Screening, Exclusion and Violence in the Partner Market*

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Abstract

Screening tools can help women avoid risky partners, but making records searchable may also alter the opportunities and behavior of the men whose histories are disclosed. We provide causal evidence on both sides of this trade-off. Our setting is the unexpected 2014 launch of a Swedish platform that made criminal charges immediately, anonymously, and freely searchable. Linking platform records to population-wide administrative data, we exploit court-specific retention cutoffs that generated a 73-percentage-point discontinuity in actual listing among males charged with a crime. Effects turn on relationship status at launch. Among men with a partner at launch, listing reduces continuation of the baseline relationship by 7.8 percentage points, and their female partners are subsequently less likely to be non-employed. Among men without a partner at launch, listing reduces partnership formation by 8.7 percentage points and increases the probability of a recorded suspicion of violence and sex crimes against women by 6.1 percentage points. The additional crime extends beyond current partners to other women known to the defendant, while we detect no significant increase in other types of crime. Listing also increases men's alcohol- or drug-related diagnoses and among the partnerships that form, women have characteristics associated with greater observable disadvantage. A complementary analysis shows women's realized exposure shifting away from listed relative to unlisted men in the focal defendant sample. These findings reveal a neglected consequence of safety-oriented disclosure: facilitating exit from existing relationships can coincide with reduced relationship access, changes in who matches, and violence on the new-match margin.

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1 Introduction

Criminal-history disclosure is increasingly used as a tool to help women assess risk before entering or continuing intimate relationships. The premise is a standard screening argument: when a payoff-relevant attribute is costly to observe, lowering the cost of observing it should improve the quality of matches (Akerlof, 1970). Applied to partnerships, information about a partner's history shifts beliefs about match quality and can precipitate dissolution (Weiss and Willis, 1997; Becker, 1973), and remaining in an abusive relationship carries large measurable costs (Adams et al., 2024). This logic motivates policies such as the United Kingdom's Domestic Violence Disclosure Scheme, commonly known as Clare's Law, as well as criminal-background checks offered through dating platforms.[1]

Yet public searchability changes more than a potential partner's private information. Searchability exposes the record to current partners, relatives, friends, and other members of the local community. It may therefore change the listed man's social opportunities and behavior even when people close to him already knew his history: what changes is not only what any one person knows, but who can find out, and at what cost (Goffman, 1963). Mandated disclosure accordingly produces responses beyond the screening margin it targets (Dranove and Jin, 2010), and in matching markets revealing an attribute reallocates partners rather than simply informing them (Tadelis and Zettelmeyer, 2015). The criminal-records literature supplies a direct precedent: reforms that changed access to records altered outcomes through channels other than the screening they were designed to improve (Agan and Starr, 2018; Doleac and Hansen, 2020). Public disclosure may thus precipitate separation, restrict access to future relationships, or alter the behavior of the person whose history becomes visible. The consequences for women's safety cannot be inferred from the screening motive alone.

This paper provides causal evidence on these effects using the unexpected launch of Lexbase, a Swedish online criminal-record platform, in January 2014. Before Lexbase, court records were legally public but costly to find in practice. A private individual generally needed to identify and contact the relevant local court, and older cases could require a manual search of archived records. Lexbase replaced this decentralized process with an immediate, anonymous, name-based search. The platform attracted extensive media attention and recorded 32 million page views during its first year and more than 10 million person search queries in the following years. According to company data,

[1] Clare's Law is named after Clare Wood, a 36 year old mother who in 2009 was killed by her abusive partner (Belur et al., 2022). Tinder introduced access to criminal-background checks through its Safety Center in 2022. See *Wall Street Journal*, "Tinder Adds Background Checks to Its Dating App," March 9, 2022. Roughly half of online-dating users report researching a prospective date before meeting in person (Avast, 2021), in a market that accounts for a large and growing share of new relationships (Rosenfeld et al., 2019).

approximately two-thirds of registered users were women, and checking a prospective partner was the most commonly reported reason for using the service. Lexbase therefore provides a setting in which a formally public but practically obscure attribute suddenly became searchable at scale.

A free search revealed that an individual had appeared as charged in a criminal court case, but generally did not disclose the offense, disposition, or surrounding circumstances. For most users, listing therefore provided a coarse binary signal of criminal involvement rather than a detailed criminal history. This feature is important for interpretation. A partner may have learned something genuinely new, but listing could also have made previously known information more salient or credible, or extended it to her social network. We consequently refer to Lexbase as a public-visibility shock. Our design identifies the combined causal effect of information, salience, and potential stigma channels.

We identify the effect of listing using administrative retention rules that sharply limited Lexbase's ability to retrieve older cases. Criminal cases were removed from courts' searchable administrative registers approximately five years after disposition. Defendants whose cases closed just inside the relevant retention window were therefore much more likely to appear in Lexbase than defendants whose cases closed just outside it. We use information supplied by Lexbase to observe actual listing and link the platform records to population-wide administrative data on partnerships, criminal behavior, health, and labor-market outcomes. In a fuzzy regression-discontinuity design around court-specific retention cutoffs, falling on the high-disclosure side increases the probability of listing by 73.2 percentage points. Predetermined characteristics, including the initial charge and formal sanction, evolve smoothly through the cutoff. The design therefore varies whether past criminal involvement becomes readily searchable while holding fixed the underlying case and its legal consequences.

The main results depend on whether a relationship existed when Lexbase launched. Among men who were registered with a partner at the end of 2013, listing reduces the probability that the baseline relationship continues by 7.8 percentage points. The response is rapid, with most of the decline occurring during the first year after launch. Among men who were without a partner at launch, listing reduces the probability of forming a partnership during 2014–2018 by 8.7 percentage points. This deficit develops gradually over the follow-up period. For the same men, listing increases the probability of a recorded suspicion of physical or sexual violence against an adult woman by 6.1 percentage points, roughly doubling the base rate. We find no statistically detectable increase in violence against women among men who were already partnered at launch.

Several patterns help interpret the separation response among incumbent couples. The separation point estimates are larger when the union formed after the index trial, when

the relationship is relatively recent, and when the woman belongs to an age group with higher search activity among female Lexbase users. These patterns are consistent with information updating. The point estimates are also larger when the female partner has greater relative bargaining power, as proxied by her earnings or education relative to the man's, consistent with outside options and the practical ability to leave mattering when relationship surplus falls. An exploratory split by municipality population density yields a larger separation point estimate in less dense municipalities, where social networks may overlap more and anonymity may be lower. This pattern is consistent with social diffusion and larger reputational or associative-stigma costs, including when the incumbent partner already knew about the underlying case. Turning to downstream outcomes for women, we also show that listing improves labor market outcomes of the female partner in the follow-up period.

We next investigate why recorded violence against women rises among men who were without a partner at launch even though fewer register-observed partnerships form. We find that the increase is specific to physical and sexual offenses rather than general: listing increases violence or sexual offending against women percentage points while the corresponding estimates for violence or sexual offending against others, and other types of crime are smaller and statistically insignificant. Our findings also show that the results are unlikely to be explained by changes in reporting behavior.

The remaining results point to several possible, and potentially complementary, responses. First, the violence point estimate is substantially larger among men with prior recorded violence or sexual offending against women, consistent with listing amplifying risk among men with a history of violence. Second, listing increases the probability of an alcohol- or drug-related diagnosis by 7.7 percentage points and yields a positive but imprecise estimate for psychiatric care, while producing no detectable deterioration in employment or earnings. These findings are consistent with distress or substance-related problems as a possible response to listing. Third, listing changes which partnerships are observed to form. Conditional on forming a partnership, listed men are matched with women who have characteristics associated with greater observable disadvantage, including lower education, more psychiatric care, and a higher predicted risk of future intimate-partner violence. Finally, the violence increase is not confined to current partners. For the years in which the offender–victim relationship is observed, estimates are positive for both current partners and women known to the defendant who are not current partners, a group that includes former partners and acquaintances.

We complement the male-side analysis with a event study of realized relationship exposure in were we follow women who were initially without a partner and age 20–35 in 2010. Before Lexbase, the difference between women's probabilities of being partnered with

listed and unlisted convicted men is close to zero relative to 2013. The contrast becomes negative after launch and grows in magnitude through 2018, reaching approximately -35 percent of the baseline probability of being partnered with a listed convicted man. These results shows that the male-side contraction is accompanied by a post-launch shift in realized relationship allocation from women's side.

The paper contributes first to research on the collateral consequences of criminal records. Existing economics work has focused primarily on employment, hiring, incarceration, and policies that restrict employer access to criminal histories (Grogger, 1995; Agan and Starr, 2018; Doleac and Hansen, 2020; Agan et al., forthcoming; Cullen et al., 2023; Rose, 2021; Mueller-Smith and Schnepel, 2021; Hjalmarsson et al., 2025). We show that record visibility also affects intimate relationships, holding fixed the underlying criminal charge and formal sanction. This distinction matters because digitization can create new consequences even when the legal status of the underlying information has not changed.

Second, the paper contributes to the economics of violence against women. Prior work emphasizes income and institutional shocks (Aizer, 2010; Card and Dahl, 2011; Anderberg et al., 2016; Hidrobo et al., 2016; Bobonis et al., 2013; González and Rodríguez-Planas, 2020; Adams et al., 2024; Bhuller et al., 2024; Erten and Keskin, 2024; Anderberg et al., 2023; Calabresi and Rodríguez-Planas, 2025; Bhalotra et al., 2026; Erten et al., 2026). We identify a different shock: public visibility of a man's past criminal involvement. The finding that listing facilitates separation in some incumbent relationships but increases recorded violence among men initially without a partner is directly relevant to disclosure policies designed to protect potential victims. It shows why their consequences must be evaluated beyond the immediate decision to screen or exit.

More broadly, the findings speak to research on relationship formation and dissolution. Existing models emphasize match surplus, relationship-specific capital, and outside options (Becker, 1973, 1974; Weiss and Willis, 1997; Chiappori et al., 2017), while related empirical work shows that partnership decisions respond to features of the institutional environment not designed with the partner market in mind, including the terms of social insurance (Persson, 2020) and the legal cost of exit (Hertegård, 2024). Our empirical results show how a public, nonpecuniary signal interacts with these margins. Separation is rapid where a relationship already exists, whereas the partnership deficit accumulates among initially unpartnered men. The conditional composition of the smaller set of relationships that forms also changes. These findings do not require interpreting Lexbase as a pure information shock. They instead show that a change in the accessibility and audience of adverse personal information can alter relationship transitions whether it operates through learning, salience, social diffusion, or stigma.

The policy distinction between targeted and universal disclosure is therefore important.

Confidentially providing detailed information to a person considering a particular relationship differs from making a coarse criminal-record label searchable by anyone. Universal searchability may facilitate screening and exit, but it can also expose the listed person and those associated with him to a broader audience, reduce future relationship opportunities, and produce behavioral responses outside established partnerships. Our results show that evaluating such policies requires tracing consequences for incumbent relationships, future relationships, and violence, rather than treating disclosure as a change in private information alone.

The rest of the paper proceeds as follows. Section 2 describes the Swedish institutional setting and the launch of Lexbase. Sections 3 and 4 describe the data and research design. Section 5 presents the main estimates, mechanism evidence, and the complementary woman-level analysis. The final sections discuss interpretation and conclude.

2 Background[2]

Criminal-history checks are a screening technology used by employers, landlords, and other organizations that select among applicants under safety and liability concerns. Roughly half of U.S. employers checked applicants' criminal backgrounds already in 2004 (Holzer et al., 2006), and the practice has since become close to universal, increasingly through private background-check firms whose reports provide a salient but coarse signal: typically a name, a jurisdiction, and a brief offense label, with non-conviction records often included (Bushway and Sweeten, 2007). In Sweden, by contrast, access to criminal records was traditionally confined to authorized institutional uses (Backman, 2012).

The same screening logic has increasingly moved into intimate-partner markets. Digital dating platforms create opportunities for matching among strangers, but they also make searchable information and peer-to-peer warnings more valuable as tools for managing relationship risk. Some platforms have therefore begun to incorporate criminal-background checks directly into the dating process. In 2022, for example, Tinder announced a background-check service for users in the United States.[3] In 2025, the U.S.-based dating-safety app Tea similarly allowed women to use criminal-background checks and related tools to vet potential dates and avoid men with histories of violence, sexual offending, or other red flags.[4] Related forms of informal screening have also emerged through online forums such as "Are We Dating the Same Guy?", where women share

[2]This section draws on Forsberg (2024).

[3]See *Wall Street Journal*, "Tinder Adds Background Checks to Its Dating App," March 9, 2022.

[4]See *Washington Post*, "Women are reviewing men on the Tea app. Some say it's making dating safer," July 24, 2025.

warnings and experiences about potential partners.[5] These developments suggest that criminal-history disclosure is no longer confined to institutional screening by firms, landlords, or public agencies. It is increasingly part of decentralized partner markets in which individuals use searchable background information to assess relationship risk. In this setting, disclosure may affect the opportunities, behavior, and subsequent matches of the individuals whose records become visible. Lexbase, described next, is the first setting in which such disclosure occurred at national scale and at zero cost to the searcher.

