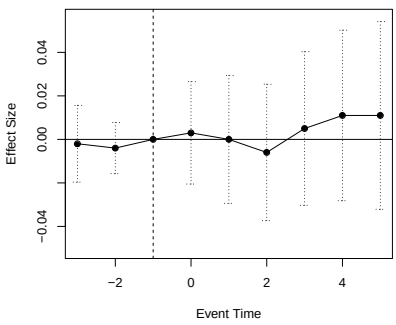
(c) Amount Past Due



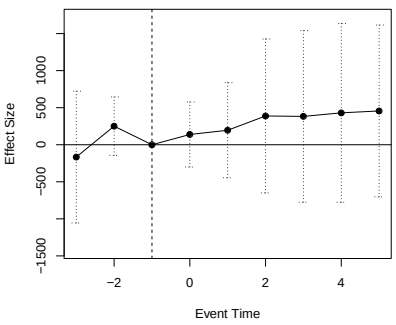
(d) Public Records

Note: These figures report coefficients from the estimation of Equation (1) for the specified outcome. The coefficients represent the change in the outcome for Turnaway group members relative to Near Limit group members in the three years before and six years after the time of birth or counterfactual birth, as compared to the year immediately prior to this event. See text for more information.

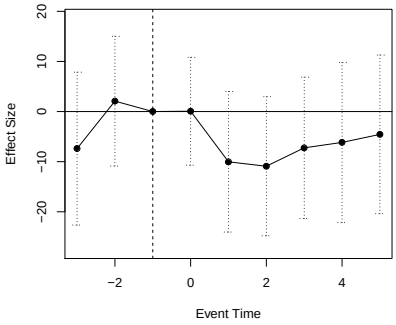
Figure A7: Event Study Coefficients: Access Component Measures



