Criminal Records, Family Formation and Violence Against Women *

Jeffrey Grogger† Hans Grönqvist‡ Susan Niknami§

February 12, 2026

Abstract

This paper studies the causal effects of public access to criminal records on partnership formation and violence against women, exploiting the unexpected introduction of aa online criminal background check platform in Sweden in 2014 that fundamentally transformed the way criminal background checks was conducted by enabling to the general public anonymous and free searches of all individuals charged with any type of crime. The database received extensive attention, generating approximately 100 million searches by 2019. Our research design isolates the informational content of a criminal record by exploiting court-specific disclosure cutoffs created by administrative rules that prevent the platform from accessing records older than about five years. Exploiting population-wide administrative registers containing complete criminal histories, we compare individuals whose records became publicly visible to otherwise similar individuals whose charges fell just outside the disclosure window.

We document substantial heterogeneity by baseline partnership status. Among men who are partnered at baseline, public listing leads to a rapid 7.8 percentage points decline in the probability of remaining partnered, with stronger effects in newly formed relationships and in couples where the female partner has relatively greater bargaining power. We find no statistically significant effect on violence against women in this group, but observe improvements in women's labor market outcomes, consistent with public listing enabling women to exit troubled relationships without increasing exposure to violence. In contrast, among men who are unpartnered at baseline, public listing reduces the probability of entering a partnership by 8.7 percentage points and increases the risk of committing violence against women by 4.1 percentage points. These effects cannot be explained by changes in reporting behavior or adverse labor market outcomes. Instead, we document increases in prescriptions for stress-related diagnoses and alcohol or drug use, as well as evidence of increased sorting toward women with a higher predicted risk of becoming victims of domestic violence. The effects on violence against women are muted among men who were treated for depression prior to listing, highlighting the role of psychological vulnerability and stress responses.

Taken together, the results reveal a trade-off in the public disclosure of criminal records. While public access appears to improve match quality and facilitate exit from harmful relationships for partnered women, it may also exacerbate psychological distress and violent behavior among unpartnered listed men. These findings inform debates on criminal record transparency, public safety, and the unintended consequences of stigma-based policies.

Keywords: *Background checks*; *Divorce*; *IPV*; *Family spillovers*; *Stigma*;
JEL Codes: K42; J12; J16

*We are grateful to comments from seminar participants at Erasmus University (Rotterdam) and Linnaeus University. Part of this work was completed while the authors visited the CEP at LSE. This work is funded by the Swedish Research Council and Handelsbankens forskningsstiftelser.

†University of Chicago Harris School of Public Policy. jgrogger@uchicago.edu

‡IFN, Linnaeus University, CESifo, RFBerlin. hans.gronqvist@ifn.se

§Stockholm University. susan.niknami@sofi.su.se

1 Introduction

Online access to personal information increasingly shapes intimate relationships. In recent years, individuals have gained unprecedented ability to search for information about potential or current partners, including criminal histories (Avast, 2021; NortonLifeLock and Poll, 2022). This development has intensified an ongoing policy debate over whether criminal records should be broadly accessible to the public.[1] Proponents argue that public disclosure enhances safety by enabling women to screen out potentially violent partners (Belur et al., 2022).[2] Standard marriage-market models predict that making a criminal record observable should reduce partnership formation and increase relationship dissolution, as a visible record acts as a negative signal of trustworthiness (Becker, 1973; Chiappori et al., 2017). Effective screening could therefore reduce intimate partner violence (IPV) by deterring or avoiding high-risk matches (Stevenson and Wolfers, 2007; Aizer, 2010). At the same time, public disclosure may also increase stigma, social exclusion, and psychological distress among listed individuals, raising stress and frustration—well-documented precursors of IPV (Western, 2015; Card and Dahl, 2011; Anderberg et al., 2016). If public listing reduces relationship prospects, damages existing unions, or intensifies social marginalization, it may inadvertently increase IPV. The net effect of criminal record transparency on partnership formation and violence is therefore theoretically ambiguous.

This paper studies the causal effects of public access to criminal records on partnership formation, dissolution, and intimate partner violence. Our analysis exploits a natural experiment in Sweden, where in January 2014, a private firm launched Lexbase, a public website that posted searchable summaries of individuals' criminal court records. While Swedish courts have long maintained such records, access prior to 2014 required in-person requests at local courthouses and was rarely used by private individuals. Lexbase fundamentally transformed access by enabling anyone to anonymously and instantly search, free of charge, whether a specific person had been charged with or convicted of a crime. The platform was rapidly adopted, attracting more than 30 million visits in its first year.

We identify the causal impact of public visibility by exploiting administrative disclosure rules that sharply limited the platform's access to older cases. Because records older than approximately five years were effectively inaccessible, individuals sentenced just after the disclosure cutoff were substantially more likely to appear in the database than otherwise similar individuals sentenced just before it. We implement a fuzzy regression discontinuity design comparing defendants on either side of this threshold.

We begin by documenting that the launch of the platform received massive public attention. In its first year of operation, the platform recorded approximately 32 million searches, while more than 1,200 articles in the printed press covered the database. Media coverage primarily focused on how the platform could be used to conduct background checks, often accompanied

[1] Many U.S. states permit online searches of arrest or conviction histories, reflecting a policy emphasis on transparency and information disclosure (Jacobs, 2015; Logan, 2009)

[2] This argument is reflected in policies that establish a right to ask or know about prior violent behavior, such as Clare's Law in the United Kingdom and Savanna's Law in Tennessee.

by anecdotal accounts from women stating that the web-site provides a crucial way for them to identify potentially dangerous men when dating,[3] and offenders concerned that the public revelation of their criminal history would adversely affect their future outcomes.[4] We further show that Google search interest in Lexbase consistently exceeded that of common benchmark terms such as "best restaurants" or "new car," underscoring the exceptional level of attention that the platform received by the general public.

We then use population-wide microdata to estimate the causal effects of public listing on family formation and violence against women. The sample consists of all first-time defendants aged 20 to 60 who were charged with a crime between 2007 and 2010 and whose cases fall within a 16-week window around the court-specific disclosure cutoff. We show that baseline characteristics of defendants, including original crime type and sentence severity, are balanced for individuals sentenced just before and just after the cutoff.

Our estimates reveal substantial heterogeneity by baseline partnership status. Among men who are partnered at baseline, public listing leads to a 7.8 percentage point decline in the probability of remaining partnered. The effect is stronger in newly formed relationships and in couples where the female partner has relatively greater bargaining power at baseline, as measured by relative earnings or years of schooling. The decline in partnership stability occurs quickly, concentrated within the first two years following the launch of the database. We find no statistically significant effect on violence against women in this group, but observe improvements in women's labor market outcomes. In particular, public listing increases women's probability of employment by 11.8 percentage points and reduces the probability of sickness leave by 11.7 percentage points. These findings are consistent with public listing enabling women to exit troubled relationships without increasing their exposure to violence.

In contrast, among men who are unpartnered at baseline, public listing reduces the probability of entering a partnership by 8.7 percentage points and increases the risk of committing violence against women by 4.1 percentage points. We find no differential effect on violence against women for charges that resulted in conviction compared to those that were dismissed, suggesting limited scope for changes in reporting behavior to explain the results. We also find no significant effects on men's own labor market outcomes, indicating that the increase in violence is unlikely to be driven by increased economic exclusion. Moreover, conditional on eventually forming a partnership, we find no significant effect on the time to partnership, suggesting that the overall decline reflects permanent exclusion from the marriage market rather than delayed matching.

To examine whether these effects are driven by changes in the composition of search markets, we proxy childhood social networks using indicators for whether partners attended the same high school or lived in the same neighborhood at age 16. We similarly proxy adult social networks by whether partners worked at the same workplace or lived in the same neighborhood three years prior to the launch of Lexbase. We find no significant effect of public listing on the likelihood of forming partnerships within either childhood or adult social networks. Taken

[3] https://www.svt.se/kultur/debattoren-soktjansterna-behovs-for-kvinnor-som-dejtar?

[4] `https://www.expressen.se/nyheter/deras-liv-forstors-av-lexbase/`

together with survey evidence indicating that most individuals at the time met partners either online or through their social networks, these results suggest that listing did not substantially alter the venues through which individuals searched for partners.

Instead, we document significant increases in prescriptions for stress-related diagnoses, as well as alcohol- and drug-related medications, among unpartnered listed men. These findings are consistent with criminological theories emphasizing that public stigmatization may exacerbate psychological distress and frustration, thereby increasing violent behavior. We also find evidence of increased sorting toward women with a higher predicted risk of becoming victims of domestic violence. Specifically, using baseline characteristics, we predict the probability that a woman experiences domestic violence over a two-decade horizon and show that public listing increases the probability that a future partner belongs to the top quartile of the predicted risk distribution by 15.8 percentage points. At the same time, listing reduces the probability that a future partner has completed high school by 6.1 percentage points. These patterns are consistent with listed men disproportionately matching with more vulnerable partners. Finally, the effects on violence against women are muted among men who were treated for depression prior to listing, highlighting the role of psychological vulnerability and stress responses. All results are robust to an extensive set of specification checks, including placebo tests in the pre-policy period, tests for bunching and monotonicity, and alternative bandwidth choices.

Taken together, the results reveal a fundamental trade-off in the public disclosure of criminal records. While public access appears to improve match quality and facilitate exit from harmful relationships for partnered women, it may also exacerbate psychological distress and violent behavior among unpartnered listed men. These findings inform debates on criminal record transparency, public safety, and the unintended consequences of stigma-based policies.

Our results contribute to several strands of the literature. First, most empirical work building on Becker's (Becker, 1973, 1974) model has focused on responses to income shocks and changes in economic conditions (Aizer, 2010; Chiappori et al., 2017). A central challenge in this literature has been to distinguish sorting on observed characteristics from sorting on correlated but unobservable aspects of match quality. A closely related challenge is to disentangle whether union dissolution reflects exogenous shocks or poor underlying match quality. In contrast, we study an informational signal of trustworthiness in a quasi-experimental setting that allows us to hold underlying match quality constant. Importantly, our research design accounts not only for correlated unobservables but also for the effects of formal sanctions (e.g., prison sentences), enabling us to isolate the pure informational effect of public listing in a criminal records database.

Second, we contribute to the literature on the relationship between criminal justice contact and the marriage market. Most existing studies focus on how incarceration affects the supply of potential partners and union stability at the aggregate level (Lopoo and Western, 2005; Apel, 2013; Fallesen and Andersen, 2017; Siennick et al., 2014; Charles and Luoh, 2010; Svarer, 2011). Far fewer studies examine how the revelation of criminal background information at the individual level affects family formation (one exception is Svarer, 2011), and existing evidence

is largely descriptive. To our knowledge, no prior study uses a design-based research strategy to identify the causal effects of public criminal record disclosure on partnership outcomes.

Third, we add to a growing literature on the determinants of violence against women and intimate partner violence (IPV) (Aizer, 2010; Anderberg et al., 2016; Calabresi and Rodríguez-Planas, 2025; Bhuller et al., 2024). While this literature has examined economic conditions, policy changes, and institutional responses, there is no prior evidence on how public access to criminal background information affects violence against women. Our findings therefore provide novel evidence on an important but previously unexplored policy margin.

More broadly, our paper also extends the literature on the extra-judicial consequences of criminal records. Like the work on the economics of marriage, this literature has focused mostly on the labor market. Holzer et al. (2006a) surveyed firms to learn how employer-initiated background checks affected hiring. They found that a criminal background check actually raised the likelihood that an employer would hire a Black applicant, suggesting that employers statistically discriminated in the absence of background checks. A number of studies have since analyzed the effect of ban-the-box laws, which prevent employers from asking job applicants about their criminal records in the early stages of the hiring process (Agan and Starr, 2018; Craigie, 2019; Doleac and Hansen, 2018; Jackson and Zhao, 2016; Rose, 2021). These studies generally find that criminal records worsen employment prospects, but that masking them does not improve employment outcomes.[5] Our paper extends this work to individuals seeking partners, who may face different constraints and have different preferences from firms seeking workers.