2.1 The platform

Lexbase AB was registered as a company in March 2013 and launched the platform on January 27, 2014. Around the time of its registration, the company started to assemble a nationwide database of recent Swedish court cases by requesting case information and judgments from individual courts. The requests were not publicly announced, and the individuals whose cases were retrieved had no reason to anticipate that their court involvement would become searchable. The records were drawn from the courts' digital case-management system, *Vera*, and from the underlying court documents. Under Sweden's constitutional principle of public access, court judgments and associated case records were generally available to any requester unless covered by a specific secrecy provision. The platform operated under a publishing certificate (*utgivningsbevis*) issued by the Swedish Media Authority, which conferred protection under Sweden's constitutional freedom-of-expression law and provided the constitutional basis on which the operator maintained that general data-protection rules did not apply to the service. On that basis, the person-searchable criminal-court database operated in Sweden for more than a decade, although its legal status was repeatedly contested.

The platform attracted immediate public attention and substantial criticism. Contemporary media coverage emphasized both its potential use for screening and concerns about privacy, reputational harm, and the accuracy and interpretation of search results. The initial version also included a map displaying the residential locations associated with people in the database. Lexbase's internet service provider terminated service two days after launch, and the platform experienced an initial interruption before relaunching in April 2014 without the original map function.[6]

The platform sharply changed access to criminal-history information in Sweden. Before

[5] These forums are not equivalent to criminal-record searches, but they point to a similar mechanism: women use digital information-sharing tools to reduce perceived risk in the dating market.

[6] The platform was first taken offline on January 29, 2014, briefly reappeared, and was relaunched in a modified form on April 4, 2014. This interruption does not materially alter the timing of the public-disclosure shock studied in the paper.

its launch, court records were formally public but costly for private individuals to locate. A requester generally needed to identify the court likely to hold the case and provide sufficiently precise information for court personnel to find the relevant documents. Because there was no centralized public database searchable by a person's name, a comprehensive search could require contacting several courts. Employers were in practice the only group with a simpler route. Sweden maintains a national criminal-record register (*belastningsregistret*), which is closed to the general public but from which individuals may obtain an extract concerning themselves. Employers routinely asked applicants to request such an extract and present it, and in some sectors—notably work involving children—statute required a check before hiring.[7] Screening in the labor market therefore had an established, if indirect, channel. Outside employment—between neighbors, acquaintances, or prospective partners—no comparably simple or centralized channel existed.

Lexbase replaced this decentralized process with a person-searchable online service. Anyone could run a search directly in a web browser: no account, registration, or payment was required, and the person searched was neither contacted nor notified. A user entered a name and immediately learned whether the individual appeared as a defendant in a criminal case covered by the database. Search results displayed identifying information such as current age and place of residence, generally allowing users to distinguish between individuals with the same or similar names.[8] Searches also returned listed individuals whose names had similar but not identical spellings, so a user did not need to know the exact spelling of a person's name to find a record. Importantly, a Lexbase hit did not necessarily mean that the person had been convicted: the database reflected appearance as a defendant in criminal proceedings and could therefore include cases ending in acquittal. The free result was consequently a salient but coarse signal of criminal-court involvement rather than a complete or official criminal record.

Obtaining more than the free indication of a hit required a user to register an account, provide personal information, and pay a small fee of approximately USD 7 by credit

[7]The extract passed through the applicant rather than going directly from the register to the employer. Except where presentation was legally required, the applicant could therefore choose whether to provide the extract.

[8]Current age and place of residence were drawn from population registers and updated continuously. Lexbase's procedure for handling name changes is not documented, but because court records contain personal identity numbers and the platform used these to link defendants to population-register information, profiles most likely followed a name change. The clearest first-person account we have found, posted on the Swedish online forum Flashback in 2019, reports exactly this: after a surname change the individual remained listed under the new surname, while the underlying judgments retained the name in use when they were issued; see `https://www.flashback.org/p66596085`. Appendix Figure A.3 further shows no break at the launch of Lexbase in the national number of surname changes unrelated to marriage, indicating no broad avoidance response through name changes, although isolated attempts cannot be ruled out. One likely reason is that the platform from the outset also allowed searches by personal identity number, the unique identifier assigned to Swedish residents at birth or immigration, so that a name change offered little protection.

card (in current prices). Payment gave access to the relevant judgment or other court documents, describing the alleged offenses, case disposition, and any sanction, together with identifying information contained in the documents. Appendix Figures A.1 and A.2 show the service as users encountered it.

In practice, users rarely purchased the underlying documents. Company records indicate that only 0.16 percent of the page views during the first year resulted in a purchase.[9] For the overwhelming majority of queries, the information treatment therefore consisted of the binary and highly visible indication that a person appeared in criminal court records. Users typically did not observe the precise offense, whether the charge resulted in conviction, or the circumstances of the case. This institutional feature is important for interpreting the effects of listing: Lexbase substantially increased the visibility of criminal-court involvement, but provided only limited contextual information to most searchers.

Interpersonal screening was part of the platform's stated rationale from the outset. In its launch announcement, Lexbase argued that consumers should have better information about people with whom they intended to form relationships.[10] Subsequent media reports provide direct examples of women using searchable court information to screen prospective partners and avoid men they perceived as potentially dangerous. One woman, for example, described such searches as the best available tool for avoiding entering a relationship with a violent man.[11] Company-reported survey evidence points in the same direction: approximately two-thirds of registered users were women, and checking a prospective partner for safety reasons was the most commonly reported use of the service.[12] Together, these sources indicate that Lexbase reduced information frictions also in decentralized partner markets.

Use of the platform was extensive. Company logs record 32 million page views during the first year after launch and more than 10 million person search quaries in the following years. These observations are searches rather than unique users, and may therefore include repeated queries by the same individual. Nevertheless, their scale indicates that the platform rapidly became a widely used source of criminal-history information. Appendix Figure A.4 plots the page views and person searches up until 2019 when the first close competing platform entered the market. Appendix Figure A.5 documents the accom-

[9]We lack data on person searches for 2014, but Appendix Figure A.4 shows that page views and person searches are of similar magnitude in subsequent years.

[10]See Lexbase, "Samtliga brottsmålsdomar från de senaste fem åren görs nu enkelt sökbara—Ny unik sajt lanseras idag," press release, January 27, 2014, Mynewsdesk.

[11]See SVT Kulturnyheterna, "Känner mig otrygg om jag inte kan kolla upp männen," November 20, 2024.

[12]Because the survey was conducted and reported by the company, these figures should be interpreted as descriptive evidence about its respondents rather than as estimates from a representative survey of Swedish users.

panying increase in print-media coverage, while Appendix Figure A.6 shows that, from the week after launch through the end of 2015, Lexbase generated more Swedish Google search interest than common terms such as "new car" and "best restaurants." At this scale, the information reached well beyond those who searched themselves: a woman who never used the platform could still learn of a partner's listing through friends, family, or colleagues who did.

The available evidence suggests that Lexbase substantially expanded access rather than simply displacing a close private substitute. No comparable general-public platform operated at the time of launch, and the introduction of Lexbase did not coincide with a meaningful break in employers' use of official criminal-record extracts (Appendix Figure A.7).[13] This comparison does not rule out some substitution away from direct requests to individual courts. It does, however, show that Lexbase operated at a scale far exceeding the established system of authorized institutional checks and reached users for whom obtaining court records had previously been cumbersome.

After launch, the company continued to update the database with new judgments, decisions, and docket sheets (*dagboksblad*) from courts across the country, but it did not remove records as they aged during our analysis period. We have found no evidence of any removal routine before 2018. Interviews with individuals involved in the platform indicate that no automated routine for removing published records existed at launch and that one was introduced around 2018, and the documentary record is consistent with this: the earliest public descriptions of Lexbase-specific retention rules date from 2018, and later descriptions specify removal after five years for offenses carrying a statutory maximum of no more than two years' imprisonment and after twenty years for more serious offenses.[14]

The service in the form studied here ended in 2025. On February 25, 2025, the Swedish Supreme Court held, in cases involving other legal-database providers, that courts could restrict or condition the bulk disclosure of criminal judgments when the recipient intended to make personal data broadly searchable or distribute them to customers. Lexbase subsequently replaced its person-search service with a platform based on editorially prepared legal-news articles, and in July 2026 the Court of Justice of the European Union held

[13]Official checks were also small relative to Lexbase search activity. In 2011, 171,229 official criminal-record extracts were requested, predominantly for employment-related purposes, compared with an average of approximately 20 million Lexbase searches per year after launch.

[14]See Lawline, "Hur länge finns domar i Lexbase?," October 20, 2018; "Hur länge finns någon kvar på Lexbase om det är stöldbrott med rättegång och böter som påföljd?," March 21, 2020; and "Vad gäller kring borttagning av uppgifter på hemsidor som Lexbase?," June 29, 2021. These sources do not establish a contemporaneous written company policy, the precise date on which the rule began, or whether it was applied consistently. Later reporting documents case-specific departures, but these events postdate our analysis; see Oico Integrity, "Nya Lexbase gallrar inte domar (längre)," August 25, 2025, and SVT Nyheter, "Grova brott städas bort från rättsdatabaserna—i hemlighet," September 7, 2026.

that making unedited criminal convictions available online for payment is not in principle journalistic processing exempt under Article 85 of the GDPR.[15] Both developments postdate our analysis period.

3 Data and sample construction

3.1 Administrative registers

We combine individual-level data from Statistics Sweden, the National Board of Health and Welfare, the Swedish National Council for Crime Prevention (*Brottsförebyggande rådet*, BRÅ), and Lexbase. The registers are linked using Sweden's unique personal identity numbers and cover the population registered in Sweden through 2018. Our main administrative panel begins in 2000. Because Lexbase launched in January 2014, we measure baseline characteristics and partnership status at the end of 2013 and follow post-launch outcomes from 2014 through 2018.[16] The earlier years are used to construct individual histories, predetermined characteristics, and pre-launch outcomes.

The Longitudinal Integrated Database for Health Insurance and Labour Market Studies (LISA), together with the Register of the Total Population, provides annual information on demographic and socioeconomic characteristics. These data include age, sex, country of birth, education, employment, labor earnings, disposable income, and receipt of social assistance and other transfers. Biological and adoptive links between parents and children are obtained from the Multigeneration Register. These links also allow us to identify shared children and to characterize defendants' partners and families.

We use the National Patient Register to measure contacts with specialized psychiatric and substance-use care. These records provide post-launch outcomes for male defendants and predetermined inputs in a separate exercise predicting women's risk of recorded violence. The register covers both inpatient care and physician encounters in specialized outpatient care. Online Appendix E reports the ICD and ATC codes used to construct the health outcomes. For the exploratory prior-history split, we also construct an indicator for an inpatient substance-use record during the ten years preceding the launch of Lexbase.

[15]The Supreme Court decisions primarily restricted databases' ability to obtain and redistribute new court records; they did not order Lexbase to remove its existing archive. The CJEU judgment, Case C-199/24, *ND v Legal Newsdesk Sweden AB*, arose from proceedings brought by an individual whose 2011 conviction had remained accessible on the platform until 2024, and addressed directly the publishing certificate on which the service's legal position had rested.

[16]We later show that listing does not significantly predict baseline partnership status.

3.2 Partnership measures

Our primary partnership measure is based on annual family links in the population registers. At the end of each calendar year, an individual is classified as being with a partner if the individual is registered at the same property as a spouse or registered partner, or as an unmarried partner with whom the individual has or has had a common biological or adopted child. The data identify both partnership status and the identity of the register-linked partner.

This is the standard measure used by Statistics Sweden to identify family links. It does not, however, capture unmarried cohabiting couples without common children or romantic relationships in which the partners do not live together, such as dating relationships. We therefore interpret the resulting outcomes as entry into and exit from *register-observed partnerships*. In the remainder of the paper, we use "partnership formation" and "separation" as shorthand for these transitions.[17]

For defendants who were partnered at the end of 2013, the baseline partner is the individual to whom the defendant was linked in that year. Relationship continuation equals one while the defendant remains linked to that baseline partner and zero thereafter. Repartnering does not reverse a separation from the baseline partner. For defendants who were not partnered at the end of 2013, partnership formation equals one when the defendant first becomes linked to any partner under the same register definition.[18] Our cumulative formation outcome indicates whether such a partnership is observed at any time from 2014 through 2018; the year-specific analyses use annual partnership states.

3.3 Criminal-case and violence data

We obtain individual-level criminal-case data from the Swedish National Council for Crime Prevention (BRÅ). These data originate from Swedish district courts, which are the courts of first instance. The unit of observation is a defendant. For each case, we

[17]The register-observed partnerships can be said to capture established partnerships with legal and financial consequences. Statistics Sweden can also provide a broader dwelling-based household measure that attempts to identify childless cohabiting couples using information on sex, age difference, kinship, and household composition. Although this measure potentially captures a sub-set of the relationships omitted by our primary definition, it is known to also misclassify non-partner co-residents as couples and is sensitive to errors in registered residence. These concerns are especially relevant for our sample. Shared and temporary housing is more common among younger adults (Andersson et al., 2023), and housing instability increases with criminal-justice involvement (Brottsförebyggande rådet, 2001). Our preferred measure is narrower but is consistently available throughout the study period and has high specificity because relationships are established through marriage or a common child. Dating and other non-cohabiting relationships remain outside the scope of the register-based analysis.