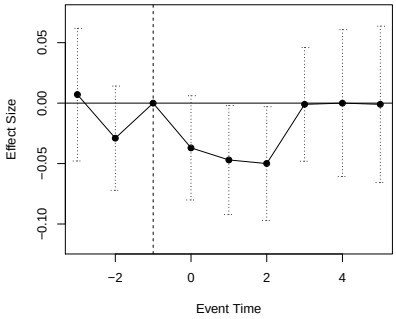
(a) Any Mortgage



(b) Credit Available



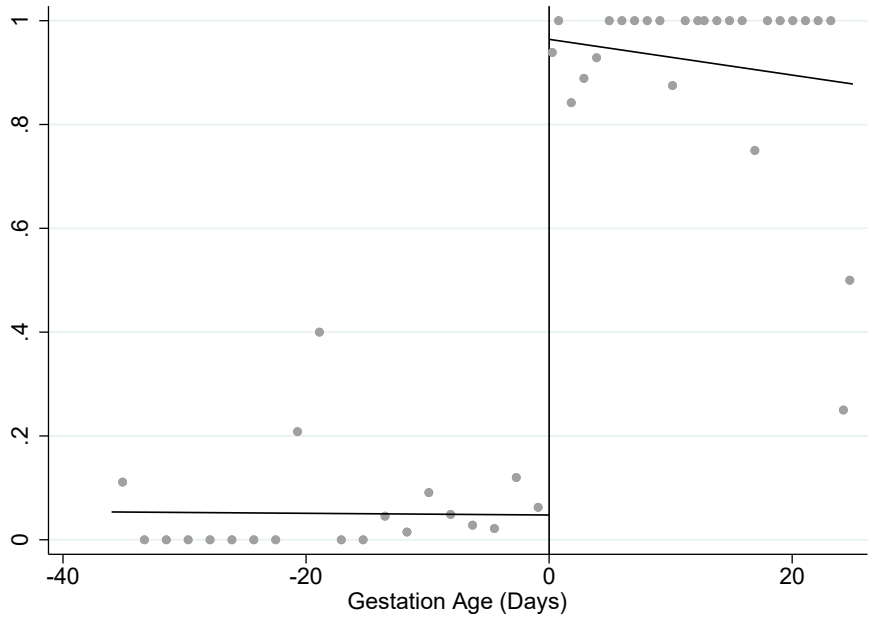
(c) Credit Score



(d) Prime Credit Score

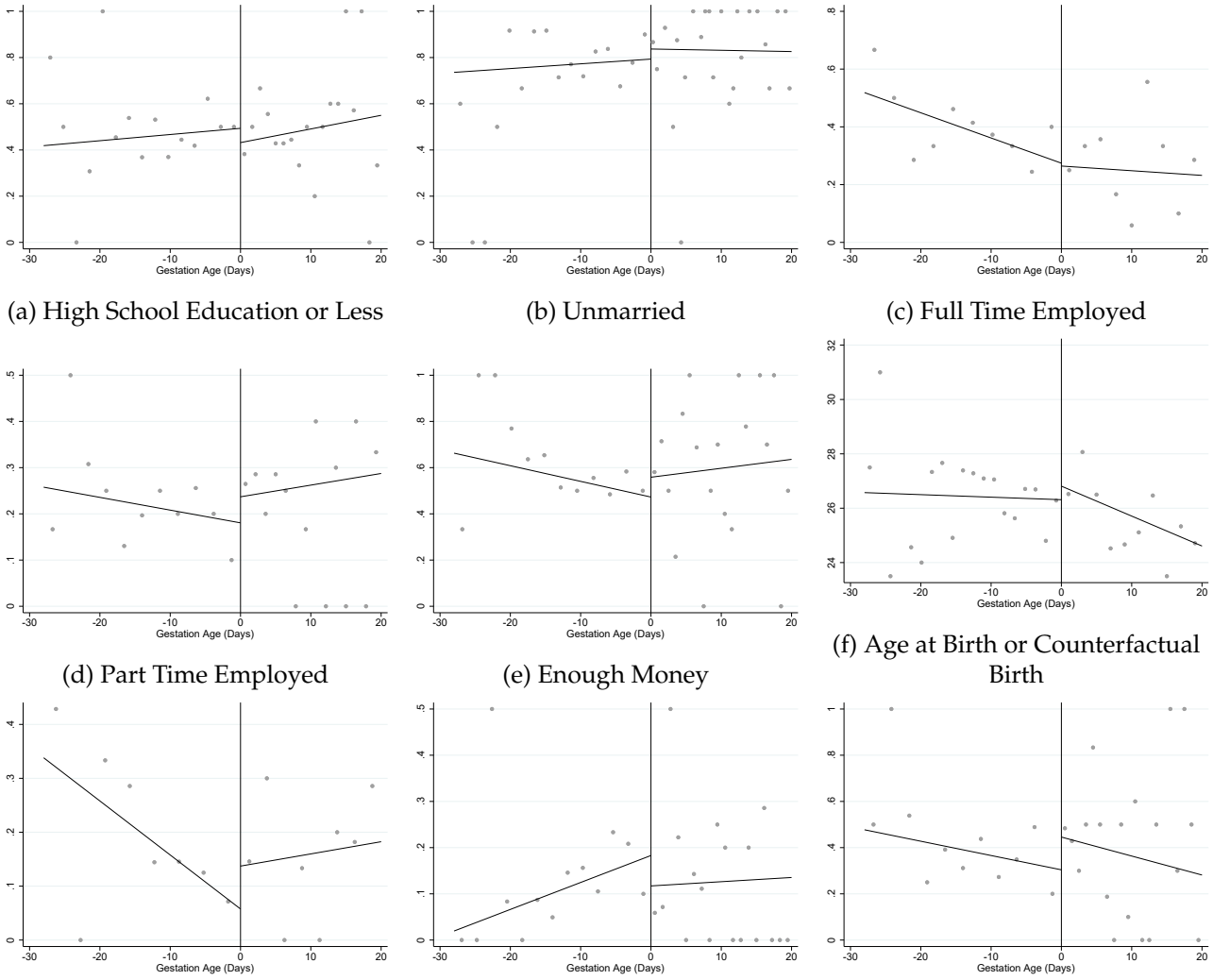
Note: These figures report coefficients from the estimation of Equation (1) for the specified outcome. The coefficients represent the change in the outcome for Turnaway group members relative to Near Limit group members in the three years before and six years after the time of birth or counterfactual birth, as compared to the year immediately prior to this event. See text for more information.

Figure A8: Change in Fraction Turned Away Relative to Estimated Clinic Cutoff



Note: This figure shows the fraction of women who were turned away at each day relative to the estimated clinic-specific cutoff. Points represent means of the gestation age-specific denial rate. The lines are fitted values from a regression that includes a linear trend in gestational age and a dummy for gestation ages greater than or equal to the cutoff age.