In the next section, we provide more background on the setting, including the online publication by Lexbase of abbreviated criminal records for virtually the entire Swedish population. We then discuss how the unexpected release of those data, together with Swedish laws limiting the length of time that criminal records are retained, set up our RDD design. The next section discusses the data, followed by our results. The final section concludes.

2 Background

2.1 Prior literature on the economics of marriage

The empirical literature on the economics of marriage began with Becker et al. (1977), who analyzed marriages in cross-sectional data. They found that divorce probabilities were higher at both higher and lower levels of income. Since variation in the tails is more likely to be the effect of surprises than it is in the middle of the distribution, they inferred that their results were due to shocks, but could not rule out a role for match quality. Weiss and Willis (1997) analyzed divorce rates using longitudinal data, potentially letting them control for initial match quality, but focused on differences between income realizations and predictions from an auxiliary earnings regression, rather than true shocks. Charles and Stephens (2004) also analyzed longitudinal

[5] Exceptions include Kaestner and Wang (2024), who find that ban-the-box laws have no effect on employment, and Shoag and Veuger (2021), who report favorable effects in high-crime areas.

data, focusing on divorce responses to layoffs. This approach may distinguish the effects of shocks from the effects of match quality if the layoffs are unanticipated. Two studies, by Cesarini et al. (2023) and Hankins and Hoekstra (2011) study lottery winners. Such winnings have the advantage of providing exogenous variation in income shocks, but they are relatively rare and involve only individuals who participate in lotteries.

Although much of the empirical work on the economics of marriage has focused on interactions between marriage and the labor market, other factors may affect partners' perceptions of their gains from union as well. Charles and Stephens (2004) observe that layoffs increase divorce risk, but that disabilities with similar implications for income do not. This leads them to conclude that surprises about non-economic aspects of one's partner's suitability as a mate may be more important than the financial losses entailed by a job loss in raising divorce risk.

One such non-economic aspect of a partner's suitability may entail their law-abidingness, or lack thereof. A handful of studies focuses on links between crime, dating, and marriage.[6] Svarer (2011) analyzes population register data from Denmark, and reports that criminal convictions reduce the likelihood of forming a union with a partner from a better-off family. van Schellen et al. (2012) study official records from the Netherlands, and find that convictions both reduce the likelihood of marrying, and among those who marry, raise the likelihood of marrying someone else with a criminal record. They also report that dissolution risk is higher among men with convictions. Evans et al. (2025) conducted a street-intercept survey, asking respondents whether they would be willing to date prospective matches with various types of criminal records. They report that most respondents were seriously opposed to dating someone with a criminal record, especially for a serious offense. Evans and Vega (2021) conducted a correspondence experiment on a online dating service to gauge how prospective matches reacted to information about a criminal record on one's profile. Results were curiously positive for white males, but negative for Black males.

Most of these findings are broadly consistent with what Becker's model, in some cases augmented for a criminal past, would predict. However, most struggle to disentangle assortative mating on observable characteristics, or the role of shocks in dissolutionos, from unobserved aspects of match quality. The studies that are most successful along these lines tend to have been drawn from special populations, to be based on small or potentially unpresprentative samples, or to analyze stated preferences rather than actual behavior. In our analysis below, we make use of a large sample of data from national population registers, observe actual formations and dissolutions of unions, and deploy an RDD to deal with potential confounding due to observed match quality.

[6]We distinguish the household-level studies discussed here from several market-level studies that estimate how widespread incarceration, by reducing the sex ratio, affects marriage. These studies include Charles and Luoh (2010), Liu (2019) and Caucutt et al. (2021).

2.2 Criminal background checks

In most countries, criminal background checks are used by agents such as employers, landlords, and higher education institutions to address concerns surrounding public safety and the notion that individuals with criminal histories may be less inclined to fulfill their responsibilities. Additionally, these checks may be employed to mitigate potential liability, particularly in the realms of negligent hiring or negligent renting doctrines.[7] The criminal background are compiled from various public sources, such as law enforcement agencies and state courts, or via web scraping technology from public websites (NLCS, 2019). The information provided to consumers tends to be accessible through various channels, including public agencies and, increasingly, private companies, especially in the United States. A background check typically reports the individual's name associated with the record, the jurisdiction of origin, the record's creation date, and a case number or law enforcement identifier (NLCS, 2019). While a brief description of the offense, such as "possession of marijuana", may be provided, details about the offense or related circumstances are usually omitted. Some public sources only display criminal convictions, whereas private firms may report arrests for misdemeanors that later result in dismissal (Bushway and Sweeten, 2007).

2.2.1 The Swedish setting

The regulations governing criminal record checks in Sweden have traditionally been stringent, primarily restricted to employment-related purposes (Backman, 2012). The general public has not been permitted to conduct searches beyond requesting their own records. Employers are limited in their ability to request a candidate's criminal record, which is typically permitted only at the concluding stages of the hiring process. Subsequently, the job applicant is required to formally request his criminal record from the police, who dispatches it in a sealed envelope. Upon receipt of the record, the applicant must then forward the envelope directly to the employer. This protocol often results in a significant time delay, with the entire process spanning several weeks. It is therefore not surprising that survey data from a large sample of employers collected in 2011 indicates a relatively low utilization of criminal background checks by employers in Sweden, with only approximately 14 percent of the 1,200 employers reported to have engaged in such practices (SOU, 2011). This conclusion is also supported by Figure 11 showing that only about two percent of the job ads posted on the Public Employment Office's in 2011 mentioned criminal background checks. Consistent with this, the same year the ratio of the number of official background checks to the number of firms was 17 percent. Since then, there has been a slowly growing trend towards increased use of official background checks.[8]

[7] About 94 percent of employers in the United States conduct some form of criminal history check, and about 90 percent of landlords run background checks on prospective tenants (NLCS, 2019) In 2004, a survey from the United States showed that about 50 percent of the employers check the criminal background of job applicants (Holzer et al., 2006b)

[8] One key aspect is Swedish law surrounding criminal record access for employers that the limits it sets on the length of time that information is retained. Official criminal records, excluding Lexbase, are expunged after 3-10 years. The exact amount of time depends on factors such as the age of the offender when committing the crime and the sentence. We elaborate further on these details below, when we discuss our RDD design.

2.2.2 The Lexbase database

Lexbase was officially established as a business entity in 2012. During the two years preceding the public launch of the database, the company began acquiring criminal background data through direct engagement with individual courts, requesting access to criminal charges stored in the digital administrative system known as Vera. A government decree mandated courts to grant access to Vera to anyone seeking information on criminal charges for a given individual.[9]

With the release of its website in January 2014, Lexbase completely transformed the landscape of criminal background checks in Sweden. Instead of targeting solely employers, the platform offered unrestricted access to criminal records for the general public. The database allowed anyone to anonymously search individuals by name, leveraging data sourced from the records maintained by the courts. The search algorithn links the name of the searched individual to their current age and place of residence and reveals whether that person has been charged with a crime. This means that even for common names it is typically possible to uniquely identify an individual.[10]

Unlike many other online criminal databases, which specialize in specific crime types or cater exclusively to certain actors such as employers or landlords, the Lexbase platform presents a comprehensive repository of every criminal charge in Swedish courts. The basic search, which simply indicates whether the subject of the search has any criminal record, is free to the consumer. For a nominal fee, registered users may also obtain full court proceedings, gaining access to information about the number of charges, conviction and crime date, type of crime, circumstances surrounding the offense, the sentence, and the address and personal id number (cf. social security number) of the individual.[11].

The database contains records on acquittals as well as convictions.[12] Appendix Figure 7 show

[9] See decree (2001:639).

[10] It is also possible to search using the personal identity number (cf. SSR). The personal identity number is a unique identifier assigned to each resident at birth or upon immigration. It is used across virtually all areas of society—healthcare, taxation, banking, education, employment, social insurance, and many private services. Its use is universal and deeply integrated into both public and private systems, making it one of the most widely used personal identification numbers in the world. In Sweden, anyone can request a person's personal identity number from the Tax Agency by providing the individual's full name, unless the information is protected by confidentiality rules.

[11] Despite its low cost, acquiring the complete court proceedings seems to have been a rare occurrence. Data from the database's company indicate that 0.16 % of all background searches actually resulted in a purchased court proceeding in the first year (see Appendix Figure 12)

[12] The following text is a translation of the official press release from the company sent on the day of the launch: "All criminal convictions from the last five years are now easily searchable - New unique site is launched today. Today, a beta version of a new site, Lexbase.se, is being launched, which makes all criminal convictions, five years or younger, easily searchable. Lexbase will be Sweden's largest database with legal information and a new ground-breaking service on the Swedish market." (https://www.lexbase.se/site/press/21?s=285b36b14745ae4aedb1d218ea9989c4). Reports in the media also highlighted that only criminal convictions were included. For instance, the following text is a translation of parts of a report in one of Sweden's largest newspapers called Expressen. "At Lexbase you could find out everything about the convictions of your friends, neighbors and acquaintances. Is the neighbor convicted of rape? Or maybe just for speeding? The only thing you needed to find the convictions was their name or social security number. The convictions were exposed as red dots on a map and could then be downloaded for a small fee. The new site was launched at a high-profile press conference on Monday. Already on the same day, lots of curious people started logging in, while harsh criticism was immediately directed at Lexbase. And immediately the first reports from

the homepage of the platform and Appendix Figure 8 shows the different stages in the search process.

The platform received immediate media attention and gained immense popularity. Data provided by Lexbase show that 32 million background checks were conducted in just the first year. Appendix Figure 12 shows that by 2019, when the first competing platform was introduced, the cumulative number of searches had soared to 103 million. Appendix Figure 9 shows the sharp increase in the number of media articles written in printed press mentioning the database. Appendix Figure 10 shows that from the week after its inception and up until 2019, Lexbase consistently outnumbered other popular Google search terms in Sweden, surpassing common queries such as "new car" or "best restaurants".

Since the platform did not have any competitors until 2019, the massive use of the database cannot be explained by a shift from other similar platforms. It is also seems unlikely that employers substituted away from official background checks by the police to using the platform instead. First, the introduction of the platform did not coincide with any meaningful shift in the long-run trend towards increased use of official criminal background checks (see Appendix Figure 11). Second, the demand for regular background checks was relatively low among employers. As discussed earlier, a large survey of 1,200 firms just before the introduction of the platform revealed that 14 percent of employers reported that they checked criminal records (SOU, 2011) and only two percent of the job ads posted on the Public Employment Office's mentioned criminal background checks. Third, the number of official background checks, 171,229 in 2011, is completely dwarfed by the roughly 20 million searches conducted every year on the platform. Taken together, the extensive use of the platform is most likely explained by a high demand for the service by the general public.[13]

Although the information provided by the database in theory was accessible through public records even before 2014, the process was laborious, necessitating visits to multiple courts to potentially find a matching name. The introduction of this particular platform represented a significant reduction in the cost of accessing criminal background information, especially for the general public.

desperate people came in to the Chancellor of Justice (JK). "I was so sick I couldn't go to work. I'm in that database. I've done everything to get back to a normal life and have been holding a job for the past two years. If someone searches for me, I'll lose my job immediately. You have to do something about this. My neighbors can see my house on a map with a red dot. I'm totally stressed out now by this! I feel so damn bad I might kill myself," one man writes to JK ." (https://www.expressen.se/nyheter/deras-liv-forstors-av-lexbase/)

[13]The platform was closed down by the Internet service provider just two days after its launch, after the site came under heavy criticism from Swedish media and the general public for making criminal records too easily accessible. However, since April 2014 the platform has been fully operational. Several attempts by the Swedish government to shut town the platform have failed because the information is protected by the Swedish constitution.

3 Retention limits and the RDD

A critical element of our RDD is the legal retention limit for criminal records, which in combination with the Lexbase release date determines assignment to treatment. In principle, courts are required to expunge criminal records after five years.[14] This requirement pertains not to Lexbase, but to the underlying Vera system from which Lexbase obtains data. Thus in principle, Lexbase's requests should have resulted in criminal records extending back five years from the date the request was made.