[18]We show that having an index decision on the post-boundary side does not significantly predict partnership status in 2013.

observe the relevant court, the date on which the district court issued its judgment or final decision, the offenses included in the case, and any sanction imposed. We refer to this date as the decision date. A single court case may contain several alleged offenses, which we retain separately when constructing measures of offense type and severity.

The distinction between alleged offenses and convictions is important in our setting. Lexbase could indicate that an individual had appeared as a defendant in criminal proceedings even when the proceedings did not ultimately result in conviction. We therefore use "charged defendant" or "defendant" when referring to the original court sample and reserve "convicted" for cases with a guilty disposition.[19]

Post-launch violence outcomes are constructed from BRÅ's register of suspected persons, which records suspicions that reached the level of reasonable suspicion (*skäligen misstänkt*) and were processed by the public prosecutor or the police. The same distinction between allegation and guilt applies here. The register's advantage is that it identifies the victim's gender and age and the offender–victim relationship.[20]

Our principal outcome indicates whether a male defendant was registered as reasonably suspected of a violent or sexual offense against an adult woman during 2014–2018, equal to one if at least one such suspicion is recorded over the five-year follow-up; the annual analyses use year-specific indicators.

Lexbase supplied a dated extract identifying the XXX,XXX court cases made searchable on the platform from its launch through 2016 check. Statistics Sweden linked these records to the anonymized administrative data. Using the date on which each record first became searchable, we reconstruct the database available at launch. We define *listed* as an indicator equal to one if a search for the defendant produced at least one criminal-case hit at launch. Thus, listing refers to the man's actual public visibility on the platform, not merely to whether his index case was formally retrievable. A defendant can be listed because of a case other than the index case, which is one source of imperfect compliance in the fuzzy design.

Lexbase also provided aggregate information on platform use, including the number of searches and selected characteristics of registered users. We use these data descriptively and to construct the search-intensity measures used in the heterogeneity analysis.

[19] Overall, defendants are found not guilty in only 6 percent of cases.

[20] Another benefit is that crimes against women often do not result in a conviction, so suspicions also capture incidents where the evidence was insufficient for a guilty verdict.

3.4 Defendant sample

The regression-discontinuity sample is constructed from male defendants whose index district-court decisions were issued between 2007 and 2011 CHECK and lie within 16 weeks of the relevant court-specific effective retrieval boundary, defined in Section 4. The running variable is the number of weeks between the decision date and the estimated boundary for the court.

We restrict the main sample to defendants aged 20–60 at the time of the index case and resident in Sweden at the launch of Lexbase. The main specification further restricts the sample to defendants with no district-court conviction before the index decision. Each defendant contributes a single index case.[21]

We divide the defendant sample according to relationship status immediately before the public-disclosure shock. Defendants linked to a partner at the end of 2013 constitute the baseline-partnered sample and defendants with no register-observed partner at the end of 2013 constitute the baseline-unpartnered sample.

3.5 Summary statistics

Table A.1 reports descriptive statistics for the full defendant sample and separately for defendants who were partnered and unpartnered at the end of 2013. Demographic, socioeconomic, relationship, and health characteristics are measured in 2013 unless otherwise indicated. Characteristics of the original criminal case—including offense type, disposition, and sanction—are measured at the index case rather than in 2013. The table also reports the number of defendants and courts contributing to each sample. CHECK

4 Retention limits and the RDD

4.1 Institutional assignment and effective retrieval boundaries

Our research design exploits discontinuities in the availability of older court cases when Lexbase assembled its launch database. These discontinuities arose from a statutory limit on the retention of searchable case information in the courts' case-management system, combined with the timing of Lexbase's court-by-court collection of records.

[21] Most non-disclosure and record-sealing policies explicitly target first-time defendants.

Section 5 of Decree (2001:639), then in force, required information concerning criminal cases to be removed from the courts' automated activity registers no later than five years after the calendar year in which the case was decided.[22] Deletion concerned the searchable case metadata in *Vera*, not the underlying judgment. The judgment remained a public document available from the court archives but became harder to locate through a person's name once its metadata had been removed. For Lexbase, obtaining older judgments that were no longer accessible electronically could require manual archival retrieval and digitization, adding costs beyond those of collecting existing electronic files.[23] The retention rule thus reduced the practical accessibility of older cases without changing their formal public status.

The retention period was defined by decision year rather than as a rolling 60-month window. Figure 1 illustrates the resulting assignment logic for a court whose records were retrieved in January 2014. If the court retained information until the statutory maximum, records from criminal cases decided in 2008 should have been removed by the end of 2013, whereas records from cases decided in 2009 could remain searchable until the end of 2014. The legally implied boundary in this example is therefore January 1, 2009. Cases decided just after that date were substantially more likely to be retrievable by Lexbase than cases decided just before it.

The legal rule generated the underlying discontinuity, but it did not determine the exact threshold in every court. Lexbase requested records from different courts at different times, courts differed in how quickly they processed those requests, and effective coverage could also be affected by permitted earlier deletion, occasional recovery of archived material, and incomplete delivery or matching of records. We therefore distinguish the legally implied boundary from each court's *effective retrieval boundary*: the decision week at which the probability that a case appeared in the Lexbase database increased most sharply.

We estimate the effective retrieval boundaries using the full linked data, including approximately 200,000 defendants who appeared in Lexbase, rather than the restricted regression sample. For each court and decision week, we calculate the share of all criminal cases in

[22]The five-year period was an outer limit rather than a guaranteed retention period; applicable rules could require information to be deleted earlier. Contemporaneous inspections provide some evidence on implementation. In inspections of Attunda and Västmanland district courts, the Swedish Data Inspection Board found that information in *Vera* was retained through the applicable statutory maximum and recommended that the courts consider whether some information could be deleted earlier. The inspections do not establish practice at every court or rule out case-specific technical deviations. See Datainspektionen, decisions 1316-2013 and 1319-2013, September 30, 2014.

[23]Public access did not generally entail a right to electronic delivery, and obtaining copies could entail fees. Sections 15–16 of the Fees Ordinance (1992:191) governed charges for paper copies, while Sections 4–5 permitted charges for supplying documents electronically; see HFD 2013 ref. 21. These charges, together with the work required to digitize paper records, could constrain the extent of bulk collection even though the underlying judgments remained legally accessible.

the administrative registers that appeared in the Lexbase launch database.[24] For each candidate boundary within the institutionally relevant search interval, we compare the mean inclusion share during the subsequent 16 weeks with the mean during the preceding 16 weeks. We define the court's effective retrieval boundary as the week associated with the largest increase.

The boundaries are determined before we impose the sex, age, criminal-history, relationship-status, and bandwidth restrictions defining the regression sample. We then construct the main estimation sample of approximately 6,000 male defendants whose index decisions fall within 16 weeks of the relevant court-specific boundary. Thus, the defendants included in the regression sample constitute only a small fraction of the much broader population used to locate the boundaries. Online Appendix F describes the search interval, the construction procedure, and the distribution of the resulting boundaries.

Crossing the effective retrieval boundary predicts, but does not perfectly determine, whether a defendant produced a Lexbase search hit at launch. Position relative to the boundary therefore serves as an instrument for actual person-level listing in the fuzzy regression-discontinuity design specified below.

4.2 The RDD

We estimate the fuzzy regression-discontinuity design on the sample described in Section 3.4. Let X_{ij} denote the number of weeks between the decision date for defendant i's index case and the effective boundary for court j, with week zero denoting the first post-boundary week. Let

$$Z_{ij} = 1\{X_{ij} \geq 0\}$$

indicate that the index decision falls on the post-boundary side of the threshold. The treatment D_{ij} equals one if a search for defendant i produced a Lexbase hit at launch. For outcome Y_{ij}, the local-linear first stage and reduced form are

$$D_{ij} = \alpha_0 + \alpha_1 Z_{ij} + \alpha_2 X_{ij} + \alpha_3 Z_{ij} X_{ij} + \mu_j^D + \lambda_{c(i)}^D + B_i'\Pi + \varepsilon_{ij}, \tag{1}$$

$$Y_{ij} = \beta_0 + \beta_1 Z_{ij} + \beta_2 X_{ij} + \beta_3 Z_{ij} X_{ij} + \mu_j^Y + \lambda_{c(i)}^Y + B_i'\Gamma + u_{ij}, \tag{2}$$

where μ_j^D and μ_j^Y are equation-specific court fixed effects, $\lambda_{c(i)}^D$ and $\lambda_{c(i)}^Y$ are equation-specific fixed effects for the calendar year of defendant i's index decision, and B_i contains predetermined covariates. In these specifications, α_1 captures the discontinuity in the probability of appearing in Lexbase and β_1 captures the reduced-form discontinuity in

[24] The linked data exclude defendants without a Swedish personal identity number.

the outcome. The corresponding 2SLS equation is

$$Y_{ij} = \tau D_{ij} + \rho_2 X_{ij} + \rho_3 Z_{ij} X_{ij} + \mu_j^{IV} + \lambda_{c(i)}^{IV} + B_i'\Theta + v_{ij}, \tag{3}$$

where D_{ij} is instrumented by Z_{ij}. With the same estimation sample, controls, and weights in all three equations, the just-identified 2SLS estimate satisfies $\widehat{\tau} = \widehat{\beta}_1/\widehat{\alpha}_1$.

Under continuity of the underlying potential-outcome means, exclusion, and monotonicity, the ratio of the boundary discontinuities has a local-complier interpretation: the effect of initial public listing for defendants whose listing status is changed by boundary assignment.[25] Because the running variable is observed only in weeks, continuity alone does not point-identify these discontinuities; estimation requires additional restrictions on the conditional mean functions (Kolesár and Rothe, 2018; Noack and Rothe, 2024).

We estimate equations (1)–(3) using $X_{ij} \in \{-16, \ldots, 15\}$, comprising 16 weeks on each side of the boundary, with a uniform kernel and separate linear trends on the two sides.[26] The linear specification provides a parsimonious approximation to gradual change across decision cohorts within a window containing only 32 possible weekly values of the running variable. Using the same bandwidth across outcomes and subgroups facilitates comparison of the estimates. We assess sensitivity to narrower and wider bandwidths and to a local-quadratic specification.

The main results report conventional confidence intervals based on standard errors clustered at the court level to allow for within-court dependence.[27] These intervals account for sampling uncertainty but do not explicitly account for potential bias from the linear approximation. As a robustness check, we also report bias-aware confidence sets that allow departures from the local-linear specification (Kolesár and Rothe, 2018; Armstrong and Kolesár, 2020; Noack and Rothe, 2024).

[25] Lexbase did not expunge records during the 2014–2018 observation period, but it continued to add records after launch. These additions included older cases collected before launch and records obtained subsequently, so initially unlisted defendants could become searchable later. The estimand concerns launch listing under these subsequent updating practices. The exclusion restriction requires that boundary assignment have no independent effect on outcomes through later publication.

[26] The bandwidth balances the precision gained from additional observations against several considerations favoring a local comparison. A wider window extends the range over which the linear approximation must hold and may encompass more seasonal variation in court activity and case composition. It also compares more distant decision cohorts, whose subsequent court involvement before launch may differ and independently generate listing. Our use of local-linear estimation follows Gelman and Imbens (2019), who recommend local-linear or local-quadratic fits over global high-order polynomials.

[27] Clustering by the running variable does not provide a general correction for specification bias and can yield confidence intervals with poor coverage when the running variable is discrete (Kolesár and Rothe, 2018).

4.3 Validating the research design

4.3.1 First stage and monotonicity

Estimating equation (1), we find that having an index decision on the post-boundary side increases the probability of being searchable on Lexbase at launch by 0.732 (standard error 0.033), or approximately 73 percentage points. The corresponding effective F-statistic (Montiel Olea and Pflueger, 2013) is approximately 490, indicating a strong first stage.

Panel (a) of Figure 2 shows the first stage. It plots the share of defendants who were publicly listed on Lexbase at launch against the decision date of their index case relative to the estimated effective retrieval boundary. As expected in a fuzzy design, the listed share does not jump from zero to one. Roughly 20 percent of defendants on the older side are nevertheless listed. A subsequent case may have made such a defendant searchable before launch; records older than the normal Vera horizon may also have been recovered from court archives, and the estimated boundary is measured with error. Conversely, about 10 percent of defendants on the more recent side are not listed, potentially reflecting incomplete retrieval or imprecision in the estimated boundary.

As a robustness check on the first stage, Panel (b) of Figure 2 uses a placebo boundary in the same calendar week one year earlier for each court. The outcome remains whether the defendant was publicly listed at the January 2014 launch. The figure shows no visible jump comparable to the discontinuity at the actual retrieval boundaries. Because the placebo preserves the calendar week, this pattern provides evidence against a recurring seasonal discontinuity in listing probabilities as an explanation for the main first stage. Check why the listing rate is so high.

Monotonicity is supported primarily by the institutional mechanism: moving an index decision to the more recent side of its court's effective retrieval boundary could make the defendant easier to retrieve but did not activate any countervailing rule that would remove an otherwise available launch listing. Consistent with this mechanism, Table A.2 reports positive and similar first-stage estimates across the principal subgroups. These estimates support the proposed mechanism but do not constitute a direct test of the individual-level no-defiers condition.

4.3.2 Continuity and exclusion

The timing of the index cases makes manipulation in anticipation of Lexbase implausible. The cases were decided several years before the platform's unexpected launch in January 2014. Although the effective retrieval boundaries are estimated retrospectively

from Lexbase coverage, the underlying decision dates were determined before launch. The identifying requirement is that case composition and other determinants of potential outcomes do not change discontinuously at these boundaries.