Figure A9: RDD Graphs for Characteristics at Baseline Survey



(a) High School Education or Less

(b) Unmarried

(c) Full Time Employed

(d) Part Time Employed

(e) Enough Money

(f) Age at Birth or Counterfactual Birth

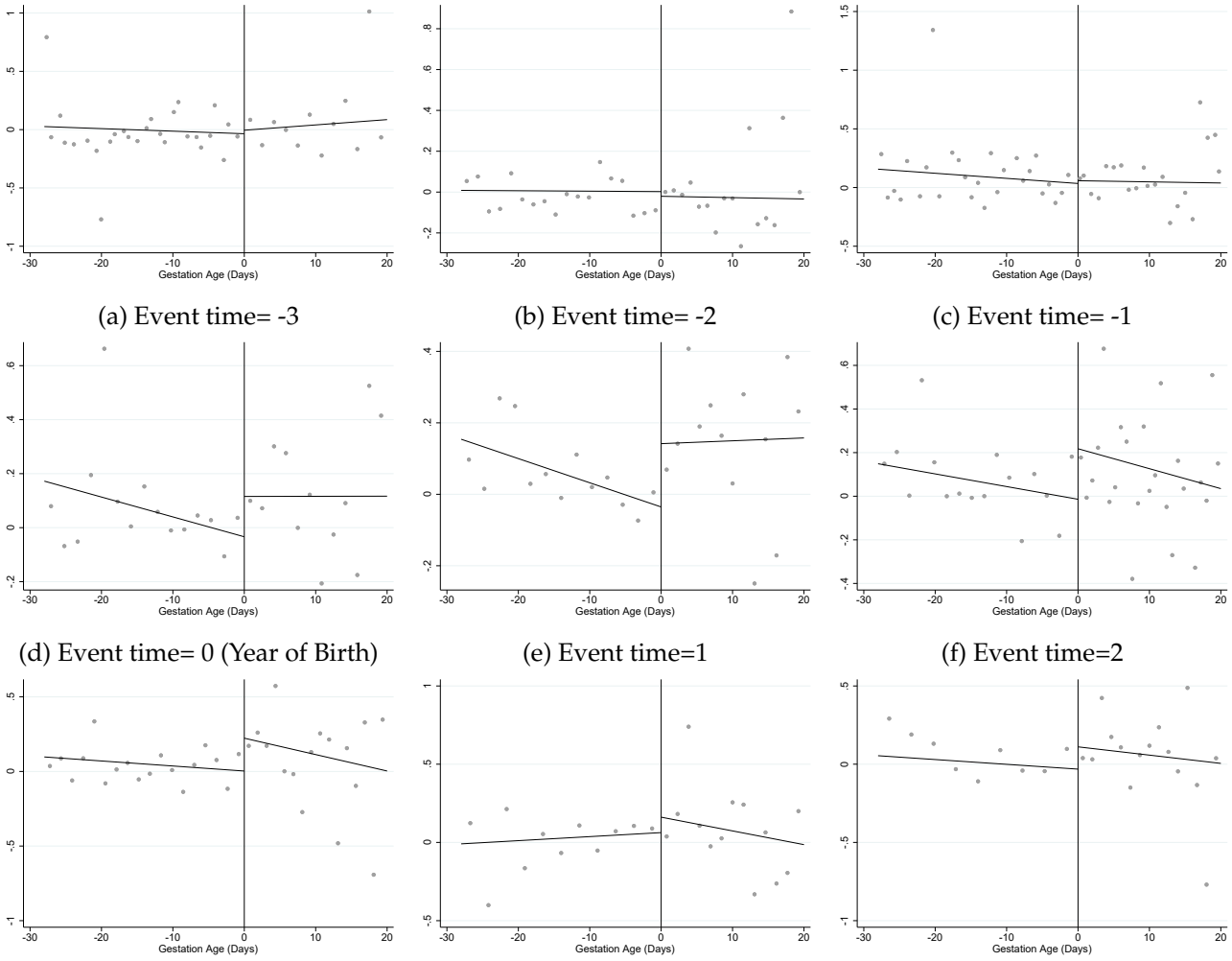
(g) Received WIC

(h) Received TANF

(i) Received Food Stamps

Note: For each characteristics, points represent means for each gestation age relative to the cutoff. The lines are fitted values from a regression that includes a linear trend in gestational age and a dummy for gestation ages greater than or equal to the cutoff age.