Assuming for simplicity that five years of criminal history records were made available when Lexbase was released in January 2014, Figure XX helps to illustrate our design. With a five-year retention limit, any offenses committed prior to the cutoff date of January 2009 would not have appeared in Lexbase, and thus would not have been available to anyone visiting the website. In contrast, any offenses committed after the January 2009 cutoff date would have appeared in Lexbase upon its January 2014 release.

Thus our treatment group consists of all individuals who offended within a short interval after the cutoff date. Our comparison group consists of individuals who offended within a similarly short interval prior to the cutoff date. The figure illustrates treatment and comparison groups based on an interval of 32 weeks, on which we base our main results.[15] If the intervals are short enough, the only systematic difference between the treatment and control groups should be the fact that the treatment group's offenses were searchable in Lexbase, whereas the control group's offenses were not. The RDD exploits this difference to identify the effects of a criminal record on marriage and similar unions, as well as related outcomes, over the January 2014-December 2018 follow-up period.

In practice, the massive scale of the Lexbase data request, variation in dates at which those requests were made, and some vagaries surrounding the retention limits, created variation among courts in effective cutoff dates. The specific cutoff date for each court was determined by the timing of Lexbase's request for information, the length of time it took the court to process the request, and the exact expungement procedures followed by the court.

To estimate the cutoff dates, we obtained data from Lexbase on the number of cases added to the database for each court in each month. We used these data, together with nationwide register data on the near-universe of all criminal charges in Sweden since 1986, to estimate the share of cases included in the Lexbase data for each court and month.[16] We then computed the change in the share of included cases each month by calculating the difference between the average share of cases in the preceding 12 months and the average share in the subsequent 12 months. We estimated the cutoff date as the month exhibiting the largest increase in the share

[14] The courts were still obliged to preserve cases in printed form. In order to search for a specific case a person had to visit the court to manually search for the case number in a volume of books that compiles all the cases that have been processed by the court.

[15] We use this interval for our main results, but experiment with other intervals as well to check robustness.

[16] The criminal justice registers do not include offenses by persons without national ID numbers, who are mostly immigrants.

of included cases over the entire period.[17]

Figure 1 shows the average share of cases included in the database around the estimated cutoff for the courts in our sample. As one might expect, the share of included cases does not jump from zero to one at the estimated cutoff month. However, there is a clear discontinuity in the share of cases, with the an estimated jump at the cutoff of 0.64 (0.13). Figure **??** show the cutoff for each court in our sample.[18]

Accounting for cutoff dates that vary by court, the regressions we use to implement our RDD can be written as

$$D_{ij} = \alpha_0 + \alpha_1 Z_{ij} + \alpha_2 W_{ij} + \alpha_3 W_{ij} Z_{ij} + \varepsilon_{ij} \tag{1}$$

$$Y_{ij} = \beta_0 + \beta_1 Z_{ij} + \beta_2 W_{ij} + \beta_3 W_{ij} Z_{ij} + u_{ij} \tag{2}$$

The variable D_{ij} is a binary indicator equal to one if individual i has a criminal record that appears in Lexbase, and equals zero otherwise. The variable Y_{ij} is an outcome, such as marital status, for the ith individual covered by the jth court. The running variable Z_{ij} is the number of days between individual i's offense and the cutoff date for court j. The dummy variable $W_{ij} = 1(Z_{ij} > 0)$, which equals one if i is in the treatment group and equals zero if he is in the comparison group. The terms ε_{ij} and u_{ij} are unobserved disturbance term.

The α and β terms are coefficients to be estimated. Equation (1) is the first stage regression, and the coefficient α_2 shows the difference in the likelihood of being included in the Lexbase database just after the release date. We expect $\alpha_2 < 1$ for two reasons. The first has to do with possible measurement error in the estimated cutoff date, as discussed above. The second is that members of the comparison group may reoffend at some point between the cutoff date and the Lexbase release date. In this case, they will have an offense that is visible in Lexbase on the release date and $D_{ij} = 1$.

Equation (2) is the reduced form. The coefficient β_2 shows the intent-to-treat effect of offending (for the first time) during the period immediately following the cutoff date. Provided that W_{ij} is uncorrelated with ε_{ij}, β_2 should identify the intent-to-treat effect of a criminal record on the outcome variable. That is, if W_{ij} is driven solely by the running variable, then OLS applied to

[17] In the empirical analysis we focus on the 32 courts with observed cutoff dates before 2010 (XX to ensure the representation of all courts in our sample within the database at its launch.XX) It is unclear whether all courts were actually included in the database at the time it was launched. Thus, it is possible that some courts were added at a later point in time. Additionally, we excluded four courts with no clear evidence of cutoff. These courts, which represent 7.5 percent of the total number of cases, are DC Umeå, DC Gällivare, DC Haparanda, DC Värmland.

[18] When interpreting this figure it should be noted that the estimates are affected by measurement error. First, our criminal history register does not include the complete number of cases that exist in Lexbase since we miss information on individuals that are lacking a personal id number. Most of these individuals are asylum seekers. Second, in some cases, courts accidentally delivered not only criminal cases but also civil cases that were not made searchable on the platform. Data provided by Lexbase show that 20.81 percent of the total number of records delivered were civil cases.

equation (2) should consistently estimate the reduced-form effect of a criminal record.

In practice, our regressions also control for the court-specific cutoff date. Some specifications add higher-order terms in W_{ij} and Z_{ij}, whereas others incorporate various control variables constructed from Swedish population registers. We discuss those registers and the variables we construct from them in the next section.

4 Data and sample construction

4.1 Administrative data

Our analysis uses population-wide administrative data covering the entire Swedish population from 2000 to 2018. The data holds demographic and socioeconomic information such as age, place at birth, gender, educational level, employment and income measures. The estimation sample consists of all individuals charged with a crime between 2007 and 2010 whose cases fall within a 16-week window around the relevant court-specific disclosure cutoff. We restrict the sample to first-time defendants and to ensure comprehensive tracking of the for outcomes, we restrict the sample to individuals aged 20 to 60 in the year of the initial charge. Our measure of partnership status follows the standard used by Statistics Sweden as being married or cohabiting with shared children in any year between 2014 and 2018.

These data have been linked to the Swedish Conviction Register maintained by the National Council for Crime Prevention (*Brottsförebyggande rådet* - BRÅ). Within these records, we have access to comprehensive details concerning criminal charges during this period. The data include information on the date of the conviction as well as the sentence. This information pertains to criminal charges in Swedish district courts, which is the court of first instance. A single criminal charge may encompass multiple crimes, and we observe all crimes within a given charge.[19] The data also include information on acquittals. Violence against women is measured as the male being recorded by the prosecutor—following investigation—as a plausible suspect of violence against a female in a close relationship in any year between 2014 and 2018. This definition includes violence against both current and former partners as well as adult female relatives. Appendix B describes how the variables in our analysis are constructed.

4.2 Classifying offenders

Table 1 columns (1) and (2) shows descriptive statistics for our sample separately for exposed and non-exposed defendants. These baseline characteristics are measured in one year before the initial trial. As expected, the background characteristics of the individuals are very similar in the two groups. We can also see that there are striking similarities in terms of the case characteristics and the sentence. This does not only show that the covariates in the two groups

[19]The data exclude minor offenses such as speeding tickets but include offenses such as driving without a license and DUI.

balance but also that our research design should be able to identify the role of stigma net of that from formal sanctions.

5 Results

This section presents the results from our empirical analysis. We start by providing the main results and robustness. We then show results for relevant subgroups of the population. We end by examining some of the underlying mechanisms.

5.1 First stage and specification checks

Panel A of Figure **??** illustrates the first stage. For members of the comparison group, the probability of appearing in Lexbase averages about 0.20, indicating that roughly 20 percent of individiuals who offended in the during the 16 weeks prior to the cutoff date also reoffended at some point between the cutoff and the release date. The slight downward slope arises because individuals who offended closer to the cutoff date had a shorter period prior to the release date during which they were at risk of re-offending.

At the cutoff date, the probability of appearing in Lexbase rises substantially. As seen in column (2) of Table , our estimate of α_2 is 0.657 (0.13). This indicates that the likelihood of appearing in Lexbase is 66 percentage points higher for the treatment group than the comparison group. This is a very strong first stage, as indicated by the Sanderson-Windmeijer F-statistic of 654.9 and the Cragg-Donald F-statistic of 4569.7 Column (3) shows that adding covariates to equation (1) has little effect on the estimated first stage.

Column (1) of Table **??** reports results from a regression of treatment status D_{ij} on the covariates that appear in Table (1). Several of these variables predict appearance in Lexbase, including sex, age, household structure in 2013, education, earnings, and conviction type. Several of these variables are highly significant, and the joint F-statistic for the regresion has a p-value of zero to five decimal places. Column (2) of Table **??** tests the exogeneity condition by regressing the assignment-to-treatment variable W_{ij} on the same variables. Only one coefficient rejects the null hypothesis at the 10 percent level, and none reject at the 5 percent level. The joint F-statistic equals 0.546. These results are consistent with the exogeneity assumption.

Figure XX displays the estimated density of offenses about the the cutoff date. There is no visible evidence of manipulation. The XX statistic for McCrary's test of manipulation about the threshold is XX.

5.2 Main results

We begin by documenting that the launch of the platform generated a substantial increase in public attention. In its first year of operation, the platform recorded approximately 32 million

searches, while more than 1,200 articles in the printed press covered the database. Media coverage primarily focused on how the platform could be used to conduct background checks, often accompanied by anecdotal accounts from individuals concerned that the public revelation of their criminal history would adversely affect their future outcomes.[20] We further show that Google search interest in Lexbase consistently exceeded that of common benchmark terms such as "best restaurants" or "new car," underscoring the exceptional level of public attention the platform received.

We then use population-wide microdata to estimate the causal effects of public listing on family formation and violence against women. The sample consists of all first-time defendants aged 20 to 60 who were charged with a crime between 2007 and 2010 and whose cases fall within a 16-week window around the court-specific disclosure cutoff. Partnership status is defined as being married or cohabiting with shared children in any year between 2014 and 2018. Violence against women is defined as the male being recorded by the prosecutor—following investigation—as a plausible suspect of violence against a female intimate partner in any year between 2014 and 2018. We show that baseline characteristics of defendants, including original crime type and sentence severity, are balanced for individuals sentenced just before and just after the cutoff. Being sentenced after the cutoff increases the probability of being publicly listed in the database by 0.719 (s.e. = 0.029).

Our results reveal substantial heterogeneity by baseline partnership status. Among men who are partnered at baseline, public listing leads to a 7.8 percentage point decline in the probability of remaining partnered. The effect is stronger in newly formed relationships and in couples where the female partner has relatively greater bargaining power, as measured by relative earnings or years of schooling. The decline in partnership stability occurs quickly, concentrated within the first two years following the launch of the database. We find no statistically significant effect on violence against women in this group, but observe improvements in women's labor market outcomes. In particular, public listing increases women's probability of employment by 11.8 percentage points and reduces the probability of sickness leave by 11.7 percentage points. These findings are consistent with public listing enabling women to exit troubled relationships without increasing their exposure to violence.

In contrast, among men who are unpartnered at baseline, public listing reduces the probability of entering a partnership by 8.7 percentage points and increases the risk of committing violence against women by 4.1 percentage points. We find no differential effect on violence against women for charges that resulted in conviction compared to those that were dismissed, suggesting limited scope for changes in reporting behavior to explain the results. We also find no significant effects on men's own labor market outcomes, indicating that the increase in violence is unlikely to be driven by increased economic exclusion. Moreover, conditional on eventually forming a partnership, we find no significant effect on the time to partnership, suggesting that the overall decline reflects permanent exclusion from the marriage market rather than delayed matching.