Table A.3 examines the relationship between observed characteristics, actual listing, and boundary assignment. Column (1) regresses treatment status, D_{ij}, on the covariates listed in Table A.1. Several characteristics significantly predict listing, including age, education, charge type, and selected sentence outcomes, and the joint test strongly rejects the null that all covariate coefficients are zero. These associations describe systematic differences between listed and unlisted defendants; they do not test continuity at the retrieval boundary.

Column (2) instead regresses the post-boundary indicator, Z_{ij}, on the same covariates. Here, we do not reject the null that the covariate coefficients are jointly zero ($p = 0.234$). Although nonrejection does not establish independence, this result is consistent with comparability across the boundary. The smooth patterns in individual covariates, including crime type and sentence severity, and the absence of detectable discontinuities in pre-launch outcomes provide additional evidence consistent with local continuity [add figure/table reference]. Appendix Figure A.8 also shows no visible discontinuity in the frequency of index decisions at the effective boundary.

The exclusion restriction additionally requires that position relative to the effective retrieval boundary affect post-launch outcomes only through Lexbase listing at launch. The absence of anticipatory manipulation does not establish this condition. However, the institutional setting helps distinguish the proposed mechanism from several alternative channels.

First, deletion from Vera concerned the courts' activity registers. It did not itself remove entries from the separate national criminal-record or suspicion registers used by criminal-justice authorities.[28] The loss of searchable court metadata therefore did not mechanically erase a defendant's history from those registers. This distinction makes an explanation based on the Vera rule directly altering official criminal-record information less compelling.

Second, deletion from Vera did not itself change the contents of an official criminal-record extract used in an employment check. Such extracts depended on the national register's own retention and disclosure rules. Retention varied with the sanction and could run from the decision date or from release, rather than from the end of the deci-

[28]These registers were governed by the Criminal Records Act (1998:620) and the Suspicions Register Act (1998:621), respectively, with their own provisions for registration, access, and deletion. See Sections 2 and 16–18 of the former and Sections 2 and 13 of the latter.

sion year.[29] These schedules were not determined by Lexbase's court-specific requests. An explanation operating through official extracts would instead require the separate national-register retention rules to generate an outcome discontinuity at the same effective retrieval boundaries.

Third, the Vera rule could affect access to records obtained directly from courts. The access costs described in the institutional background limited this route to information but did not eliminate it. If differential access had already affected relationships, employment, or offending before Lexbase, we would expect corresponding differences in pre-launch outcomes. The absence of detectable discontinuities in those outcomes therefore provides evidence against this channel, although it cannot rule out effects that emerged only after launch.

5 Results

This section proceeds in four steps. We first report the average effects of public listing on men's partnership status and recorded violence against women and document heterogeneity by relationship status at launch. We then examine mechanisms separately for men who were partnered and unpartnered at baseline. As a complementary descriptive exercise, we ask whether the contraction is also visible in the realized relationship states of a fixed female cohort. We conclude with a discussion that integrates the evidence on public visibility, exit, reduced partnership formation, selection among observed matches, and violence.

5.1 Main results

Panel A of Table 1 reports estimates for first-time male defendants aged 20–60 whose cases closed within a 16-week window around the court-specific cutoff. The outcomes are indicators for having a partner—being married or cohabiting with shared children—and for being recorded as a suspect of violence against women in any year during 2014–2018. We report four sets of estimates: OLS in column (2), the first stage in column (3), the reduced form in column (4), and 2SLS in column (5). All regressions include court and cutoff-year fixed effects, predetermined covariates, and local-linear controls for weeks from the cutoff with separate slopes on either side.

[29] For example, fines were generally removed five years after the judgment, decision, or approval of the relevant penalty order, whereas imprisonment was generally removed ten years after release. Subsequent entries could extend retention. See Sections 17–18 of the Criminal Records Act (1998:620) and SOU 2014:48, pp. 40–41.

The OLS estimate in column (2) associates public listing with a 3.8 percentage-point decline in the probability of having a partner during the subsequent five years. Columns (3)–(5) use post-cutoff assignment as an instrument for realized listing. In the main outcome specification, having a case close after the cutoff raises the probability of appearing in Lexbase by 73.2 percentage points, and the instrument easily passes standard weak-instrument diagnostics. The reduced-form estimate in column (4) implies that post-cutoff assignment lowers the probability of having a partner by 6.6 percentage points. Scaling this reduced form by the first stage, the 2SLS estimate in column (5) implies that public listing lowers the probability of having a partner by 9.0 percentage points, or approximately 21.5 percent of the sample mean.

Panel A also reports effects on whether the defendant was registered as reasonably suspected of physical or sexual violence against an adult woman during 2014–2018. The 2SLS estimate in column (5) indicates that public listing increases this probability by 5.6 percentage points.

Appendix Table A.4 presents corresponding estimates for the partnership outcome among female defendants. We find no statistically significant effect of public disclosure on this outcome; however, the sample size is substantially smaller, which reduces precision.[30]

Appendix Table A.5 shows results from robustness checks and presents additional specifications. Column (1) controls for the running variable by binning weeks and flexibly accounting for it using year-by-month fixed effects (e.g., Kolesár and Rothe, 2018). Column (2) increases the bandwidth to 20 weeks, while Column (3) reduces the bandwidth to 8 weeks and estimates a local randomization RD without controlling for the running variable. The estimates generally retain their signs and are similar in magnitude in the discrete-RD and 20-week specifications. Precision falls in the 8-week specification, where the partnership estimate is not statistically significant. Column (4) uses, as a placebo, the dependent variable measured in 2013, i.e., the year before the launch of Lexbase. These estimates are closer to zero and statistically insignificant. Appendix Figure A.9 juxtaposes the post-launch reduced-form discontinuities with the corresponding 2013 placebo outcomes. The post-launch outcomes display clear breaks at the cutoff, whereas the same outcomes measured before Lexbase launched show no visible discontinuity.

5.1.1 Heterogeneity by baseline relationship status

Panel B of Table 1 reports results for the subsample of men with a partner at baseline in 2013, while Panel C reports results for men without a partner at baseline. The estimates

[30]The appendix table does not report the violence-against-women outcome for female defendants.

reveal heterogeneity by baseline partnership status. Among men with a partner at baseline, public listing leads to a 7.8 percentage point decline in the probability of remaining partnered. In contrast, among men without a partner at baseline, public listing reduces the probability of forming a partnership by 8.7 percentage points. Turning to violence against women, we find a statistically insignificant 3.4 percentage point increase among men with a partner at baseline. For men without a partner at baseline, however, public listing increases the risk of violence against women by a statistically significant 6.1 percentage points.

Panel A of Figure **??** examines the dynamics of the effect of public listing on the probability of being partnered over the period 2014–2018 using year-by-year estimates. Figure (a) shows estimates for individuals with a partner at baseline, while Figure (b) shows estimates for individuals without a partner at baseline. Figure (a) indicates that the decline in partnership is immediate for men who already had a partner at baseline, with most of the effect occurring in the first year. In contrast, for men without a partner at baseline, the effect is more gradual and becomes larger over time.

Panel B of Figure **??** plots the corresponding year-by-year estimates for violence against women. Figure (c) shows no statistically significant change among men with a partner at baseline. In Figure (d), the point estimate is positive in several post-launch years—most clearly in 2014, 2016, and 2017—but the annual estimates are imprecise and are not uniformly elevated in every year. The timing is nevertheless inconsistent with an explanation based exclusively on violence within subsequently formed partnerships, because the partnership-formation response develops more gradually. We return below to evidence on violence against women outside current partnerships.

5.1.2 Interpretation of the IV estimates

The OLS estimates are systematically smaller in absolute value than the corresponding 2SLS estimates. One possible explanation is treatment-effect heterogeneity in the novelty or salience of public visibility. The fuzzy-RD estimate pertains to defendants whose listing status is changed by whether the index case falls within the retention window. For these defendants, Lexbase made an aging and practically obscure record newly searchable. Because they had not accumulated a newer offense that would independently return them to public view, listing could generate a comparatively large change in visibility.

By contrast, the group observed as listed in OLS also includes defendants who would have appeared on Lexbase regardless of the cutoff status of the index case. As an illustrative case, some may have newer criminal-justice contact that independently makes them visible. Their current or prospective partners may already know about that contact; their

behavior may convey a similar adverse signal even without a Lexbase search; or the newer case may already expose them to the audience and stigma consequences of listing. If the effect of listing is smaller in absolute value for defendants who would be listed regardless than for the cutoff-induced group, this heterogeneity could contribute to a smaller OLS association. But the OLS coefficient may also reflect selection in untreated potential outcomes, and neither the coefficient nor the OLS–IV gap identifies treatment-effect heterogeneity.

Imperfect recovery or linkage of Lexbase records may also contribute to the difference. To the extent that realized listing is misclassified approximately nondifferentially, measurement error would attenuate the OLS association. Because the same measured treatment enters the first stage, the precise implications for 2SLS depend on the structure of the measurement error, so we view this as a secondary explanation rather than a complete account of the OLS–IV gap. We therefore treat OLS as descriptive and use 2SLS as the preferred causal estimate. Online Appendix B provides a compact compliance-type interpretation, retains the first-stage illustration, and describes the control complier mean used to assess relative magnitudes.

5.1.3 Summary

Thus far we have seen that public listing reduces men's probability of being with a partner in the full sample and increases violence against women. The subgroup estimates differ by baseline partnership status. Among men with a partner at baseline, the partnership effect reflects increased separation with no detectable increase in violence. Among men without a partner at baseline, the partnership effect reflects reduced partner formation, and the violence effect is statistically detectable only in this group. Next, we ask which mechanisms may explain these findings and their implications.

5.2 Mechanisms among men with a partner at baseline: public visibility and exit

We begin with men who had a partner in 2013. Public listing could lower the continuation value of these relationships through several channels. A woman may learn about previously unknown criminal-court involvement, reassess information she already possessed, or attach greater weight to it once it becomes a salient public label. Listing may also matter because relatives, friends, or co-workers can discover the record, potentially imposing reputational or associative-stigma costs on the couple. We do not observe individual searches, partners' prior knowledge, or who initiated a separation and therefore cannot

separate these channels. We first examine whether the response varies with dimensions that may affect the novelty, salience, or reach of disclosure. We then ask whether women with stronger outside options were better positioned to leave following a reduction in relationship surplus and examine their subsequent outcomes.

5.2.1 Novelty, salience, and the reach of public disclosure

Table 2 examines heterogeneity within the partnered-at-baseline sample along dimensions that may affect the novelty, salience, or reach of public disclosure. We begin by asking whether the woman observed as the man's partner at the end of 2013 was already together with him at the time of the index trial. Women already partnered with the man during the proceedings had a greater opportunity to know about the case before Lexbase, whereas women whose relationships formed afterward may have had less prior information. The point estimate implies a 5.7 percentage point reduction in remaining with the baseline partner when the couple was already together at the index trial and a 10.5 percentage point reduction when the relationship formed later; only the latter estimate is statistically significant. This pattern is consistent with a larger role for direct learning in relationships formed after the case, but it is not a clean test. Later-forming relationships may also contain less relationship-specific capital or have lower costs of dissolution. Appendix Table A.6 reports the complementary split by relationship length. We interpret both patterns as suggestive evidence that public listing has larger consequences in newer relationships, rather than as proof of belief updating.

Next, we investigate whether the effect differs by the severity of the initial charge. We define a felony as a crime type for which first-time defendants in the population at some point receive a prison sentence and a misdemeanor as a crime type for which they never receive one.[31] The point estimate implies a 15.7 percentage point decline for men initially charged with a felony offense, while the estimate for misdemeanor charges is close to zero. A more serious underlying case may make public visibility more consequential. Importantly, however, the free Lexbase result did not reveal offense type or severity, and only 0.16 percent of searches resulted in a document purchase. The severity gradient therefore should not be interpreted as evidence that ordinary searchers learned offense severity from Lexbase. It could arise if the woman or her network already knew enough to connect the listing to the underlying case, if a small subset obtained the judgment, or if public visibility amplified the salience or reputational cost of more serious criminal-court involvement.

Finally, we examine heterogeneity by age-group platform engagement using data pro-

[31] Sweden does not formally classify crimes as felonies or misdemeanors, as is common in, for example, the United States.

vided by Lexbase on the age distribution of searches by registered users. Although most individuals used the platform without creating a profile, registered users were asked to report their age. We classify women as belonging to a higher-engagement age group if their age group accounted for at least 18 percent of registered-user searches; the split is close to the median of the age-specific distribution. This measure captures the potential reach of Lexbase within an age group. It does not show whether a particular female partner searched and may also proxy exposure through her social network. The point estimate implies a 12.5 percentage point reduction among partners in higher-engagement age groups and is smaller and statistically insignificant in lower-engagement age groups.

Appendix Table A.6 combines relationship length and age-group platform engagement. The largest point estimates occur for newer relationships involving women in age groups with greater platform engagement. This pattern is consistent with a stronger response when disclosure is more likely to reach the woman or her network and when the public signal is more consequential. It does not distinguish direct learning from salience, relationship-specific capital, or audience effects, and we therefore treat it as suggestive mechanism evidence. Because statistical significance in one subgroup and not another does not itself establish heterogeneity, a covariance-adjusted test of equality is required before concluding that the effects differ statistically.