Figure A10: RDD Graphs By Event Time, Outcome: Financial Distress Index



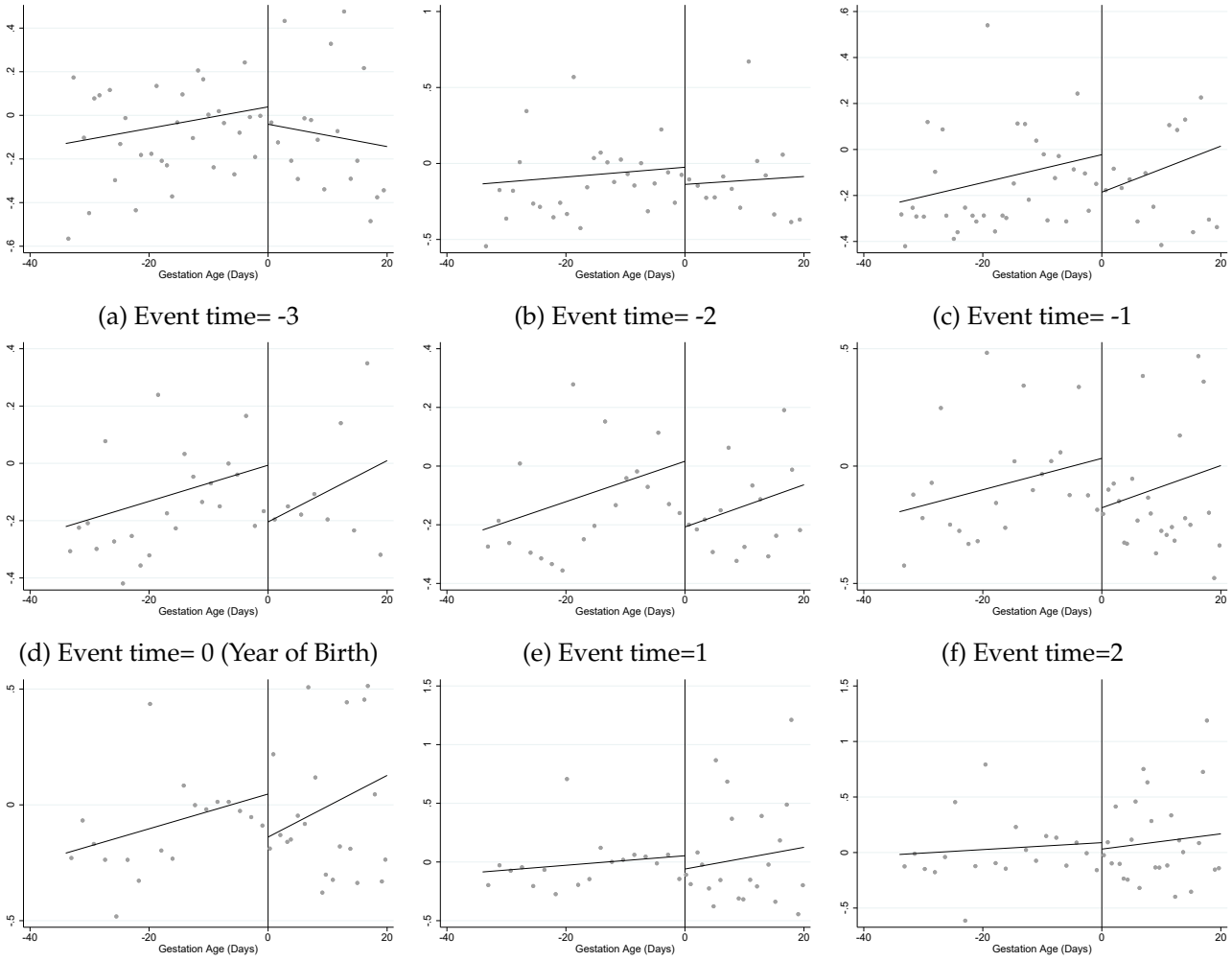
(a) Event time= -3 (b) Event time= -2 (c) Event time= -1

(d) Event time= 0 (Year of Birth) (e) Event time=1 (f) Event time=2

(g) Event time=3 (h) Event time=4 (i) Event time=5

Note: For each outcome, points represent means for each gestation age relative to the cutoff. The lines are fitted values from a regression that includes a linear trend in gestational age and a dummy for gestation ages greater than or equal to the cutoff age.

Figure A11: RDD Graphs By Event Time, Outcome: Credit Access Index



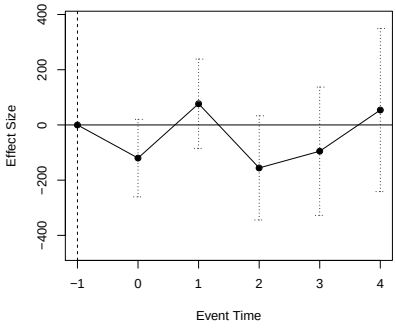
(a) Event time= -3 (b) Event time= -2 (c) Event time= -1

(d) Event time= 0 (Year of Birth) (e) Event time=1 (f) Event time=2

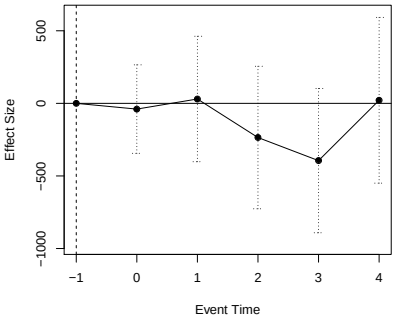
(g) Event time=3 (h) Event time=4 (i) Event time=5

Note: For each outcome, points represent means for each gestation age relative to the cutoff. The lines are fitted values from a regression that includes a linear trend in gestational age and a dummy for gestation ages greater than or equal to the cutoff age.