[20] `https://www.expressen.se/nyheter/deras-liv-forstors-av-lexbase/`

5.3 Further robustness checks and additional results

5.4 Mechanisms

To examine whether these effects are driven by changes in the composition of search markets, we proxy childhood social networks using indicators for whether partners attended the same high school or lived in the same neighborhood at age 16. We similarly proxy adult social networks by whether partners worked at the same workplace or lived in the same neighborhood three years prior to the launch of Lexbase. We find no significant effect of public listing on the likelihood of forming partnerships within either childhood or adult social networks. Taken together with survey evidence indicating that most individuals at the time met partners either online or through their social networks, these results suggest that listing did not substantially alter the venues through which individuals searched for partners.

Instead, we document significant increases in prescriptions for stress-related diagnoses, as well as alcohol- and drug-related medications, among unpartnered listed men. These findings are consistent with criminological theories emphasizing that public stigmatization may exacerbate psychological distress and frustration, thereby increasing violent behavior. We also find evidence of increased sorting toward women with a higher predicted risk of becoming victims of domestic violence. Specifically, using baseline characteristics, we predict the probability that a woman experiences domestic violence over a two-decade horizon and show that public listing increases the probability that a future partner belongs to the top quartile of the predicted risk distribution by 15.8 percentage points. At the same time, listing reduces the probability that a future partner has completed high school by 6.1 percentage points. These patterns are consistent with listed men disproportionately matching with more vulnerable partners. Finally, the effects on violence against women are muted among men who were treated for depression prior to listing, highlighting the role of psychological vulnerability and stress responses. All results are robust to an extensive set of specification checks, including placebo tests in the pre-policy period, tests for bunching and monotonicity, and alternative bandwidth choices.

5.4.1 Family formation

5.4.2 Violence against women

6 Concluding Remarks

We study the unexpected launch of Sweden's first online criminal records database in 2014, which provided unrestricted, anonymous, and cost-free public access to the criminal histories of all individuals charged with a crime. To estimate the causal effects of public access to criminal records, we combine rich population-wide administrative data with an institutional feature of the platform's design: administrative disclosure rules restricted the company behind the database from accessing records sentenced before specific cutoff dates. This constraint

generates quasi-random variation in public exposure to criminal records, allowing us to isolate the informational effect of public listing from both underlying individual characteristics and formal criminal sanctions.

We document substantial heterogeneity by baseline partnership status. Among men who are partnered at baseline, public listing leads to a rapid 7.8 percentage points decline in the probability of remaining partnered, with stronger effects in newly formed relationships and in couples where the female partner has relatively greater bargaining power. We find no statistically significant effect on violence against women in this group, but observe improvements in women's labor market outcomes, consistent with public listing enabling women to exit troubled relationships without increasing exposure to violence. In contrast, among men who are unpartnered at baseline, public listing reduces the probability of entering a partnership by 8.7 percentage points and increases the risk of committing violence against women by 4.1 percentage points. These effects cannot be explained by changes in reporting behavior or adverse labor market outcomes. Instead, we document increases in prescriptions for stress-related diagnoses and alcohol or drug use, as well as evidence of increased sorting toward women with a higher predicted risk of becoming victims of domestic violence. The effects on violence against women are muted among men who were treated for depression prior to listing, highlighting the role of psychological vulnerability and stress responses.

Taken together, the results reveal a trade-off in the public disclosure of criminal records. While public access appears to improve match quality and facilitate exit from harmful relationships for partnered women, it may also exacerbate psychological distress and violent behavior among unpartnered listed men. These findings inform debates on criminal record transparency, public safety, and the unintended consequences of stigma-based policies.

References

Agan, A.Y., Starr, S.B., 2018. Ban the box, criminal records, and racial discrimination: A field experiment. The Quarterly Journal of Economics 133, 191–235. doi:10.1093/qje/qjx028.

Aizer, A., 2010. The gender wage gap and domestic violence. American Economic Review 100, 1847–59. URL: https://www.aeaweb.org/articles?id=10.1257/aer.100.4.1847, doi:10.1257/aer.100.4.1847.

Anderberg, D., Rainer, H., Wadsworth, J., Wilson, T., 2016. Unemployment and domestic violence: Theory and evidence. Economic Journal 126, 1947–1979.

Apel, R., 2013. Sanctions, perceptions, and crime: Implications for criminal deterrence. Journal of quantitative criminology 29, 67–101.

Avast, 2021. 50% of online dating services users research their date before their first face-to-face encounter. URL: https://www.prnewswire.com/news-releases/50-of-online-dating-services-users-research-their-date-before-their-first-face-to-face-e html. survey of 15,233 Avast users from 15 countries about online dating safety and research behavior before first dates.

Backman, C., 2012. Criminal records in sweden: Regulation of access to criminal records and the use of criminal background checks by employers .

Becker, G.S., 1973. A theory of marriage: Part i. Journal of Political Economy 81, 813–846. doi:10.1086/260084.

Becker, G.S., 1974. A theory of marriage: Part ii. Journal of Political Economy 82, S11–S26. doi:10.1086/260170.

Becker, G.S., Landes, E.M., Michael, R.T., 1977. An economic analysis of marital instability. Journal of Political Economy 85, 1141–1187.

Belur, J., Tompson, L., Thornton, A., Bowers, K., 2022. Does disclosure of police information reduce domestic abuse? a systematic review of clare's law. Criminology & Criminal Justice 22, 87–108.

Bhuller, M., Dahl, G.B., Lø ken, K.V., Mogstad, M., 2024. Domestic violence reports and the mental health and well-being of victims and their children. Journal of Human Resources 59, S152–S186. URL: https://doi.org/10.3368/jhr.1222-12698R1, doi:10.3368/jhr.1222-12698R1. this study uses Norwegian administrative data to examine the effects of domestic violence reports on mental health and other outcomes for both victims and their children.

Bushway, S.D., Sweeten, G., 2007. Abolish lifetime bans for ex-felons. Criminology & Pub. Pol'y 6, 697.

Calabresi, E., Rodríguez-Planas, N., 2025. Female Empowerment and Intimate Partner Violence. IZA Discussion Paper 18207. IZA – Institute of Labor Economics. URL: https://www.iza.org/publications/dp/18207. discussion paper overviewing economic research on IPV and female empowerment.

Card, D., Dahl, G.B., 2011. Family violence and football: The effect of unexpected emotional cues on violent behavior. Quarterly Journal of Economics 126, 103–143.

Caucutt, E.M., Guner, N., Rauh, C., 2021. Incarceration, unemployment, and the racial marriage divide. BSE Working Paper 1300, November 2021. Available at: https://bse.eu/research/working-papers/racial-marriage-divide.

Cesarini, D., Lindqvist, E., Östling, R., Terskaya, A., 2023. Fortunate families? the effects of wealth on marriage and fertility. URL: http://www.nber.org/papers/w31039. nBER Working Paper No. 31039.

Charles, K.K., Luoh, M.C., 2010. Male incarceration, the marriage market, and female outcomes. The Review of Economics and Statistics 92, 614–627.

Charles, K.K., Stephens, M.J., 2004. Job displacement, disability, and divorce. Journal of Labor Economics 22, 489–522. doi:10.1086/381258.

Chiappori, P.A., Salanié, B., Weiss, Y., 2017. Partner choice, investment in children, and the marital college premium. Journal of Political Economy 125, 1791–1837.

Craigie, T.A., 2019. Ban the box, convictions, and public employment. Economic Inquiry 58, 425–445. doi:10.1111/ecin.12837.

Doleac, J.L., Hansen, B., 2018. The unintended consequences of "ban the box": Statistical discrimination and employment outcomes when criminal histories are hidden. Journal of Labor Economics URL: https://www.jenniferdoleac.com/wp-content/uploads/2018/09/Doleac_Hansen_JOLE_preprint.pdf. forthcoming; Texas A&M University Working Paper.

Evans, D.N., Begum, P., Porter, J.R., 2025. Love in the time of stigma: Public interest in relationships following criminal conviction. Journal of Crime and Justice 48, 96–112. doi:10.1080/0735648X.2024.2353236.

Evans, D.N., Vega, A., 2021. Experimental analysis of male online dating on parole. Journal of Crime and Justice 44, 33–48. doi:10.1080/0735648X.2020.1781678.

Fallesen, P., Andersen, L.H., 2017. Explaining the consequences of imprisonment for union formation and dissolution in denmark. Journal of Policy Analysis and Management 36, 154–177. doi:10.1002/pam.21933.

Hankins, S., Hoekstra, M., 2011. Lucky in life, unlucky in love? the effect of random income shocks on marriage and divorce. Journal of Human Resources 46, 403–426.

Holzer, H.J., Raphael, S., Stoll, M.A., 2006a. Perceived criminality, criminal background checks, and the racial hiring practices of employers. Journal of Law and Economics 49, 451–480.

Holzer, H.J., Raphael, S., Stoll, M.A., 2006b. Perceived criminality, criminal background checks, and the racial hiring practices of employers. The Journal of Law and Economics 49, 451–480.

Jackson, O., Zhao, B., 2016. The effect of changing employers' access to criminal histories on ex-offenders' labor market outcomes: Evidence from the 2010–2012 massachusetts cori reform. URL: https://www.bostonfed.org/-/media/Documents/Workingpapers/PDF/wp1630.pdf. federal Reserve Bank of Boston Working Paper No. 16-30.

Jacobs, J.B., 2015. The Eternal Criminal Record. Harvard University Press, Cambridge, MA.

Kaestner, R., Wang, X., 2024. Ban-the-box laws: Fair and effective? International Review of Law & Economics 78, 106192. doi:10.1016/j.irle.2024.106192.

Liu, S., 2019. Incarceration of african american men and the impacts on women and children. URL: https://sitianliu.github.io/papers/siu_2019_no_appendix.pdf. unpublished Manuscript.

Logan, W.A., 2009. Knowledge as Power: Criminal Registration and Community Notification Laws in America. Stanford University Press, Stanford, CA.

Lopoo, L.M., Western, B., 2005. Incarceration and the formation and stability of marital unions. Journal of Marriage and Family 67, 721–734. doi:10.1111/j.1741-3737.2005.00165.x.

NortonLifeLock, Poll, T.H., 2022. 2022 Cyber Safety Insights Report: Global Results. Technical Report. NortonLifeLock Inc. URL: https://www.nortonlifelock.com/content/dam/nortonlifelock/pdfs/reports/2022_NLCSIR_Global_Report.pdf. report based on an online survey of 10,003 adults in 10 countries, January 2022.

Rose, E.K., 2021. Does banning the box help ex-offenders get jobs? evaluating the effects of a prominent example. Journal of Labor Economics 39, 79–113. doi:10.1086/711847.

van Schellen, M., Poortman, A.R., Nieuwbeerta, P., 2012. Partners in crime? criminal offending, marriage formation, and partner selection. Journal of Research in Crime and Delinquency 49, 545–571. doi:10.1177/0022427811414197.

Shoag, D., Veuger, S., 2021. Ban-the-box measures help high-crime neighborhoods. Journal of Law and Economics 64, 85–105. doi:10.1086/712761.

Siennick, S.E., Stewart, E.A., Staff, J., 2014. Explaining the association between incarceration and divorce. Criminology 52, 371–398.

Stevenson, B., Wolfers, J., 2007. Marriage and divorce: Changes and their driving forces. Journal of Economic Perspectives 21, 27–52.