Appendix Table A.7 reports an exploratory split by pre-launch municipality population density. The estimated effect on relationship continuation is -14.7 percentage points in below-median-density municipalities and -1.2 percentage points in above-median-density municipalities. This pattern is consistent with public visibility having greater consequences where anonymity is lower, social networks overlap more, or association with a publicly listed partner carries larger audience costs. Population density is not a direct measure of information diffusion or stigma, however, and may also proxy for market thickness, mobility, prior information, socioeconomic composition, or other local conditions. Because a pooled equality test and subgroup first stages are not available, we treat the split as suggestive rather than as evidence that density identifies a specific channel.

5.2.2 Outside options and capacity to exit

Table 3 provides evidence aimed at better understanding why partnered men were less likely to remain partnered after public disclosure. Previous research emphasizes the importance of women's relative resources and outside options for household decisions and relationship stability (e.g., Aizer, 2010; Becker, 1973; Chiappori et al., 2017). We follow Aizer (2010) and measure relative bargaining power within the household using relative earnings. We complement this with information on relative educational attainment (years

of schooling). We then stratify the sample according to whether the female partner has higher earnings or more education than the male partner.

The point estimates in Table 3 are larger in couples where the female partner has relatively greater bargaining power. In column (1), we find a statistically significant 13.4 percentage point reduction in the probability that the male remains partnered in couples where the female partner has relatively higher educational attainment. By contrast, the estimate for couples in which the male has more education than the female partner is smaller and statistically insignificant. A similar difference in point estimates emerges in columns (3) and (4), where we stratify the sample by relative earnings.

These patterns are consistent with public listing being more likely to translate into separation when the female partner is better positioned to leave. Relative earnings and education may capture outside options, bargaining position, or the practical cost of exit. The splits are suggestive of whether a woman can act on a reduction in relationship surplus; they do not identify whether that reduction arises from direct learning, increased salience, or audience and stigma effects. As with Table 2, inference about heterogeneity should rest on formal tests of equality across subgroup estimates.

5.2.3 Downstream outcomes for women

Although public disclosure could in principle destabilize relationships in ways that harm women, it is also possible that open-access crime databases allow women to avoid or exit risky matches. We cannot study effects on women who were not already in a relationship with the listed man, but we can examine downstream outcomes for women who were partnered with these men at baseline.

In this analysis, we focus on two measures of socioeconomic outcomes: *Non-employed*, an indicator for the female being non-employed on November 1 in any year during 2014–2018, and *Welfare use*, an indicator for having received welfare in any year during 2014–2018. We complement these measures with data on inpatient and outpatient care: *Psychiatric care*, an indicator for having been treated at a mental health care facility in any year during 2014–2018, and *Alcohol/drug use*, an indicator for having been diagnosed with alcohol or drug abuse during 2014–2018.

Table 4 reports the downstream outcomes. In the full partnered sample reported in Panel A, public listing reduces the probability that the female partner is non-employed by 11.8 percentage points, statistically significant at the 10 percent level. The estimates for welfare receipt, psychiatric care, and alcohol/drug use are statistically imprecise.

To better understand these findings, we ask whether the effects differ according to cou-

ples' predicted risk of separation. We construct the prediction using only characteristics measured before Lexbase and split the partnered sample at the median of the resulting risk score. The prediction is used only to define heterogeneity groups; it is not interpreted as a causal model. The model has an adjusted R^2 of about 0.24, and approximately 90 percent of observed separations occur in the above-median group. This descriptive concentration should not be interpreted as an out-of-sample classification rate. Nor does the high-risk group identify the theoretically marginal couples whose continuation decision is changed by listing.

In the high predicted-risk subgroup, the estimate implies a 23.7 percentage point reduction in non-employment, statistically significant at the 10 percent level, whereas the corresponding point estimate is closer to zero in the low predicted-risk subgroup. We detect no statistically significant deterioration in psychiatric care, alcohol- or drug-related diagnoses, or recorded violence against women. These null estimates should not be interpreted as proving the absence of adverse effects. Nor does employment alone sign women's welfare: it may reflect greater autonomy, increased labor supply following separation, or the loss of household resources. Given the subgroup split and multiple outcomes, we treat the economic-outcome pattern as suggestive rather than conclusive.

5.3 Why does recorded violence rise when fewer relationships form?

Panel C of Table 1 and Figure **??** show the central puzzle among men who were unpartnered at launch: public listing lowers entry into register-observed partnerships but raises recorded suspected violence against women. These outcomes are not mechanically inconsistent because the partnership measure excludes dating relationships, rejected approaches, former partners, and other known women. We first ask whether the result is simply part of general recidivism, then examine four patterns that may help organize the violence response. After the complementary woman-level exercise, we return to the possibility that the recorded increase reflects reporting or case processing. These exercises provide mechanism evidence but do not constitute a causal mediation analysis.

5.3.1 Is this simply general recidivism?

Panel A of Table 5 compares post-launch offending across broad categories. Under the common decomposition used for this exercise, public listing increases recorded violence or sexual offending against women by 7.8 percentage points. The corresponding estimates for violence or sexual offending against others, property crime, and other crime are smaller

and statistically imprecise. The estimate for DUI or drug offending is also positive, at 5.7 percentage points, and is statistically significant at the 10 percent level. The decomposition dates offenses by the date of the alleged conduct and pools physical and sexual offenses, so its violence-against-women estimate is not directly comparable to the 6.1 percentage point headline estimate.

The increase in recorded offending is therefore not broad-based across the major offense categories. The largest and most precisely estimated response is for violence or sexual offending against women, with a weaker positive response for DUI or drug offending. Because the remaining estimates are imprecise, however, we cannot claim that the effect is unique to violence against women or rule out smaller changes in other forms of offending. No aggregate "any recidivism" estimate is available, and comparisons across categories should be based on covariance-adjusted tests rather than on differences in statistical significance alone.

5.3.2 What other explanations are there?

The offense-category results do not reveal why violence against women rises. We next examine four patterns that distinguish broad classes of explanation. None separately identifies a causal channel or constitutes a mediation analysis.

1. Does Lexbase create violence—or amplify existing risk? Panel C of Table 5 divides initially unpartnered men by whether they had a documented suspicion of violence or sexual offending against women before Lexbase. The point estimate is substantially larger among men with prior violence against women: 33.8 percentage points, compared with 3.0 percentage points among men without such a history. This pattern is consistent with public listing amplifying risk among already high-risk men rather than generating a uniform response. The high-risk subgroup is small, however, and subgroup fuzzy-RD estimates may represent different complier populations. We therefore treat the split as suggestive. A formal interaction test and subgroup first stages are required before drawing a conclusion about statistical heterogeneity.

2. Does listing lead to distress or marginalization? Table 6 examines labor-market and health outcomes among men who were unpartnered at launch. We detect no deterioration in the measured labor-market outcomes: the estimates for non-employment and welfare receipt are small and statistically imprecise. Public listing increases the probability of an alcohol- or drug-related diagnosis by 7.7 percentage points. The psychiatric-care estimate is also positive but imprecise.

The increase in diagnoses is accompanied by a 5.7 percentage point increase in recorded suspicions of DUI or drug offending in Panel A of Table 5. Because the latter comes from criminal-justice rather than health-care records, increased health-care contact alone cannot account for both findings. The criminal-justice outcome may nevertheless reflect enforcement or detection as well as behavior, and the two outcomes do not establish that distress or substance use mediates the violence effect. The absence of detectable labor-market effects likewise does not rule out other forms of marginalization.[32]

Appendix Table A.10 splits the violence outcome by two pre-Lexbase measures of substance-related history. The violence estimate is larger among men with a prior DUI or drug suspicion, whereas it is larger among men without a prior inpatient substance-use record. The subgroup estimates are imprecise and provide no consistent evidence that the violence response is confined to men with documented pre-existing substance-related problems. These splits do not show that the post-launch diagnosis response represents newly initiated substance use: the outcome in the table is violence against women, not a post-launch substance-related diagnosis, and the absence of a register record does not imply the absence of pre-existing use. Establishing incidence would require estimating a first post-launch diagnosis among men without a comparable pre-launch record.

3. Does listing change the composition of observed matches? A separate potential channel operates through selection into the smaller set of partnerships that still forms. Table 7 describes the characteristics of women in the partnerships observed to form among men who were unpartnered at launch. Public listing is associated with a 15.8 percentage point increase in the probability that an observed partner belongs to the top quartile of the predicted IPV-risk distribution.[33] Observed partners are also more likely to have a large age gap with the man, to receive welfare and psychiatric care in the measured windows, and less likely to meet the table's higher-education threshold.

The joint pattern is consistent with selection toward women with fewer observable resources among the partnerships that remain. Because partnership formation is itself affected by listing, however, these conditional estimates are descriptive. They do not identify a causal change in any woman's characteristics, recover effects for a fixed principal stratum, or establish partner composition as a mediator of violence. The estimate for time to partnership and the full set of partner characteristics are reported in Table 7.

[32]Employers may also have had access to criminal-history information through official background checks, potentially limiting Lexbase's marginal effect on employment.

[33]The prediction is obtained by relating predetermined covariates to the probability of receiving inpatient or outpatient treatment related to IPV.

4. Is the violence increase driven by changes in matching? If selection within newly formed partnerships were the entire explanation, the increase in violence should be confined to current partners. Panel D of Table 5 instead shows positive estimates for recorded violence against both current partners and known non-partners. Victim relationship is observed only from 2016 onward. Known non-partners include former partners and acquaintances and do not directly identify rejected matches. The evidence therefore shows that the violence response is not confined to newly formed register-observed unions; it does not identify the specific relationship interaction in which the additional violence arose.

Appendix Table A.8 provides ancillary evidence on matching channels and mobility. Conditional on observing a partnership, we find no detectable shift toward matches from measured childhood or adult social networks, and there is no detectable effect on residential mobility. These estimates are imprecise and selected in the matching columns, so we treat them as robustness evidence rather than as central mechanism tests.

The male-side evidence documents a contraction in register-observed partnership formation, differences in the matches observed to form, and violence extending beyond current partnerships. We next ask whether a related allocation pattern is visible from women's side.

5.4 Relationship allocation in a fixed female cohort

The male-side regression-discontinuity results show that public listing reduces register-observed partnership formation among men who were unpartnered at launch. We next ask whether a corresponding pattern is visible from women's side. We follow a fixed cohort of women who were single and age 20–35 in 2010 and observe their annual relationship states from 2011 through 2018.

For each woman-year, we distinguish four mutually exclusive states: no register-observed partner, a partner outside the convicted-male cutoff sample, an unlisted convicted partner in that sample, and a listed convicted partner in that sample. We expand each woman-year into one observation for every possible state and estimate a stacked state-occupancy event study with woman-year and destination fixed effects. The plotted coefficient is the probability of being partnered with a listed convicted man minus the probability of being partnered with an unlisted convicted man, relative to the same difference in 2013. Online Appendix C gives the exact specification.

Figure **??** shows that the listed-minus-unlisted contrast is close to zero before Lexbase. It turns negative in 2014 and becomes progressively more negative through 2018. By 2018,

the contrast is approximately -9×10^{-5}, or -0.009 percentage points, relative to 2013. The absolute magnitude is small because partnerships with men in either focal state are rare in the full female cohort.

This exercise measures realized current-state exposure rather than entry into a partnership alone. It therefore incorporates partnership formation, persistence, dissolution, re-partnering, and remaining single. It also differs from the male-side design in an important respect: the partner states are classified by actual Lexbase listing rather than by cutoff assignment. Listed and unlisted convicted men may differ in other ways, so we interpret the figure as descriptive evidence on relationship allocation that complements the causal male-side estimates. It is not a separate causal estimate of listing and does not identify women's searches or unilateral screening decisions.

5.5 Could the increase simply reflect more reporting?

Because the baseline outcome begins when an incident is registered at the reasonable-suspicion threshold, it can respond to changes in reporting or detection as well as to changes in underlying violence. We therefore examine the unconditional probability that a post-launch violence-against-women case advances to trial. Reaching trial requires the report to survive investigation and the prosecutor to judge the evidence sufficient to proceed, making it a more demanding case-processing threshold than a recorded suspicion. Panel B of Table 5 shows that public listing increases this probability by 4.5 percentage points. The result is difficult to reconcile with the narrow hypothesis that Lexbase generated only additional weak or unfounded reports that were screened out. It does not rule out increased reporting of genuine incidents, however, or changes in detection, investigation, prosecution, victim cooperation, or other features of case processing. We use this trial outcome because a complete post-launch conviction estimate was not supplied and must not be inferred from baseline conviction measures.

6 Discussion

Taken together, the results reveal two distinct margins through which public criminal-record disclosure changes relationship outcomes. For men already in relationships, listing lowers the probability that the incumbent relationship continues. The response is rapid, and the point estimates are largest in newer relationships, for more serious underlying charges, in higher-engagement age groups, and when women have stronger outside options. The separation point estimate is also substantially larger in below-median-density

municipalities, although that split is exploratory and population density is an ambiguous proxy for social diffusion or audience costs. These patterns are consistent with public visibility reducing continuation surplus and with larger relationship responses where women have greater relative resources; they do not establish a capacity-to-exit, learning, or stigma mechanism. Nor do they establish that incumbent partners first learned about the criminal history from Lexbase. Listing could instead make partly known information more salient or credible, diffuse it through social networks, or create audience and associative-stigma costs because family, friends, and co-workers can now discover it. Because we observe neither searches, prior knowledge, nor who initiated separation, our estimates identify the combined effect of public searchability rather than the separate contribution of these channels. Women's non-employment falls in the high predicted-risk subgroup, but this pattern is suggestive and does not identify welfare gains.