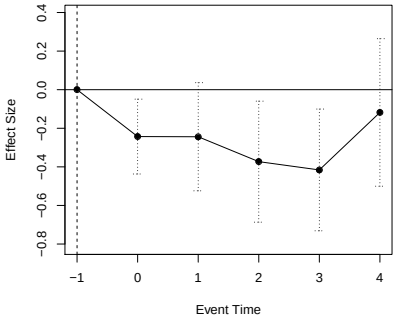
Figure A12: Exploring Mechanisms with Survey Data: Economic Outcomes



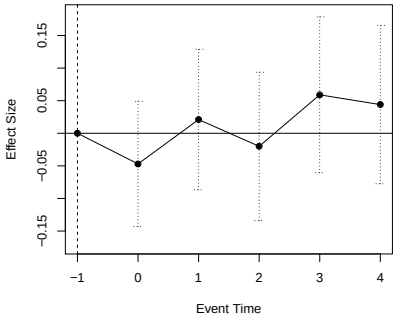
(a) Personal Income



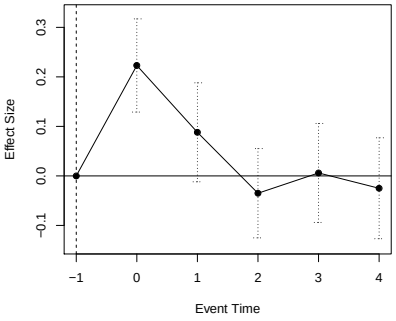
(b) Household Income



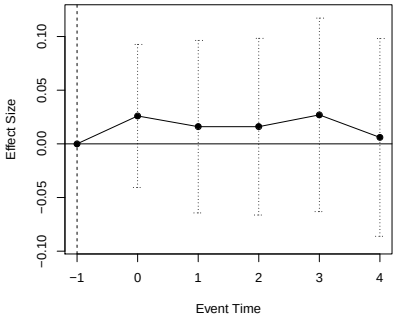
(c) Income Relative to FPL



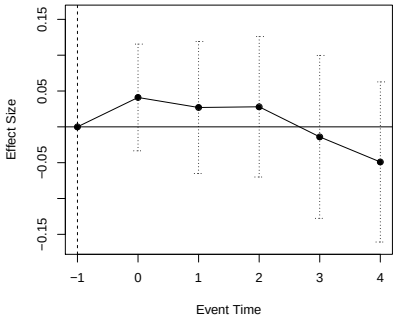
(d) Employed



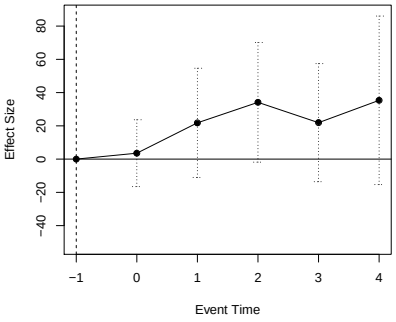
(e) Receives WIC



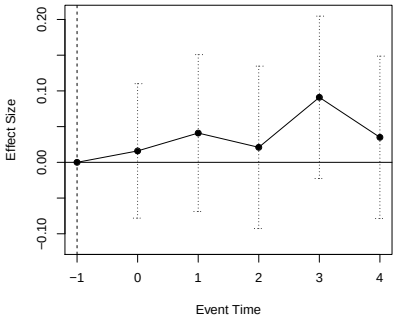
(f) Receives TANF



(g) Receives Food Stamps



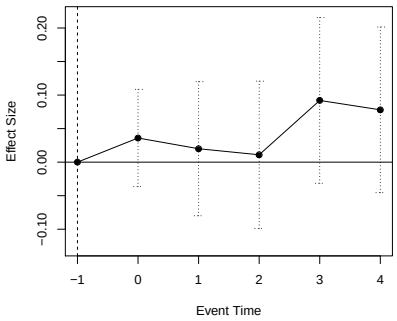
(h) Child support



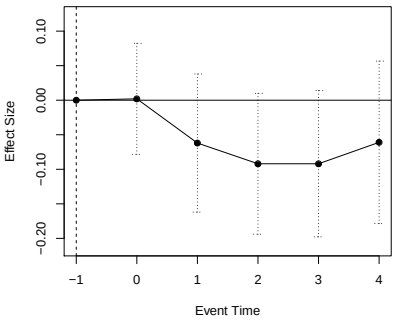
(i) Not enough money "most of the time" or more