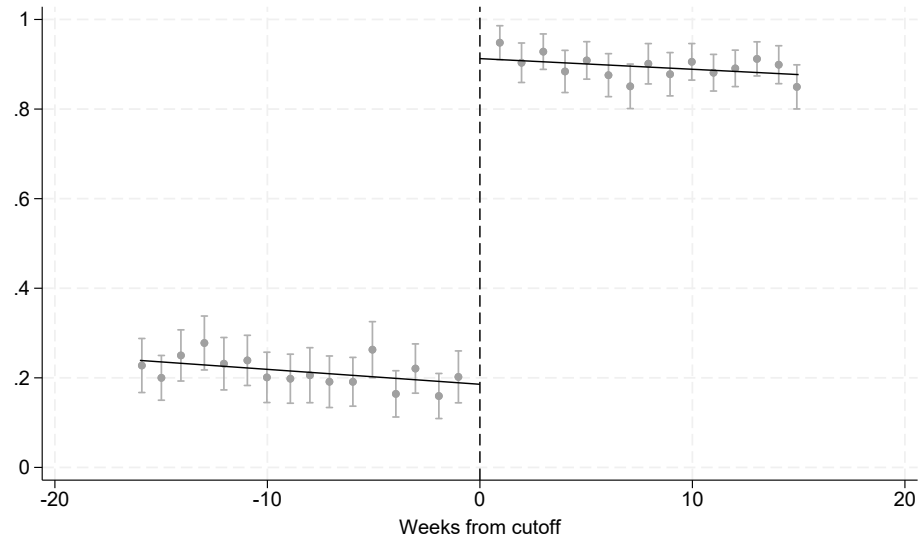
Svarer, M., 2011. Crime and partnerships. Review of Economics of the Household 9, 307–325. doi:10.1007/s11150-010-9104-3.

Weiss, Y., Willis, R.J., 1997. Match quality, new information, and marital dissolution. Journal of Labor Economics 15, S293–S329. doi:10.1086/209864.

Western, B., 2015. Punishment and Inequality in America. Russell Sage Foundation, New York.

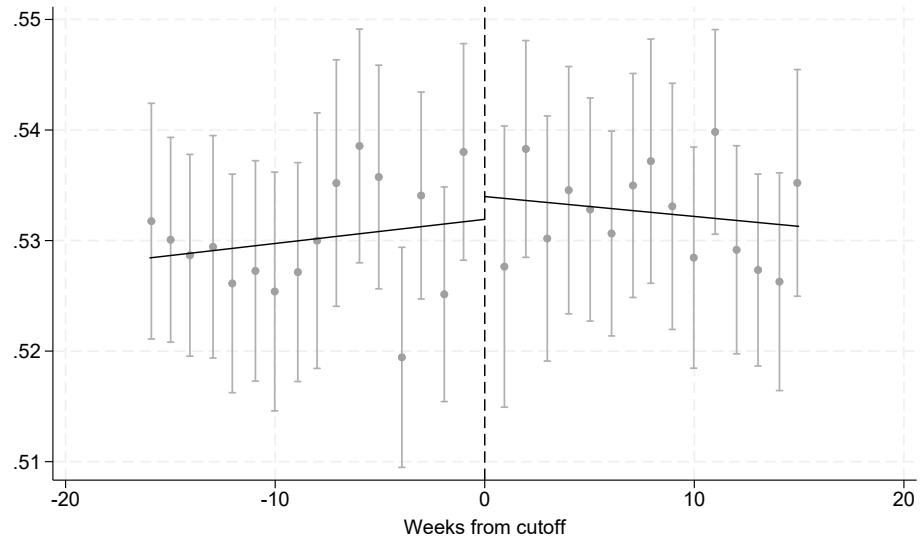
Figures and Tables

Figure 1: First Stage



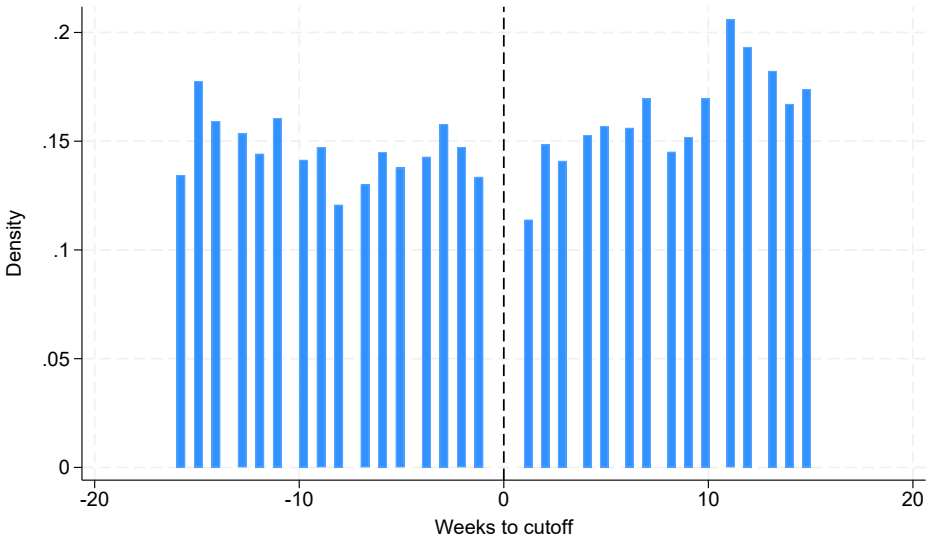
Note: The figure shows the share of cases listed in Lexbase by week to cutoff. The sample consists of first-time male defendant aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Figure 2: Balance plot



Note: The sample consists of first-time male defendant aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. The dependent variable is the predicted value of the endogenous variable "Appearing in the database" from a regression where this variable is regressed on baseline covariates. The prediction is plotted against weeks until the court-specific cutoff. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

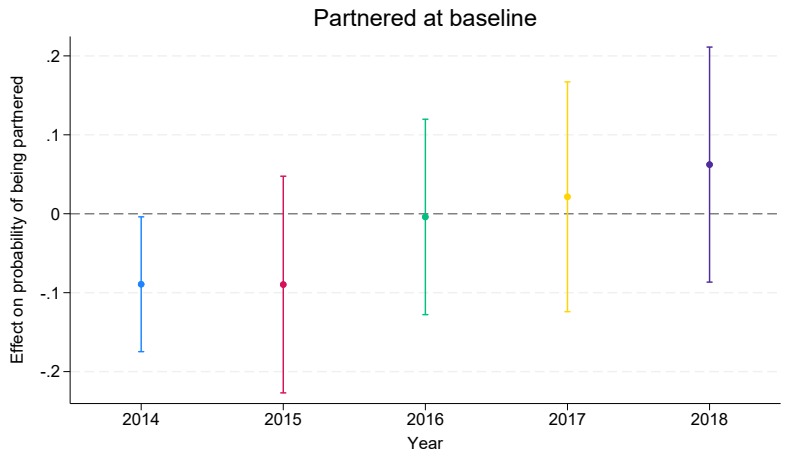
Figure 3: Density around the cutoff



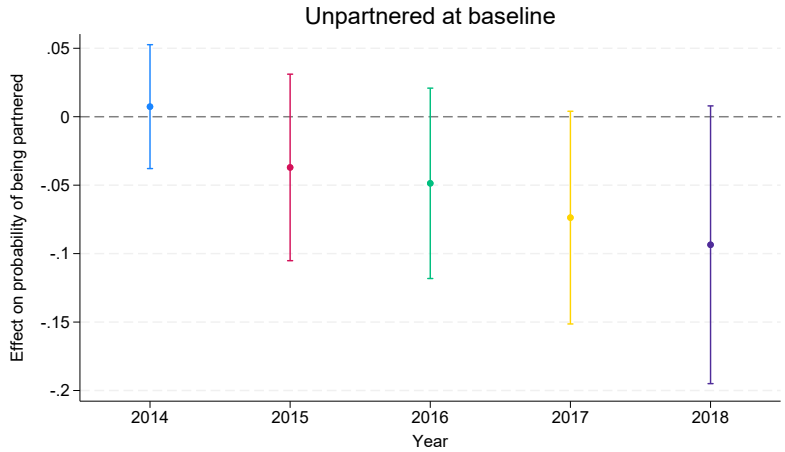
Note: The sample consists of first-time male defendant aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff at week 0. RDD estimates show no significant change in the (log) number of cases around the cutoff (est. 0.141; se 0.207).

Figure 4: IV estimates of the effect being listed on the probability of being partnered year-by-year

(a) Partnered at basline



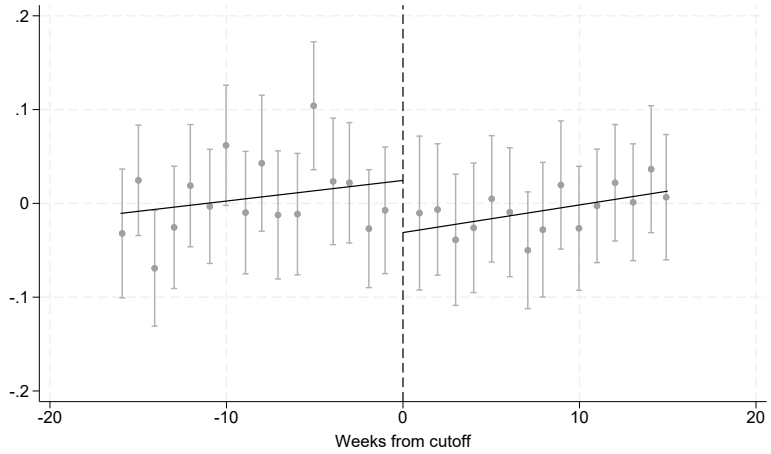
(b) Unpartnered at baseline



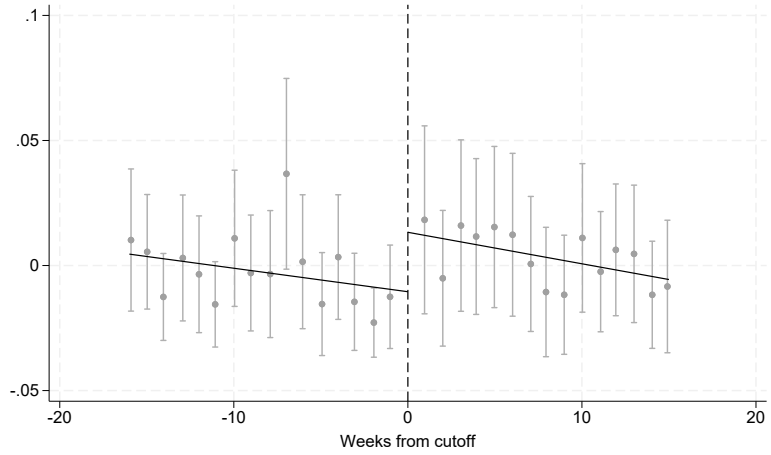
Notes: The sample consists of first-time male defendant aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. The dependent variable is an indicator for whether the individual was partnered in each year 2014-2018. Figure A shows year-by-year estimates for individuals partnered at baseline (1,561 observations) and Figure B shows year-by-year estimates for individuals unpartnered at baseline (4,598 observations). All regressions control for year and court fixed effects as well as for weeks-to-cutoff allowing variying slopes above and below the cutoff. Baseline controls include the following pre-determined covariates: age, immigrant, high school degree, college degree, missing education. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Figure 5: Reduced form effect of being sentenced after the cutoff date

(a) Effect on the probability of being partnered



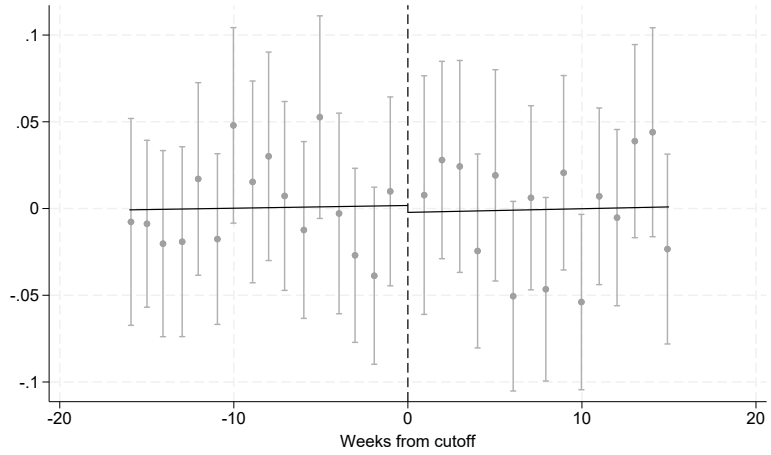
(b) Effect on the probability of committing violence against women