For men who were unpartnered at launch, listing reduces register-observed partnership formation, increases alcohol- or drug-related diagnoses, and yields a positive but imprecise estimate for psychiatric care. The criminal-justice registers also show a positive, marginally significant increase in DUI or drug offending, so the substance-related pattern is not confined to health-care contacts. Recorded offending does not rise detectably across violence against others, property crime, and other crime, although the estimates are imprecise and do not establish that the response is unique to violence against women. Among partnerships observed to form, partner characteristics differ in a pattern consistent with selection toward women with fewer observable resources. Recorded violence against women rises, survives the higher threshold of being brought to trial, and extends beyond current partners to other known women. Prior-substance-history splits do not consistently locate this violence response among men with documented pre-existing substance problems, but they do not establish that the post-launch diagnosis response is new. The joint pattern is consistent with reduced relationship access, substance-related responses, and changes in the contacts or matches that remain; the evidence does not identify these variables as causal mediators. In particular, the formation response could reflect potential partners' acceptance decisions, men's own search or withdrawal, or both.

A complementary woman-level event study yields a consistent allocation pattern. In a fixed cohort of women who were single before Lexbase, the relative probability of being partnered with a listed rather than an unlisted convicted man is flat before launch and becomes increasingly negative afterward. Because this exercise uses actual listing rather than cutoff assignment, it is descriptive and cannot distinguish women's screening from changes in men's search, relationship persistence, separation, re-partnering, or other adjustments.

Baseline relationship status indexes different feasible transition margins. In an incumbent

relationship, listing can change the surplus from continuing an existing match. Among unpartnered men, it can change access to new matches, search behavior, and the composition of the relationships and contacts that remain. This distinction helps organize the dynamics: separation is immediate among partnered men, whereas the deficit in register-observed partnership formation accumulates over time among initially unpartnered men. Because baseline status is not randomly assigned and subgroup 2SLS estimates may pertain to different complier populations, the cross-group contrast should not be interpreted as a causal effect of relationship status itself.

The coexistence of fewer formal matches and more violence is not paradoxical. The register-based partnership measure captures established relationships, not rejected approaches, dating interactions, former partnerships, or other social contacts. A lower probability that interactions develop into registered partnerships can therefore coexist with rejection, distress, substance use, or other responses, although those channels are not directly observed. Because the conditional match-composition estimates are selected, they should be interpreted as descriptive evidence on observed matches rather than as causal effects for a fixed population. The evidence that violence also rises against known women outside current partnerships further supports an interpretation that extends beyond conflict inside newly formed unions.

Appendix Table A.11 summarizes the mechanism evidence by baseline partnership status. Appendix Table A.12 documents the corresponding observed differences. Unpartnered defendants are less educated, less attached to the labor market, more likely to reoffend, and more psychologically vulnerable than partnered defendants. This selection may make public listing a particularly severe shock for men who already occupy a weak position. Baseline status also indexes whether the immediately available empirical margin is continuation of an incumbent match or access to new matches; it does not itself randomly assign men to those margins.

Online Appendix D presents a simple framework for the paper's central baseline-status distinction. Public listing can induce separation when the continuation surplus from an incumbent relationship falls below the partner's outside option. For an initially unpartnered man, an acceptance channel can lower match formation and select the matches that remain toward women with weaker outside options. Public visibility may also affect men's distress directly or through interactions that do not become formal matches. The framework is an organizing device rather than a structural model or a causal mediation analysis.

The broader policy implication is that the consequences of disclosure depend on both the target's relationship status and the audience given access. Making records searchable can facilitate screening and exit from some incumbent relationships. Universal access,

however, differs from confidential disclosure to a potential victim: it may also impose reputational or associative-stigma costs on listed individuals and their partners even when the underlying history was already known. Among individuals who were unpartnered at disclosure, listing may reduce access to matches and induce adverse behavioral responses. The design identifies the total behavioral response to public visibility, not the separate informational and stigma channels, and the available outcomes do not sign aggregate welfare.

7 Concluding Remarks

We study the unexpected launch of Sweden's first large-scale online criminal-record database, which transformed criminal-court information that was legally public but practically difficult to obtain into a coarse signal that was immediate, anonymous, and searchable at scale. Court-specific retention rules generate quasi-random variation in whether otherwise similar defendants appeared on the platform. The design therefore isolates the causal effect of public listing from the underlying criminal charge and formal sanction. It does not separately identify whether listing changes a particular woman's private information, increases the salience of information already known, or imposes costs through wider social visibility.

The subgroup estimates differ by defendants' relationship status at launch. For men already in relationships, public listing produces rapid separation, with larger point estimates in newer relationships and when the woman has stronger outside options. Women's non-employment subsequently falls in the high predicted-risk subgroup, although the pattern is suggestive. For men without a partner, listing reduces new partnership formation, increases alcohol- or drug-related diagnoses, and produces a pattern of partner characteristics among selected observed matches consistent with greater observable disadvantage. The offending response is not broad-based across the major categories, although DUI or drug offending also increases at the 10-percent level. Recorded violence against women rises in this group, appears for cases brought to trial, and extends beyond current partners to other women known to the defendant.

A complementary woman-level event study shows that, after launch, a fixed cohort of women became progressively less likely to be partnered with listed rather than unlisted convicted men, relative to the same contrast in 2013. This comparison describes a shift in realized relationship allocation. Because it uses actual listing rather than cutoff assignment, it is not a separate causal estimate of listing or a direct estimate of women's screening behavior.

The joint pattern reveals an asymmetry between continuation and formation. Public searchability can lower the continuation surplus of an incumbent relationship and facilitate exit. At the same time, it can reduce access to future matches, alter the composition of observed relationships, and generate adverse responses among men who were already unpartnered. These effects are not contradictory: the same public signal can affect incumbent relationships, prospective matches, and the behavior of the person whose record becomes visible through different margins.

Our setting provides a useful benchmark for other digital disclosure regimes, including online court portals, commercial background-check services, and dating-safety applications. Their consequences are likely to depend on which records are disclosed, how much contextual information users receive, how long records remain visible, and whether access is confidential, targeted, or universal. More generally, reducing information frictions while broadening the audience for a stigmatizing attribute can create potential screening benefits alongside exclusion and behavioral costs. Evaluating public disclosure therefore requires tracing its effects across both incumbent relationships and the market for new matches rather than treating it as a pure shock to private information.

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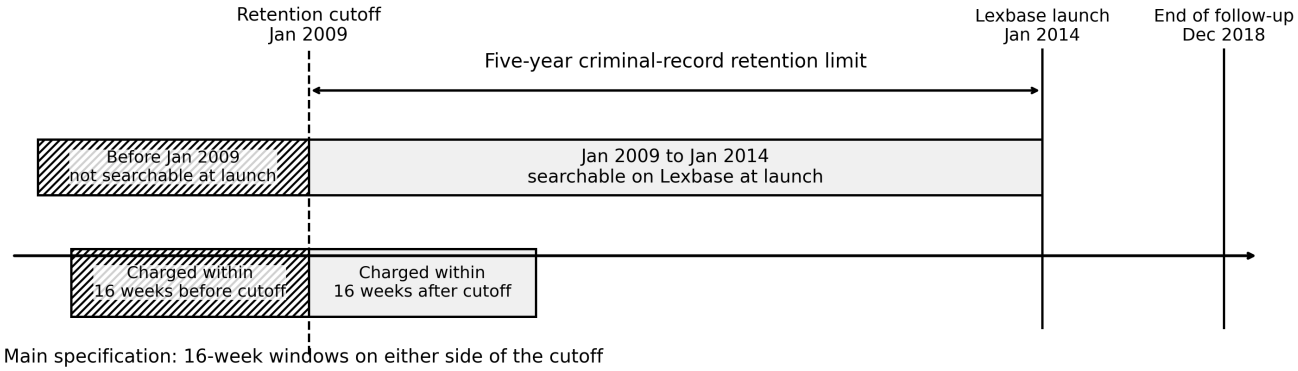
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Figures and Tables

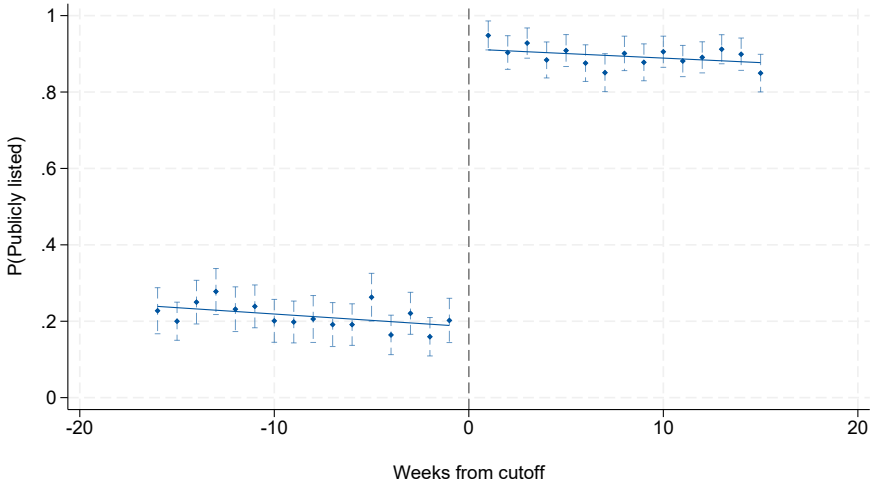
Figure 1: The retention period and treatment status



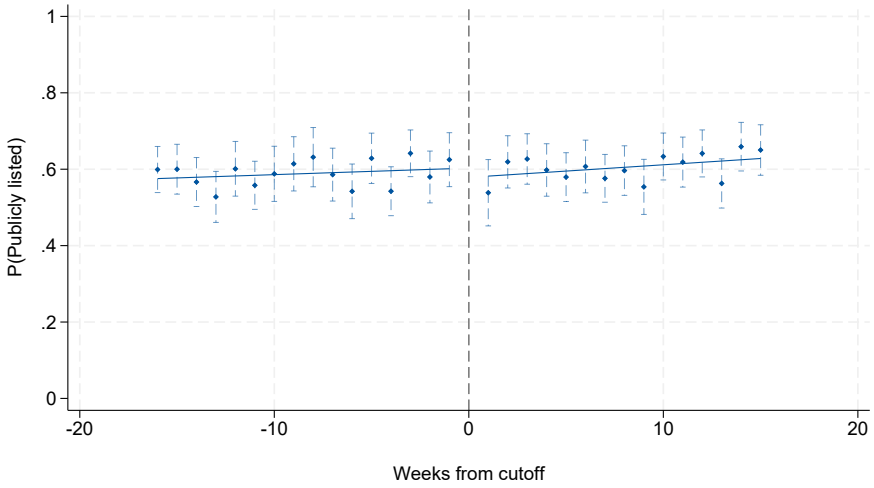
Note: Schematic (not to scale) illustration depicting the RD design for a hypothetical courthouse whose records were obtained in January 2014, when the Lexbase website was launched. With a five-year retention limit, the retention cutoff is January 2009. In our main specification, the treatment group consists of individuals who were first charged with a crime during the first 16 weeks of 2009. The comparison group consists of individuals first charged during the last 16 weeks of 2008. Outcomes are computed over the follow-up period extending from the January 2014 Lexbase launch date to December 2018.

Figure 2: First Stage and Placebo Test

Panel A: Actual court-specific cutoff



Panel B: Placebo cutoff one year earlier



Notes: Panel A plots the probability that a defendant produced at least one Lexbase search hit at launch against the decision week of the defendant's index case relative to the court-specific effective retrieval boundary. Panel B repeats the analysis after assigning each court a placebo boundary exactly 52 weeks before its actual boundary. The vertical dashed line marks the relevant boundary in each panel. Points show weekly means, vertical bars report 95 percent confidence intervals, and the solid lines are linear fits estimated separately on either side of the boundary. The positive listing probability to the left of the actual boundary reflects, among other things, defendants who were searchable because of another case or because an older record was recovered through a different route. The listing probability below one to the right reflects incomplete retrieval, linkage error, and imprecision in the estimated boundary. Each panel uses male defendants aged 20 to 60 with no prior district-court conviction whose index decisions fall within 16 weeks of the boundary shown in that panel. Confidence intervals are based on standard errors clustered at the court level.