Note: These figures report coefficients from a variant of Equation (1) estimated with available data for the specified outcome. The coefficients represent the change in the outcome for Turnaway group members relative to Near Limit group members in the one year before and five years after the time of birth or counterfactual birth, as compared to the year immediately prior to this event. See text for more information.

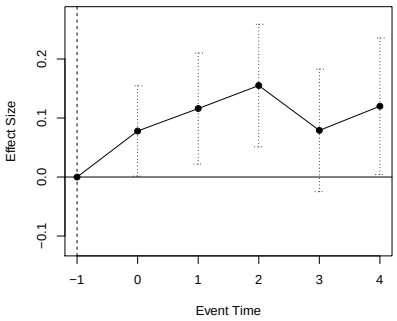
Figure A13: Exploring Mechanisms with Survey Data: Living Situation



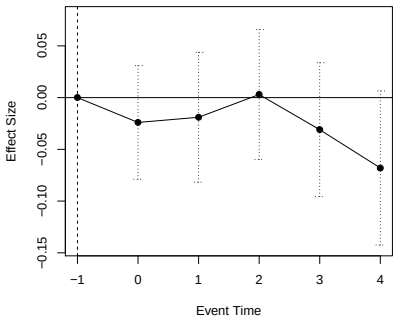
(a) Living with Male Partner



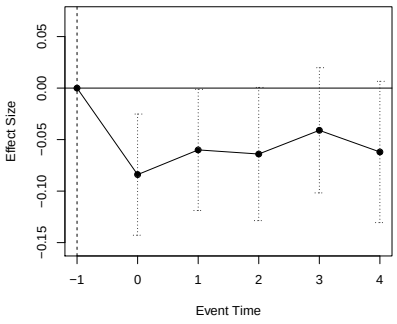
(b) Living with Adult Family



(c) Living Alone with Child/Children



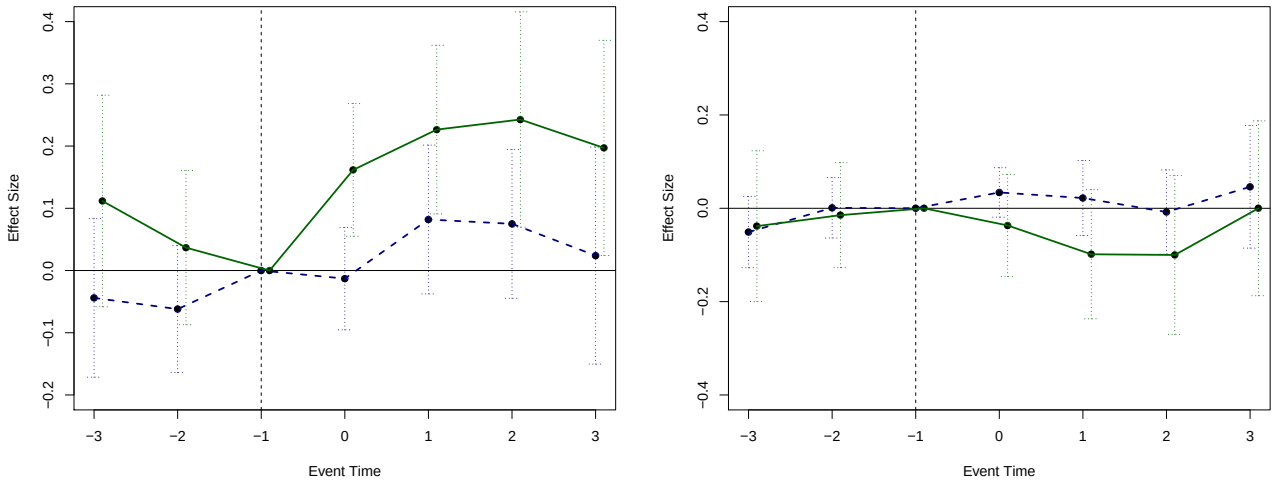
(d) Living Alone



(e) Living with (Non-Family) Roommates

Note: These figures report coefficients from a variant of Equation (1) estimated with available data for the specified outcome. The coefficients represent the change in the outcome for Turnaway group members relative to Near Limit group members in the one year before and five years after the time of birth or counterfactual birth, as compared to the year immediately prior to this event. See text for more information.