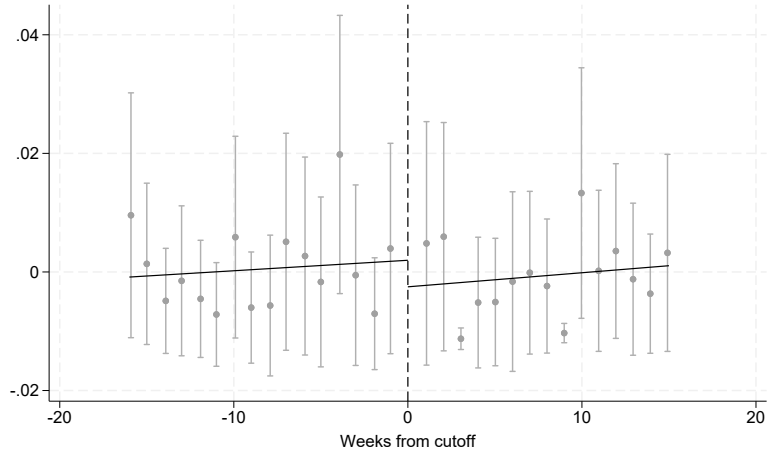
Notes: The sample consists of first-time male defendants aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. The dependent variable in Figure A is an indicator for whether the male is partnered in any year 2014-2018. The dependent variable in Figure B is an indicator for whether the male is convicted for violence against women at least once in the years 2014-2018. Both figures plots the residulized outcomes after netting out year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Figure 6: Reduced form plots for placebo year 2013

(a) Effect of being sentenced after the cutoff on the probability of being partnered in 2013



(b) Effect of being sentenced after the cutoff on the probability of committing violence against women in 2013



Notes: The sample consists of first-time male defendants aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. The dependent variable in Figure A is an indicator for whether the male is partnered in year 2013 before Lexbase. The dependent variable in Figure B is an indicator for whether the male is convicted for violence against women at least once in the year 2013 before Lexbase. Both figures plots the residulized outcomes after netting out year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 1: Descriptive Statistics

	Appearing in the database		Sentenced after the cutoff	
	Yes	No	Yes	No
Panel A: Background Characteristics	(1)	(2)	(3)	(4)
Foreign born	0.296	0.296	0.297	0.295
Age in 2013	31.012	33.372	31.855	32.349
Partnered in 2013	0.230	0.280	0.247	0.260
Compulsory degree in 2013	0.290	0.204	0.252	0.248
HS degree in 2013	0.518	0.561	0.536	0.541
College degree in 2013	0.175	0.217	0.194	0.196
Missing education	0.017	0.017	0.018	0.016
Panel B: Case characteristics				
Property conviction	0.132	0.101	0.113	0.121
Violent conviction	0.237	0.201	0.226	0.216
Drug conviction	0.055	0.048	0.052	0.051
DUI conviction	0.073	0.085	0.077	0.080
Traffic conviction	0.136	0.177	0.149	0.161
Other conviction	0.256	0.268	0.260	0.262
Acquitted	0.111	0.120	0.122	0.109
Prison sentence	0.067	0.072	0.070	0.069
Fines	0.272	0.250	0.266	0.258
Probation sentence	0.122	0.131	0.132	0.122
Panel C: Outcomes				
Partnered	0.390	0.451	0.406	0.429
Violence women 5	0.077	0.037	0.064	0.053
Obs.	3,272	2,888	2,877	3,283

Note: The sample consists of first-time male defendant aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. Appearing in the database is the endogenous variable and Sentenced after the cutoff is the instrument.

Table 2: Effect of being listed on the prob of being partnered

	Outcome	OLS	2SLS	2SLS
Panel A: Full sample	(1)	(2)	(3)	(4)
Partnered	0.419	-0.069***	-0.098*	-0.090**
	(0.493)	(0.020)	(0.050)	(0.043)
Panel B: Partnered at baseline				
Partnered	0.955	-0.022*	-0.080**	-0.078**
	(0.208)	(0.012)	(0.035)	(0.035)
Panel C: Unpartnered at basline				
Partnered	0.236	-0.029*	-0.085*	-0.087*
	(0.425)	(0.015)	(0.047)	(0.047)
Court & FE	–	Yes	Yes	Yes
Baseline Controls	–	No	No	No
Obs.		6,160	6,160	6,160
Obs. partnered		1,562	1,562	1,562
Obs. unpartnered		4,598	4,598	4,598

Note: The table shows estimates of the effect of being listed on the probability of being partnered in 2014-2018. Panel A-C show separate estimates for different subgroups. The sample consists of first-time male defendant aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. All regressions control for year and court fixed effects as well as for weeks-to-cutoff allowing variying slopes above and below the cutoff.. Baseline controls include the following pre-determined covariates: age, immigrant, high school degree, college degree, missing education. The first-stage coefficient in column (2)-(5) is .733 (.033), .732 (.033), .777 (.051) and .719 (.033). The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 3: Effect of being listed on violence against women

	Outcome	OLS	2SLS	2SLS
Panel A: Full sample	(1)	(2)	(3)	(4)
Violence against women	0.036	0.028***	0.039**	0.038**
	(0.187)	(0.006)	(0.019)	(0.018)
Panel B: Partnered at baseline				
Violence against women	0.031	0.013	0.030	0.028
	(0.174)	(0.012)	(0.030)	(0.030)
Panel C: Unpartnered at basline				
Violence against women	0.038	0.032***	0.041*	0.041*
	(0.191)	(0.007)	(0.024)	(0.024)
Court & FE	–	Yes	Yes	Yes
Baseline Controls	–	No	No	No
Obs.		6,160	6,160	6,160
Obs. partnered		1,562	1,562	1,562
Obs. unpartnered		4,598	4,598	4,598

Note: The table shows estimates of the effect of being listed on the probability of committing violence against women in 2014-2018. Panel A-C show estimates separate for different subgroups. The sample consists of first-time male defendants aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. All regressions control for year and court fixed effects as well as for weeks-to-cutoff allowing variying slopes above and below the cutoff. Baseline controls include the following pre-determined covariates: age, immigrant, high school degree, college degree, missing education. The first-stage coefficient in column (2)-(5) is .823 (.047), .824 (.046), .894 (.053) and .782 (.057). The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 4: Bargaining power within couples

	Partner has Higher education	Partner has Lower education	Partner has Higher earnings	Partner has Lower Earnings	Short Relationship	Long Relationship
	(1)	(2)	(3)	(4)	(5)	(6)
Partnered	-0.134***	-0.050	-0.119	-0.056*	-0.115**	-0.034
	(0.049)	(0.039)	(0.082)	(0.032)	(0.046)	(0.053)
	[0.955]	[0.954]	[0.957]	[0.953]	[0.940]	[0.972]
Court & Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	627	935	445	1,117	856	706

Note: The table shows the effect of being listed on the probability of being partnered in 2014-2018. The baseline sample consists of first-time male defendants aged 20 to 60 partnered at baseline who where sentenced in a 16-week window around the court-specific cutoff. All regressions control for year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. The cutoff for the sample split on relationship length is 6 years, which is the median length in our data. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 5: Search duration, search market composition, and mobility for the unpartnered

	Time to finding partner	Partner from childhood social network	Partner from adulthood social network	Change place of residence
	(1)	(2)	(3)	(4)
	-0.052	-0.000	0.038	-0.058
	(0.341)	(0.076)	(0.115)	(0.048)
	[2.870]	[0.170]	[0.339]	[0.638]
Court & Year FE	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Obs.	4,598	1,087	1,087	4,598

Note: The baseline sample consists of first-time male defendants aged 20 to 60 unpartnered at baseline who where sentenced in a 16-week window around the court-specific cutoff. The sample in column (1)-(3) is conditional on having found a partner in five years after Lexbase. The dependent variable in column (1) is years until finding a partner. The dependent variable in column (2) is an an indicator for the partner being from same high school or neighborhood (at age 16). The dependent variable in column (3) is an indicator for the partner being from the same workplace or neighborhood (three years before Lexbase). All regressions control for year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. The cutoff for the sample split on relationship length is 6 years, which is the median length in our data. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 6: Characteristics of new partners among unpartnered men at baseline

	Education above High school	Employment	Earnings	Conviction in Past 10 years	High predicted risk of Becoming victim of IPV
	(1)	(2)	(3)	(4)	(5)
	-0.061***	0.133	-0.009	0.002	0.158**
	(0.023)	(0.089)	(0.204)	(0.070)	(0.070)
	[0.079]	[0.729]	[12.059]	[0.116]	[0.261]
Court & Year FE	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Obs.	4,598	4,598	4,598	4,598	4,598

Note: The baseline sample consists of first-time male defendants aged 20 to 60 who where unpartnered at baseline, sentenced in a 16-week window around the court-specific cutoff and is conditional on finding a partner in 2014-2018. The outcomes are for the future partners of the males unpartnered at baseline. All regressions control for year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 7: Effect on mental health, alcohol and drug use

	Any prescription	Anti-depressants	Sleep/anxiety	Alcohol/drug arrest
	(1)	(2)	(3)	(4)
	0.015	0.025	0.080*	0.060
	(0.035)	(0.045)	(0.048)	(0.037)
	[0.831]	[0.201]	[0.241]	[0.188]
Court & Year FE	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Obs.	4,598	4,598	4,598	4,598

Note: The baseline sample consists of first-time male defendants aged 20 to 60 who where unpartnered at baseline, sentenced in a 16-week window around the court-specific cutoff. The dependent variable in columns (1)-(3) are indicators for prescribed medications in 2014-2018. The dependent variable in column (4) is an indicator for whether the individual has been convicted for an alcohol or drug related crime (e.g. DUI or possession) in 2014-2018. All regressions control for year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 8: Violence where the victim is previously known or stranger to the offender

	Baseline	Stranger to The victim	Not a stranger To the victim
	(1)	(2)	(3)
	0.041*	0.011	0.058**
	(0.024)	(0.009)	(0.026)
	[.]	[0.009]	[0.059]
Court & Year FE	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes
Obs.	4,598	4,598	4,598

Note: The baseline sample consists of first-time male defendants aged 20 to 60 who where unpartnered at baseline and sentenced in a 16-week window around the court-specific cutoff. The dependent variable in columns (1)-(3) are indicators for prescribed medications in 2014-2018. The dependent variable in column (2) is an indicator for whether the victim of the violent crime was a female stranger. The dependent variable in column (3) is an indicator for whether the victim of the violent crime was not a stranger (i.e. partners, friends, co-workers, classmates). All regressions control for year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 9: Effects on the outcomes of the spouse of the males partnered at baseline

	Employed	Earnings	Sickness absence	Anti-depressants	Sleep/anxiety	Victim
	(1)	(2)	(3)	(4)	(5)	(6)
	0.118*	0.109	-0.119**	-0.035	0.098	0.001
	(0.068)	(0.175)	(0.060)	(0.066)	(0.067)	(0.006)
	[0.650]	[12.126]	[0.388]	[0.250]	[0.241]	[0.006]
Court & Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	1,562	1,562	1,562	1,562	1,562	1,562

Note: The baseline sample consists of females who in 2013 where partners to first-time male defendants aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. Earnings is log average annual earnings in 2014-2018. The dependent variable in columns (4) and (5) are indicators for prescribed medications in 2014-2018. Victim is an indicator for having received in or outpatient care for victim of domestic violence in 2014-2018. All regressions control for year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Appendix