Table 1: The effect of public listing on the probability of having a partner and of violence against women

	Outcome mean	OLS	FS	RF	2SLS
	(1)	(2)	(3)	(4)	(5)
Panel A: Full sample					
Has partner	0.419	-0.038**	0.732***	-0.066**	-0.090**
	(0.493)	(0.018)	(0.033)	(0.032)	(0.043)
Violence against women	0.058	0.040***	0.732***	0.041***	0.056***
	(0.235)	(0.007)	(0.033)	(0.015)	(0.021)
Obs.		6,160	6,160	6,160	6,160
Panel B: Males with a partner at end of 2013					
Has partner	0.955	-0.016	0.777***	-0.060**	-0.078**
	(0.208)	(0.012)	(0.051)	(0.026)	(0.035)
Violence against women	0.039	0.003	0.777***	0.026	0.034
	(0.194)	(0.013)	(0.051)	(0.025)	(0.033)
Obs.		1,562	1,562	1,562	1,562
Panel C: Males without a partner at end of 2013					
Has partner	0.236	-0.028*	0.719***	-0.063*	-0.087*
	(0.425)	(0.015)	(0.033)	(0.033)	(0.047)
Violence against women	0.065	0.050***	0.719***	0.044**	0.061**
	(0.247)	(0.009)	(0.033)	(0.019)	(0.027)
Obs.		4,598	4,598	4,598	4,598

Note: The table reports estimates of the effect of public listing on having a partner at year-end in any year during 2014–2018 and on being suspected of violence against women. Panel A reports estimates for the full sample, Panel B for males with a partner at the end of 2013, and Panel C for males without a partner at the end of 2013. The sample consists of male defendants aged 20 to 60 who were first charged within a 16-week window around the court-specific cutoff. All regressions include year and court fixed effects and control flexibly for weeks-to-cutoff, allowing for different slopes on either side of the cutoff. Controls include age, immigrant status, high school degree, college degree, and missing education. Standard errors, clustered at the court level, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Heterogeneity in Prior Exposure, Charge Severity, and Platform Engagement

	Together at index trial	Not together at index trial	Felony	Misdemeanor	Higher platform engagement	Lower platform engagement
2SLS effect	−0.057	−0.105*	−0.157*	−0.008	−0.125**	−0.022
	(0.045)	(0.054)	(0.090)	(0.035)	(0.050)	(0.064)
Sample mean	0.966	0.940	0.937	0.971	0.961	0.945
Observations	878	684	751	618	967	595

Notes: The sample consists of male defendants aged 20–60 who had a register-observed partner at the end of 2013 and whose index cases closed within 16 weeks of the relevant court-specific cutoff. The outcome indicates remaining with that baseline partner during 2014–2018. Entries are 2SLS estimates of the effect of public listing, instrumenting listing with an indicator for case closure after the cutoff. All specifications include the fixed effects, predetermined controls, and local-linear running-variable terms used in the main specification; standard errors are clustered by court. Columns (1)–(2) classify couples according to whether the woman observed as the man's partner at the end of 2013 was already partnered with him at the index trial. Felony denotes a crime type for which a first-time defendant in the population ever receives a prison sentence; misdemeanor denotes a crime type for which none does. Higher platform engagement denotes female age groups accounting for at least 18 percent of searches among registered users. The free Lexbase result did not reveal offense severity, and these subgroup contrasts do not separately identify learning, salience, relationship-specific capital, social diffusion, or stigma. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Heterogeneity by women's bargaining power

	Partner has higher education	Partner has same or lower education	Partner's earnings share> median	Partner's earnings share $\leq$ *median*
	(1)	(2)	(3)	(4)
	-0.134***	-0.050	-0.105**	-0.052
	(0.049)	(0.039)	(0.051)	(0.047)
	[0.955]	[0.954]	[0.964]	[0.949]
Court & Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Obs.	627	935	771	771

Note: The table shows 2SLS estimates of the effect of public listing on having a partner at year end in any year 2014-2018. The sample consists of male defendants aged 20 to 60 who had partners at the end of 2013 and who were first charged within a 16-week window around the court-specific cutoff. All regressions control for year and court fixed effects and age, immigrant status, high school degree, college degree, missing education. Partner's education is measured with number of years of schooling imputed from data on highest completed level of education. Partner's earnings share is partner's earnings divided by household earnings. Bargaining power is measured in 2013. We control for baseline educational attainment of both the male and the female in columns 1 and 2 and baseline earnings for both in columns 3 and 4. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table 4: Outcomes for women with a partner in late 2013

	Non-employed	Welfare use	Psychiatric care	Alcohol/drug use
	(1)	(2)	(3)	(4)
Panel A: Full sample				
Full Sample	-0.118*	-0.088	0.053	0.061
	(0.068)	(0.054)	(0.040)	(0.043)
	[0.350]	[0.918]	[0.089]	[0.090]
Panel B: By predicted risk of separation				
High predicted risk	-0.237*	-0.159*	0.061	-0.017
	(0.139)	(0.084)	(0.076)	(0.037)
	[0.421]	[0.870]	[0.112]	[0.012]
Low predicted risk	-0.096	0.003	0.040	0.011
	(0.083)	(0.030)	(0.050)	(0.021)
	[0.254]	[0.976]	[0.062]	[0.013]
Court & Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Obs.	1,562	1,562	1,562	1,562

Note: The table shows 2SLS estimates of the effect of public listing on various outcomes of the females whose partner is in our main sample of analysis. A presents estimates for the full sample. Panel B presents estimates for subgroups divided by predicted risk of separation. The sample consists of male defendants aged 20 to 60 who had partners at the end of 2013 and who were first charged within a 16-week window around the court-specific cutoff. All regressions control for year and court fixed effects and age, immigrant status, high school degree, college degree, missing education. Non-employed is an indicator for the female being non-employed on Nov 1st 2013. Welfare use is an indicator for having received welfare in any year 2014-2018. Psychiatric care is an indicator for having been treated at a psychiatric health care facility in any year 2014-2018. Alcohol/drug use is an indicator for having been diagnosed for alcohol or drug abuse in 2014-2018. Separation risk is based on a prediction where the risk of separating in 2014-2018 is regressed on pre-determined covariates as explained in detail in the main text. The cutoff for high/low is based on the median of the separation risk score. */**/*** denote significance at the 10/5/1 percent level.

Table 5: Offense Specificity, Case Progression, and Victim Relationship among Men without a Partner at Baseline

Outcome or subgroup	2SLS estimate	Standard error	Sample mean	Observations
Panel A. Post-launch criminal suspicions, by offense type				
Violence or sexual offending against women	0.078***	(0.028)	0.074	4,598
Violence or sexual offending against others	0.039	(0.036)	0.127	4,598
Property crime	0.024	(0.038)	0.126	4,598
DUI or drug crime	0.057*	(0.034)	0.174	4,598
Other crime	0.048	(0.041)	0.224	4,598
Panel B. Higher case-processing threshold for violence against women				
Case brought to trial	0.045*	(0.025)	0.036	4,598
Panel C. Prior violence or sexual offending against women				
Prior recorded violence or sexual offending	0.338**	(0.166)	0.211	487
No prior recorded violence or sexual offending	0.030	(0.023)	0.048	4,111
Panel D. Victim relationship, observed from 2016 onward				
Current partner	0.041*	(0.024)	0.038	4,598
Known non-partner	0.024*	(0.014)	0.021	4,598

Notes: The sample consists of first-time male defendants aged 20–60 who had no register-observed partner at the end of 2013 and whose index cases closed within 16 weeks of the relevant court-specific cutoff. Entries are 2SLS estimates of the effect of public listing, instrumenting listing with an indicator for case closure after the cutoff. All specifications include the fixed effects, predetermined controls, and local-linear running-variable terms used in the main specification; standard errors are clustered by court. Panel A dates offenses by the date of the alleged conduct and combines physical and sexual offenses within the two violence categories. Its first row is therefore not directly comparable to the headline violence estimate. Panel B reports an unconditional person-level indicator defined over the full male sample, rather than an outcome conditional on a suspicion having been recorded. Panel C classifies prior violence using records observed before Lexbase. Victim relationship is available only from 2016 onward. Known non-partners include former partners and acquaintances. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Labor-Market, Mental-Health, and Substance-Related Outcomes among Men without a Partner at Baseline

Outcome	2SLS estimate	Standard error	Sample mean	Observations
Non-employed	0.037	(0.052)	0.485	4,598
Welfare receipt	0.011	(0.018)	—	4,598
Psychiatric care	0.052	(0.037)	0.186	4,598
Alcohol- or drug-related diagnosis	0.077**	(0.035)	0.114	4,598

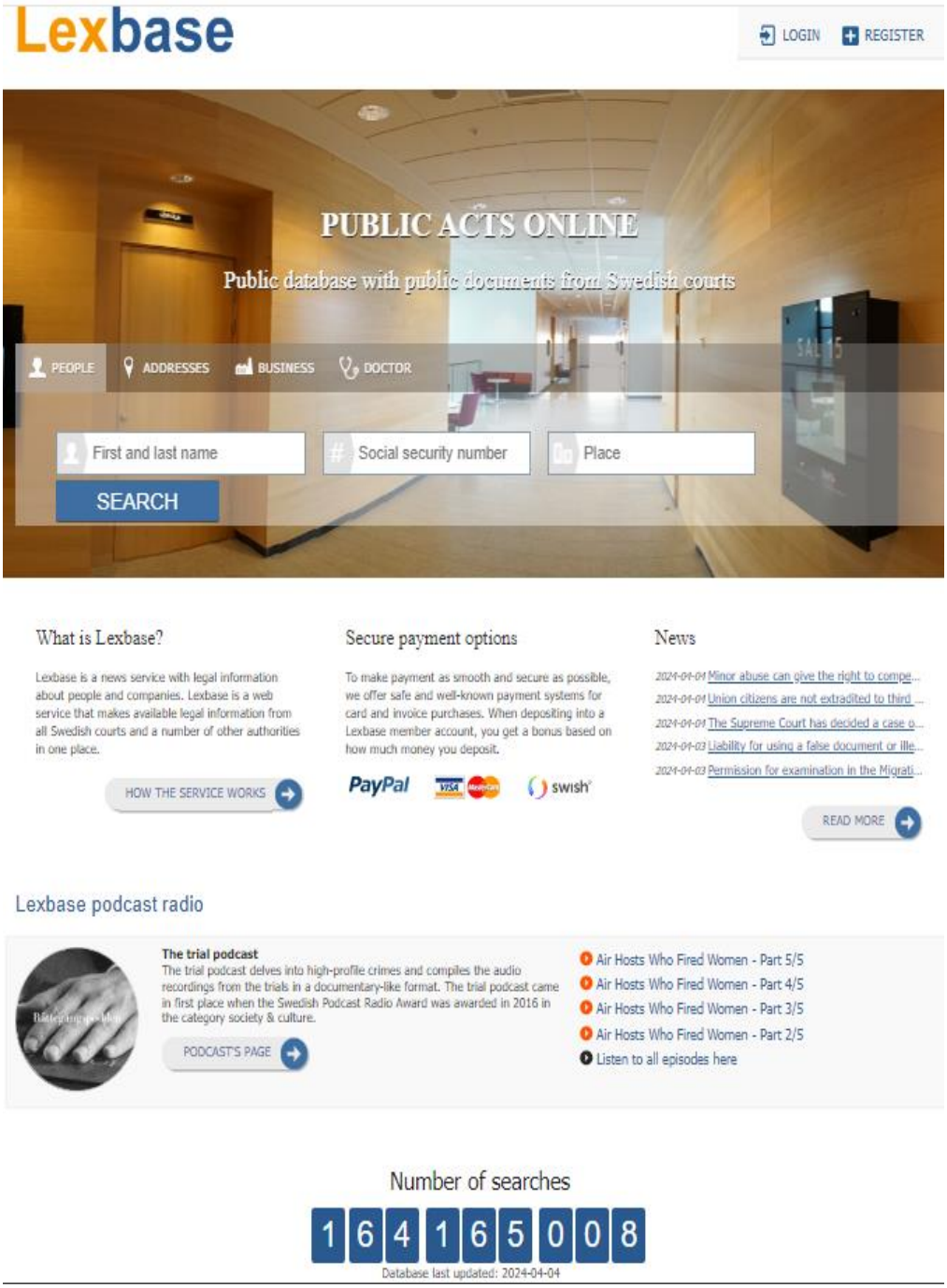
Notes: The sample and specification are the same as in Table 5. Outcomes indicate whether the man experienced the stated outcome during 2014–2018. The welfare-receipt mean is suppressed pending verification of the coding in the source data. Table 5 reports the corresponding estimate for recorded DUI/drug offending. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Timing and Composition of Observed New Matches among Men without a Partner at Baseline

Outcome	2SLS contrast	Standard error	Sample mean	Observations
Years to partnership	−0.052	(0.341)	2.870	1,087
High predicted IPV risk	0.158**	(0.070)	0.261	1,087
Age gap at least 10 years	0.114**	(0.049)	0.080	1,087
University education	−0.156*	(0.094)	0.273	1,087
Non-employed	−0.111	(0.116)	0.309	1,087
Welfare receipt	0.123*	(0.072)	0.115	1,087
Psychiatric care	0.103**	(0.044)	0.058	1,087
Alcohol- or drug-related diagnosis	0.024	(0.036)	0.017	1,087

Notes: The table reports conditional 2SLS contrasts among initially unpartnered men observed to form a register-observed partnership during the follow-up period. Listing is instrumented with an indicator for case closure after the court-specific cutoff. Because listing affects whether a partnership forms, conditioning on formation selects different populations under the two listing states. The estimates therefore describe the timing and composition of observed matches and do not identify causal effects for a fixed population or principal stratum. Partner characteristics are intended to be measured in 2013, before Lexbase; this timing, the education threshold, and the effective regression sample must be verified against the analysis code before circulation. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Online Appendix A. Figures and Tables



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

Figure A.1: Lexbase Homepage

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

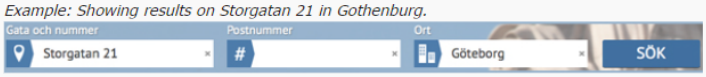


In the search fields, you can use asterisk (*) in your search text to replace part of a search term. You also don't need to search for an exact name of a person or company. Parts of the name in question are sufficient.