Figure A14: Effect of Turnaway Births (Green) relative to Near Limit Subsequent Births (Blue)



(a) Financial Distress Index

(b) Credit Access Index

Note: These figures show estimates of coefficients β_y (from equations 1 and 4) among the Turnaway group (in solid green) and the Near Limit group who gave birth following their abortion (in dashed blue). Coefficients for the Turnaway group are scaled by the fraction of women in this group who gave birth (68%). 95 percent confidence intervals are also plotted.

Table A1: Alternative Event Study Specifications

	Financial Distress Index		Access Index	
	Baseline	With Linear "Pre Trend"	Baseline	With Linear "Pre-trend"
Year=-3	0.077 (0.059)	–	-0.026 (0.056)	–
Year=-2	0.025 (0.043)	–	-0.010(0.039)	–
Year=-1	–	–	–	–
Birth Year	0.110 (0.037)***	0.153 (0.052)***	-0.025 (0.038)	-0.039 (0.051)
Year=1	0.154 (0.047)***	0.234 (0.085)***	-0.067 (0.048)	-0.094 (0.081)
Year=2	0.173 (0.061)***	0.292 (0.119)**	-0.067 (0.059)	-0.107 (0.109)
Year=3	0.126 (0.060)**	0.282 (0.144)*	-0.001 (0.065)	-0.053 (0.136)
Year=4	0.116 (0.064)*	0.311 (0.174)*	0.014 (0.073)	-0.052 (0.166)
Year=5	0.119 (0.067)*	0.352 (0.203)*	0.019 (0.075)	-0.060 (0.194)

Note: This table presents event study coefficients estimating the impact of being turned away on financial distress (Columns 1 and 2) and access to credit (Columns 3 and 4). See text for details. Significance levels: *=10%, **=5%, ***=1%.

Table A2: Regression Discontinuity Estimates in Initial Survey Responses (Survey Respondents Only)

	Parametric Linear	LLR
HS Education or Less	-0.022 (0.082)	-0.155 (0.122)
Single	0.011 (0.067)	0.054 (0.085)
Full Time Employed	-0.053 (0.076)	-0.153 (0.137)
Part Time Employed	0.161 (0.103)	0.057 (0.066)
Enough Money	0.112 (0.080)	0.23 (0.132)
Age at birth	0.201 (0.810)	1.038 (1.316)
Received WIC	0.067 (0.058)	0.064 (0.076)
Received TANF	-0.062 (0.052)	-0.069 (0.072)
Received Food Stamps	0.181 (0.122)	0.158 (0.079)**

Note: Table shows RD estimates of outcome variables listed in each row. These outcome variables were recorded on the initial survey that participants completed approximately one week after the abortion encounter. Significance levels: *=10%, **=5%, ***=1%.

Table A3: Regression Discontinuity Estimates

	Financial Distress Index		Access Index		
	Parametric Linear	LLR	Parametric Linear	LLR	N
Time Relative to Birth/Counterfactual Birth					
Three to one years prior to birth	0.011 (0.076)	0.103 (0.109)	-0.057 (0.116)	-0.031 (0.134)	1,555
Year of birth	0.169 (0.084)**	0.123 (0.108)	-0.125 (0.103)	-0.047 (0.104)	557
One to two years after birth year	0.195 (0.086)**	0.176 (0.100)*	-0.105 (0.118)	-0.105 (0.105)	1,117
Three to five years after birth year	0.150 (0.087)*	0.046 (0.107)	0.027 (0.133)	0.003 (0.110)	1,669
Regression Discontinuity/Difference in Differences Model					
RD-DD Effect	0.157 (0.074)**	N/A	0.015 (0.079)	N/A	4,898

Note: This table presents RD estimates of the impact of being turned away on financial distress (Columns 1 and 2) and access to credit (Columns 3 and 4). See text for details. Significance levels: *=10%, **=5%, ***=1%.