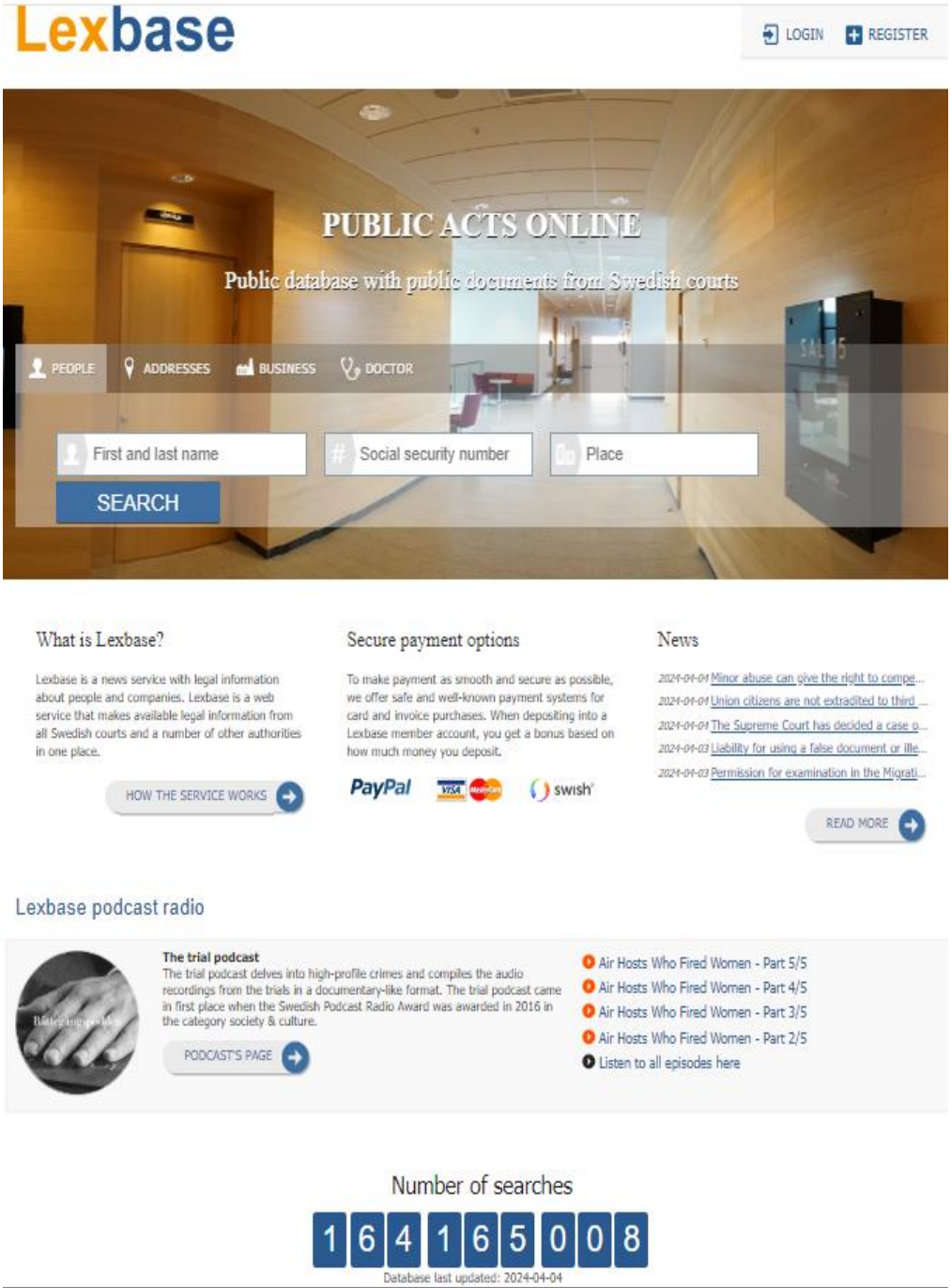


Figure 7: Homepage Lexbase

Notes: This image shows the homepage of Lexbase and was retrieved on April 4, 2024.

How to use Lexbase

With a few single button presses, you can obtain useful decision-making information. You start by choosing a search method. The guide below explains how the service works.

1. Select search method

A) Search person
In this search function Search person you can specify your search using four fields; First name, last name, social security number and city. If you get a hit on a person, it means that he or she has been subject to a court hearing. If you do not get a hit, it is probably because the person does not appear in any legal process.

Example: Shows results for Anders Andersson in Stockholm

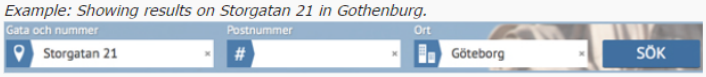


In the search fields, you can use asterisk (*) in your search text to replace part of a search term. You also don't need to search for an exact name of a person or company. Parts of the name in question are sufficient.

If you do not get a hit on a certain person, it is probably because they are not present in any legal process.

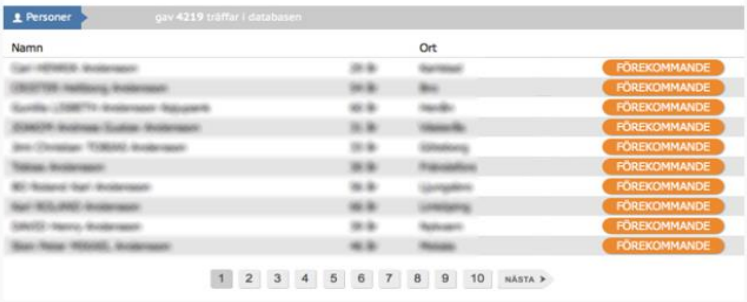
B) Search address
This search function helps you who want information about people living in a certain area or in your immediate area. The search tool sorts by Address, Postcode and City. The more fields you fill in, the narrower the hit picture you get.

Example: Showing results on Storgatan 21 in Gothenburg.



2. Search results are displayed

When you have made a search, the search result is presented in a list. To read a report, click on the button on the right which looks like this and is called FÖREKOMMANDE



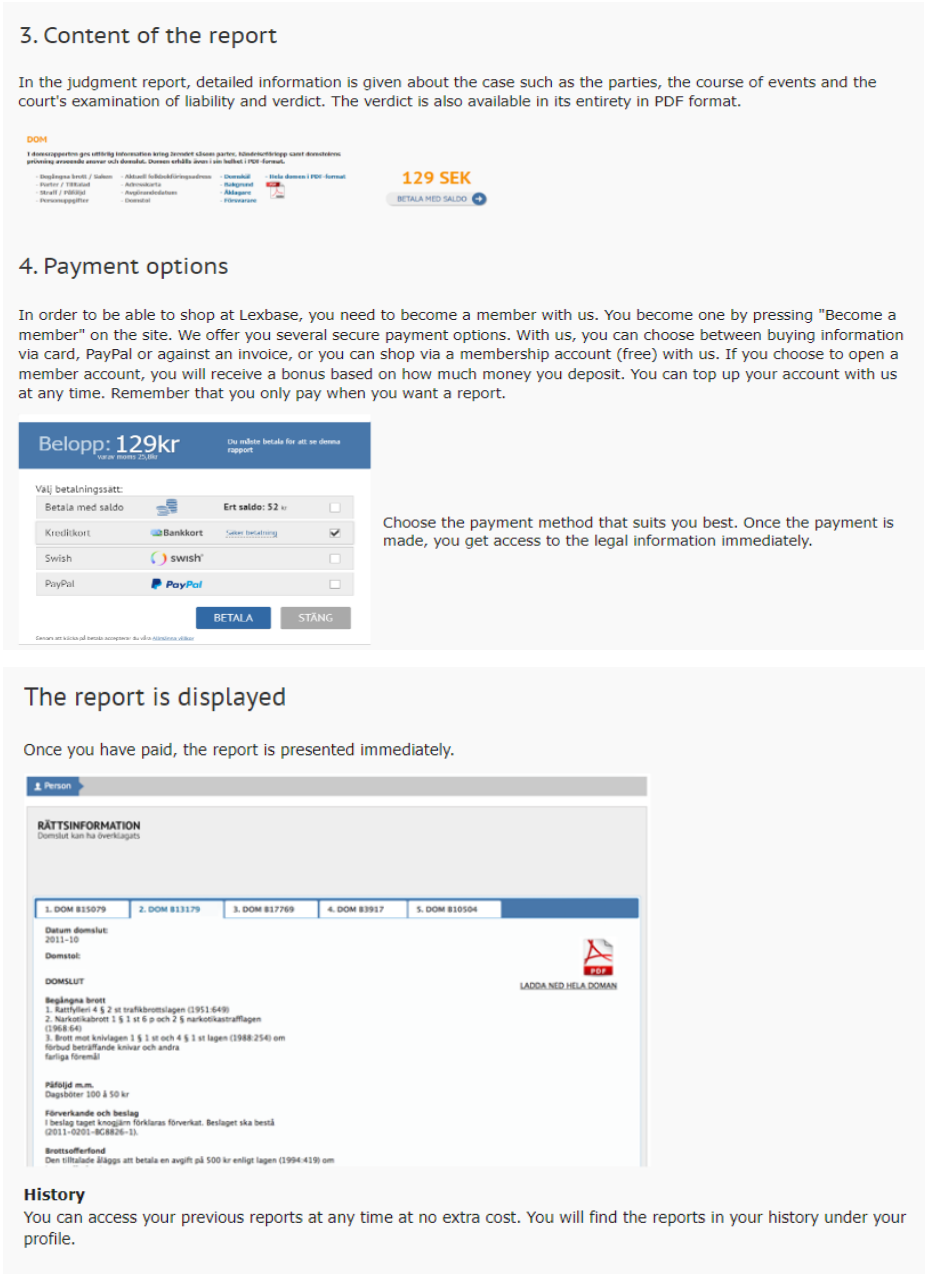


Figure 8: How the Search-Engine at Lexbase Works

Notes: This image is from the homepage of Lexbase and provides the guide from the platform on how to use the search engine. The image was retrieved on April 4, 2024.

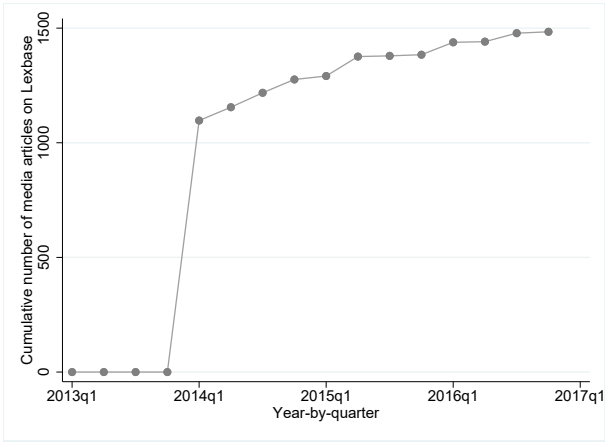


Figure 9: Cumulative number of media articles covering Lexbase 2013-2016

Notes: This figure plots the cumulative number of media articles mentioning Lexbase in 2013-2016. Data on media coverage is from the Retriever/Mediearkivet database, which is a Nordic news archive with material from mainly major Swedish newspapers. The information was collected on Jan 20th, 2024.

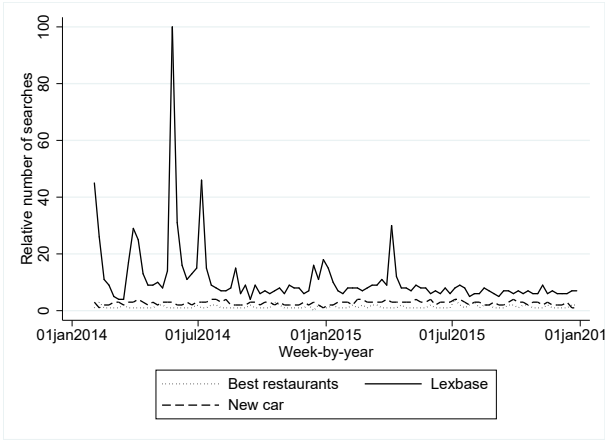


Figure 10: Relative number of searches on Google

Notes: This figure plots the relative number of searches on Google for the terms "Lexbase", "Best restaurants" and "New car". The data from Google Trends was collected on Jan 21st, 2024. Google Trends normalizes search data to make comparisons between terms easier. Search results are normalized to the time and location of a query by dividing each data point by the total searches of the geography and time range it represents. The resulting numbers are then scaled on a range of 0 to 100 based on a topic's proportion to all searches on all topics. To make the figure more easy to read we drop the days immediately following the launch of the database when the searches skyrocketed and focus on the period starting one week after the introduction and up until the last week of 2015.The information was collected on Jan 20th, 2024.