Table A4: "Donut" RD Estimates, Exclude Gestational Ages at the Cutoff

	Financial Distress Index				Access Index					
	Parametric Linear		LLR		Parametric Linear		LLR		N	
Drop Gestational Ages:	Equal Cutoff	+/- 1 Day	Equal Cutoff	+/- 1 Day	Equal Cutoff	+/- 1 Day	Equal Cutoff	+/- 1 Day	Equal Cutoff	+/- 1 Day
Time Relative to Birth/Counterfactual Birth										
Three to one years	-0.011	-0.019	0.027	0.028	-0.080	-0.078	0.022	-0.014	1,452	1,399
prior to birth	(0.085)	(0.092)	(0.124)	(0.159)	(0.125)	(0.137)	(0.161)	(0.140)		
Year of birth	0.223**	0.219*	0.244*	0.242	-0.117	-0.119	0.077	-0.109	520	501
	(0.110)	(0.119)	(0.135)	(0.150)	(0.120)	(0.135)	(0.178)	(0.168)		
One to two years after birth	0.249**	0.287**	0.202	0.351*	-0.073	-0.087	0.055	-0.045	1,043	1,005
	(0.110)	(0.118)	(0.163)	(0.190)	(0.140)	(0.156)	(0.209)	(0.233)		
Three to five years after birth	0.216*	0.265**	0.143	0.341	0.095	0.090	0.40	0.079	1,558	1,502
	(0.119)	(0.129)	(0.202)	(0.224)	(0.159)	(0.177)	(0.239)	(0.318)		
Regression Discontinuity/Difference in Differences Model										
RD-DD Effect	0.239**	0.284***	N/A	N/A	0.084	0.074	N/A	N/A	4,573	4,407
	(0.099)	(0.105)			(0.099)	(0.105)				

Note: This table presents RD estimates of the impact of being turned away on financial distress (Columns 1 through 4) and access to credit (Columns 5 through 8) with women with gestational ages at or within one day of the cutoff dropped from the data, as noted in the column heading. Significance levels: *=10%, **=5%, ***=1%.

Table A5: Exploring Mechanisms with Survey Data: Economic Outcomes

	Personal Income	Household Income	Income Relative to FPL	Employed Partner	Lives w Adult Family	Lives Alone with Child
Post × Turnaway	-60.72	-106.51	-0.276**	0.001	0.080**	0.021
	(73.92)	(182.21)	(0.123)	(0.047)	(0.041)	(0.036)
Pre-Period Turnaway Mean	917.96	1928.93	1.33	0.556	0.154	0.126
N	3,797	3,191	3,191	3,947	3,947	3,947

	Receives Food Stamps	Child Support Income	Not Enough Money
Post × Turnaway	0.016	19.92	0.039
	(0.039)	(14.05)	(0.047)
Pre-Period Turnaway Mean	0.406	19.90	0.442
N:	3,947	3,947	3,947

Notes: Analyses use 11 waves of Turnaway Study survey data for sample of women age 20 and older the year of the birth or counterfactual birth. Sample is restricted to women who had a credit report record prior to the birth or counterfactual birth in order to match sample criteria in main analysis. All regression models include individual fixed effects and an indicator that event time$\geq$0. Robust standard errors are clustered by individual. Significance levels: *=10%, **=5%, ***=1%. Mean for Turnaway mothers at baseline survey (1 to 2 weeks after abortion encounter) reported in bottom row.

Table A6: Exploring Mechanisms with Survey Data: Living Situation

	Alone with Child	With Male Partner	With Adult Family	With Room Mates	Alone
Post × Turnaway	0.106***	0.039	-0.051	-0.064**	-0.024
	(0.041)	(0.043)	(0.044)	(0.028)	(0.028)
Pre-Period Turnaway Mean	0.406	19.90	0.442		
N:	3,947	3,947	3,947	3,947	3,947

Notes: Analyses use 11 waves of Turnaway Study survey data for sample of women age 20 and older the year of the birth or counterfactual birth. Sample is restricted to women who had a credit report record prior to the birth or counterfactual birth in order to match sample criteria in main analysis. All regression models include individual fixed effects and an indicator that event time$\geq$0. Robust standard errors are clustered by individual. Significance levels: *=10%, **=5%, ***=1%. Mean for Turnaway mothers at initial survey (approximately 1 week after abortion encounter) reported in bottom row.

Table A7: Heterogeneous Effects for High Versus Low TANF Generosity States

	Full Sample	High Generosity States	Low Generosity States
Financial Distress Index			
Post × Turnaway	0.102**	0.036	0.150**
	(0.045)	(0.054)	(0.066)
Credit Access Index			
Post × Turnaway	-0.009	0.065	-0.065
	(0.054)	(0.061)	(0.082)
N:	4,914	2,158	2,756

Notes: Analyses use 2006-2016 Experian credit report files for Turnaway and Near Limit sample of women age 20 and older the year of the birth or counterfactual birth. Sample is restricted to women who had a credit report record prior to the birth or counterfactual birth. All regression models include individual fixed effects and an indicator that event time$\geq$0. Robust standard errors are clustered by individual. Significance levels: *=10%, **=5%, ***=1%.