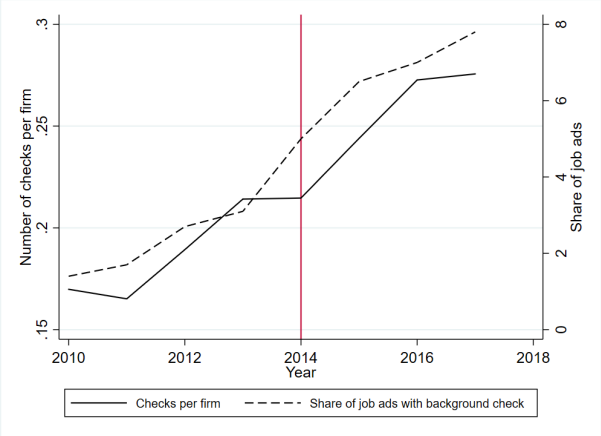


Figure 11: Criminal background checks

Notes: This figure plots the number of official criminal background check inquires for personal use (likely to be submitted to a potential employer) per registered firm submitted to the Swedish police over the period 2010-2017 using data from the Swedish police and Statistics Sweden. The figure also plots the share of ads posted at the Public Employment Office that mentions criminal background checks using data from the Public Employment Office.

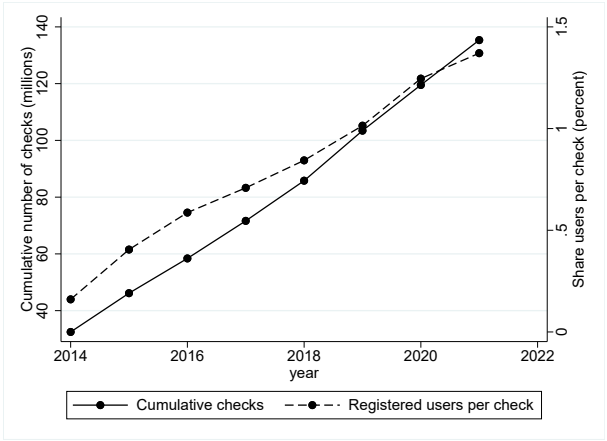


Figure 12: Background checks using the platform

Notes: This figure plots the cumulative number of searches for criminal background using the platform. The figure also plots the number of registered users per background check conducted. The figure uses data provided by the company behind the platform.

Table 10: Balancing

	Appearing in the database	Sentenced after the cutoff
	(1)	(2)
Age in 2013	-0.003***	-0.001**
	(0.001)	(0.001)
Foreign born	0.019	0.011
	(0.013)	(0.011)
HS degree in 2013	-0.081***	0.001
	(0.015)	(0.012)
College degree in 2013	-0.087***	0.008
	(0.016)	(0.016)
Missing education	-0.076	0.011
	(0.049)	(0.044)
Violent conviction	0.058**	-0.013
	(0.024)	(0.023)
Property conviction	0.067**	-0.044
	(0.034)	(0.031)
Traffic conviction	-0.010	-0.044**
	(0.024)	(0.020)
Drug conviction	0.039	-0.055*
	(0.034)	(0.031)
DUI conviction	0.025	-0.018
	(0.030)	(0.028)
Prison sentence	0.012	0.037*
	(0.028)	(0.021)
Fines	0.011	0.016
	(0.020)	(0.015)
Probation sentence	-0.030	0.023
	(0.020)	(0.019)
Joint F-Test	[0.00000]	[0.23399]
Observations	6,160	6,160

Note: The sample consists of first-time male defendant aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. Column (1) shows results from a joint regression of an indicator for appearing in the database (endogenous variable) on all baseline covariates. Column (2) shows results from a joint regression of an indicator for being sentenced after the cutoff (instrument) on all baseline covariates. Column (3) shows results from separate regressions each using baseline covariates as the dependent variable regressed on an indicator for being sentenced after the cutoff along with controls for weeks until the cutoff (running variable) allowing for differences in slopes after and before the cutoff as well as court fixed effects. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 11: First Stage Results in Subgroups

	Sample Mean	Sentenced after the cutoff	
	(1)	(2)	(3)
Full sample	0.521	0.703***	0.703***
	(0.500)	(0.027)	(0.028)
Foreign Born	0.532	0.655***	0.654***
	(0.499)	(0.035)	(0.035)
Native Born	0.531	0.694***	0.692***
	(0.499)	(0.028)	(0.028)
Aged 20-30	0.572	0.637***	0.637***
	(0.495)	(0.027)	(0.026)
Aged 31-40	0.496	0.735***	0.733***
	(0.500)	(0.039)	(0.039)
Aged 41-50	0.467	0.765***	0.768***
	(0.499)	(0.044)	(0.042)
Aged 51-60	0.452	0.751***	0.745***
	(0.498)	(0.046)	(0.046)
Compulsory degree	0.617	0.548***	0.545***
	(0.486)	(0.030)	(0.029)
HS degree	0.511	0.706***	0.704***
	(0.500)	(0.032)	(0.032)
College degree	0.477	0.812***	0.810***
	(0.500)	(0.032)	(0.033)
Court & Year FE	–	Yes	Yes
Baseline Controls	–	No	Yes

Note: The sample consists of first-time male defendant aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. The table shows the first-stage estimate for subgroups stratified by values of the covariates in the baseline model. All regressions controll for the relevant baseline covariates as well as year and court fixed effects and weeks-to-cutoff allowing for variying slopes above and below the cutoff. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 12: First Stage Results in Subgroups

	Sample Mean	Sentenced after the cutoff
	(1)	(2)
Full sample	0.521	0.703***
	(0.500)	(0.028)
Foreign Born	0.532	0.654***
	(0.499)	(0.035)
Native Born	0.531	0.692***
	(0.499)	(0.028)
Aged 20-30	0.572	0.637***
	(0.495)	(0.026)
Aged 31-40	0.496	0.733***
	(0.500)	(0.039)
Aged 41-50	0.467	0.768***
	(0.499)	(0.042)
Aged 51-60	0.452	0.745***
	(0.498)	(0.046)
Compulsory degree	0.617	0.545***
	(0.486)	(0.029)
HS degree	0.511	0.704***
	(0.500)	(0.032)
College degree	0.477	0.810***
	(0.500)	(0.033)
Court & Year FE	–	Yes
Baseline Controls	–	Yes

Note: The sample consists of first-time male defendant aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. The table shows the first-stage estimate for subgroups stratified by values of the covariates in the baseline model. All regressions controll for the relevant baseline covariates as well as year and court fixed effects and weeks-to-cutoff allowing for variying slopes above and below the cutoff. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 13: Effect of being listed on the probability of being partnered among females

	Outcome	OLS	2SLS	2SLS
Panel A: Full sample	(1)	(2)	(3)	(4)
Non-single household	0.459	-0.056**	0.005	0.005
	(0.498)	(0.026)	(0.055)	(0.054)
Panel B: Partnered at baseline				
Non-single household	0.935	-0.005	0.006	0.007
	(0.246)	(0.019)	(0.057)	(0.056)
Panel C: Unpartnered at basline				
Non-single household	0.240	-0.021	0.088	0.088
	(0.427)	(0.024)	(0.058)	(0.057)
Court & FE	–	Yes	Yes	Yes
Baseline Controls	–	No	No	No
Obs.		2,061	2,061	2,061
Obs. partnered		648	648	648
Obs. unpartnered		1,413	1,413	1,413

Note: The sample consists of first-time female defendants aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. The dependent variable is an indicator for whether the individual was partnered in any year 2014-2018. All regressions control for year and court fixed effects as well as for weeks-to-cutoff allowing variying slopes above and below the cutoff.. Baseline controls include the following pre-determined covariates: age, immigrant, high school degree, college degree, missing education. The first-stage coefficient in column (2)-(5) is .823 (.047), .824 (.046), .894 (.053) and .782 (.057). The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 14: Robustness and additional results

	Discrete RD	Bandwidth 20 weeks	Bandwidth 8 weeks	Placebo Year	Multiple time Offenders	Misdemeanor	Felony	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Non-single household	-0.106***	-0.085***	-0.015	-0.018	-0.041	-0.153**	-0.069	
	(0.039)		(0.032)	(0.052)	(0.031)	(0.030)	(0.064)	(0.054)
Violence against women	0.034**	0.027**	0.053**	-0.008	-0.022	0.001	0.058***	
	(0.016)		(0.013)	(0.026)	(0.009)	(0.021)	(0.017)	(0.022)
Court & Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Obs.	6,160	7,717	2,800	6,160	17,852	2,471	3,689	

Note: The baseline sample consists of first-time male defendants aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. All regressions control for year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. Row (1) controls for the running variable using year-by-month FEs. Row (2) shrinks the bandwidth to 8 weeks and estimate a local randomization RD without controlling for the running variable. Row (3) increases the bandwidth to 20 weeks. Row (4) uses as placebo the dependent variable measured in the year before the launch of Lexbase. Row (5) includes also multiple time offenders in the sample. Row (6) and (7) stratify the sample based on whether the the average incarceration rate for the crime committed at baseline was below 1 percent (Misdemeanor) or above 1 percent (Felony). The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 15: Changes in reporting behavior and labor market outcomes

	Baseline	Case Charged	Effect on Employment	Effect on Earnings
	(1)	(2)	(3)	(4)
	0.041*	0.045*	0.011	-0.255
	(0.024)	(0.025)	(0.038)	(0.242)
	[0.038]	[0.036]	[0.853]	[11.733]
Court & Year FE	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Obs.	4,598	4,598	4,598	4,598

Note: The baseline sample consists of first-time male defendants aged 20 to 60 who where unpartnered at baseline and sentenced in a 16-week window around the court-specific cutoff. The dependent variable in column (2) is is an indicator set to unity if the prosecutor following the investigation assessed the evidence sufficiently strong to warrant bringing it to trial and zero otherwise. The dependent variable in column (3) is an indicator for being employed in any year 2014-2018. The dependent variable in column (4) is (log) average earnings in 2014-2018 with zero earnings replaced with 1. All regressions control for year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 16: Singles are of worse quality

	Partnered	Unpartnered
Panel A: Demographic Characteristics	(1)	(2)
Foreign born	0.426	0.252
Age in 2013	39.736	29.531
Panel B: Socioeconomic characteristics		
Compulsory degree in 2013	0.196	0.268
HS degree in 2013	0.498	0.552
College degree in 2013	0.291	0.162
Missing education	0.015	0.018
Employment in 2013	0.808	0.614
Earnings in 2013	346235.551	200201.835
Panel B: Crime		
Post conviction	0.147	0.281
Post prison sentence	0.025	0.065
Post violent conviction	0.031	0.090
Post number of convictions	0.259	0.598
Panel B: Health		
On anti-depressants	0.069	0.082
On sleep/anxiety medication	0.067	0.089
On ADHD medication	0.006	0.023
Obs.	1,562	4,598

Note: The baseline sample consists of first-time male defendants aged 20 to 60 who where unpartnered at baseline and sentenced in a 16-week window around the court-specific cutoff. Convictions and incarceration are measured between the cutoff untiö the introduction of the database.

Table 17: Results by offender baseline mental health treatment

	Baseline	Anti-depressant in 2013	No anti-depressant in 2013
	(1)	(2)	(3)
	0.041*	-0.027	0.074***
	(0.024)	(0.058)	(0.026)
	[0.912]	[0.937]	[0.916]
Court & Year FE	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes
Obs.	4,598	191	1,082

Note: The baseline sample consists of first-time male defendants aged 20 to 60 who where sentenced in a 16-week window around the court-specific cutoff. All regressions control for year and court fixed effects and baseline controls: age, immigrant, high school degree, college degree, missing education. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.