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

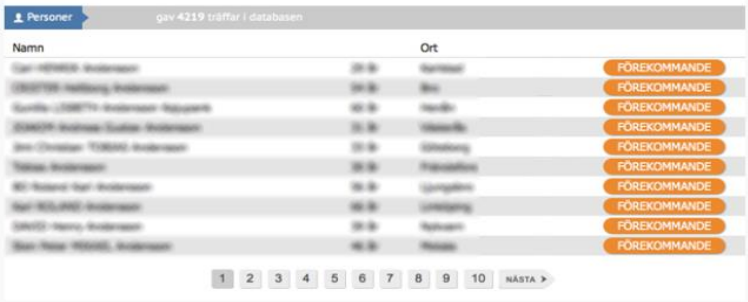
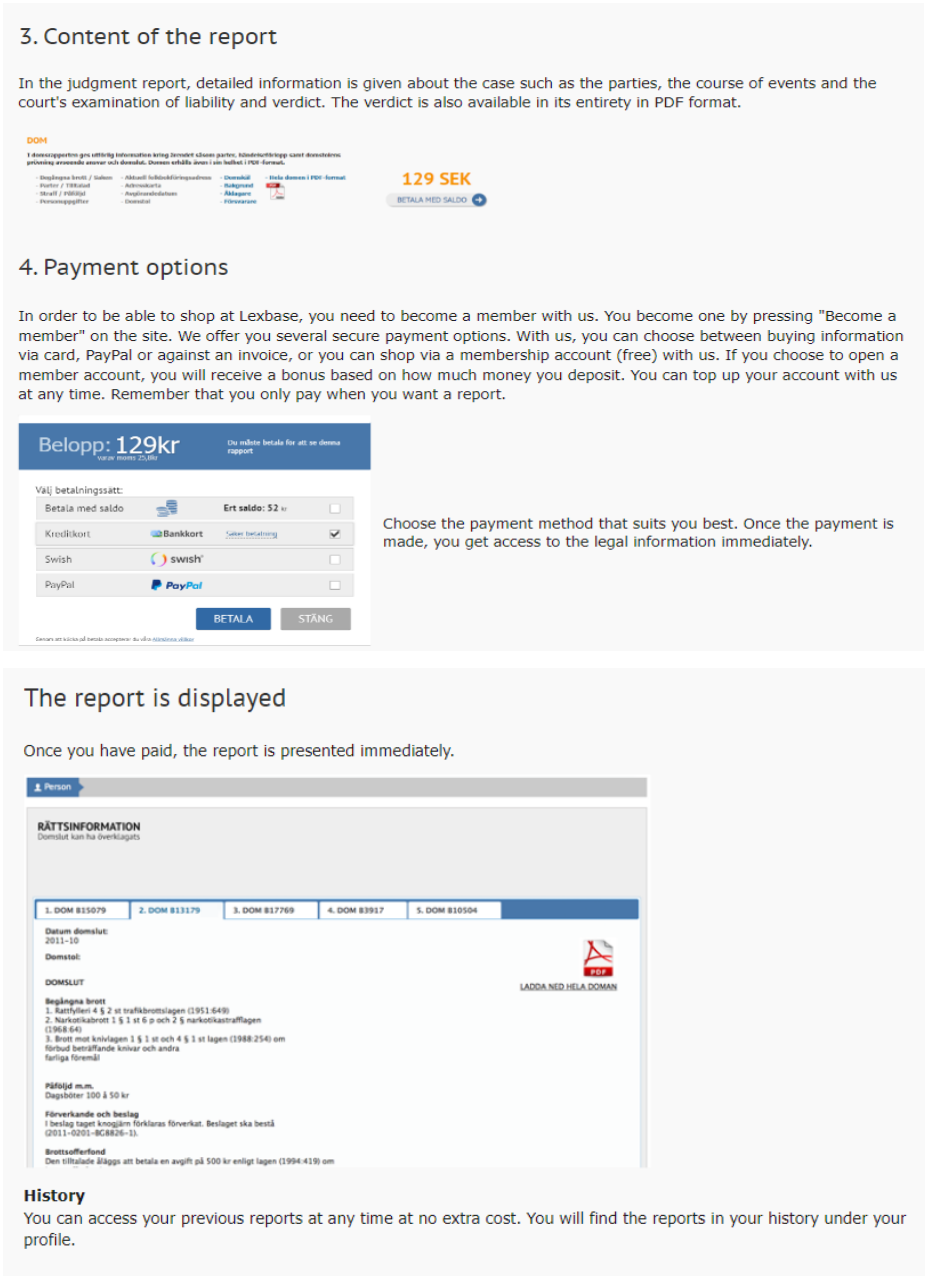


Figure A.2: How the Search Engine at Lexbase Works



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.

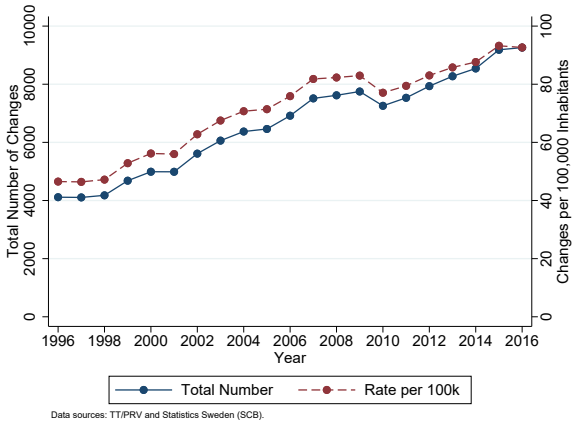


Figure A.3: Surname changes in Sweden

Notes: This figure plots the number of applications to change surname to the Swedish Intellectual Property Office (PRV). who was responsible for this statistics until July 1st 2017. The figure also plots the number of surname changes per 100,000 inhabitants. The figure uses publicly available data from PRV and SCB.

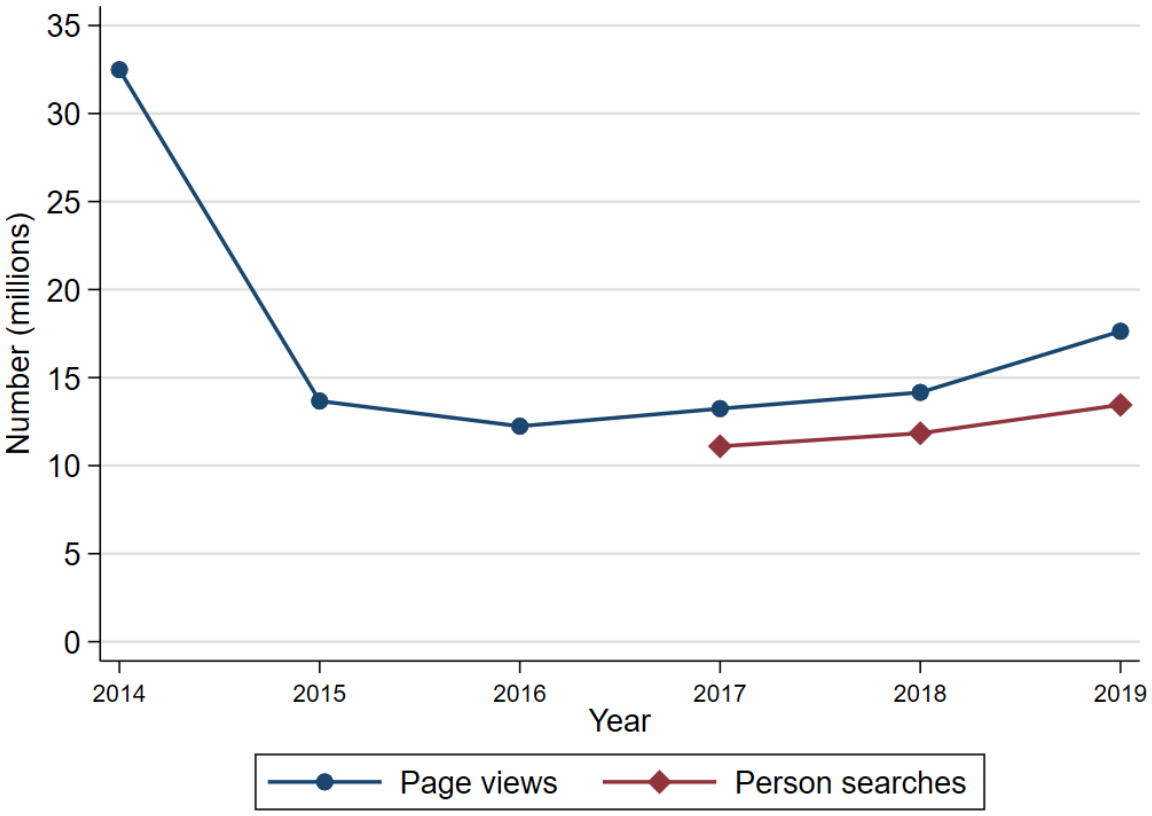


Figure A.4: Use of the Lexbase Platform. Annual Page Views and Person Searches

Notes: The figure reports annual totals from Lexbase's internal platform statistics. Page views count pages loaded on the website, whereas person searches count search queries. Neither series measures unique users, and repeated activity by the same user is included. Data on person searches are unavailable for 2014–2016, so this series begins in 2017. Both series are expressed in millions. *Source*: Lexbase.

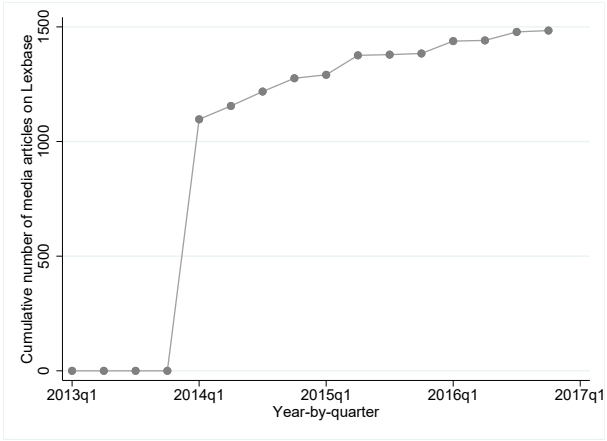
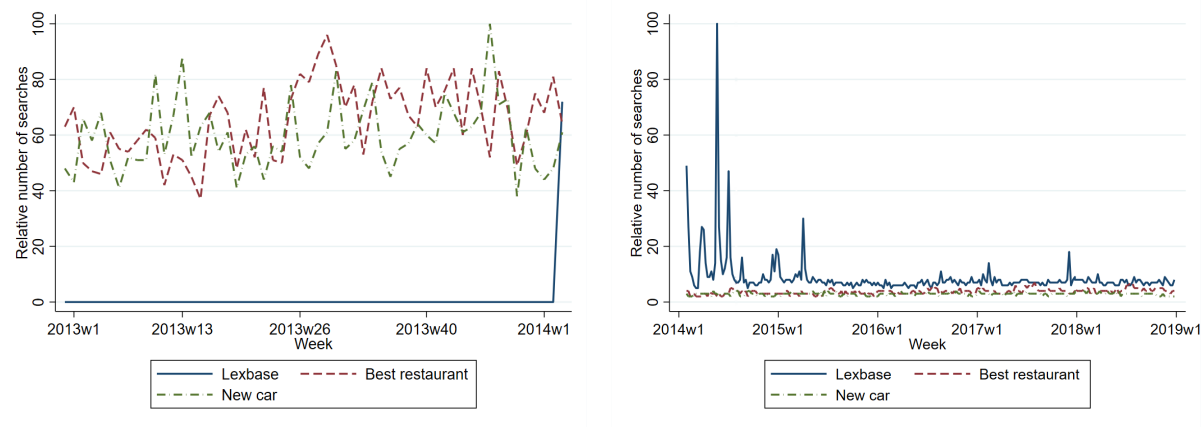


Figure A.5: Cumulative number of media articles covering Lexbase, 2013-2016

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.



(a) Before launch: 2013w1–2014w4 (b) After launch: 2014w6–2019w1

Figure A.6: Relative number of Google searches for Lexbase and comparison terms

Notes: The figure plots weekly Google Trends search intensity in Sweden for the terms "Lexbase," "Best restaurant," and "New car." Panel (a) covers 2013w1 through the launch week, 2014w1; panel (b) covers 2014w2 through 2019w1. Google Trends normalizes search intensity by dividing the number of searches for a term by the total number of searches in the same geography and week, and then scales the series so that the highest value within the requested window equals 100. Because the two panels are drawn from separate queries, each is scaled to its own maximum and levels are not comparable across panels. The launch week is the last observation in panel (a) and is excluded from panel (b), where the spike in Lexbase searches would otherwise compress the vertical axis and obscure the comparison with the other terms. Data were collected from Google Trends on January 20, 2024.

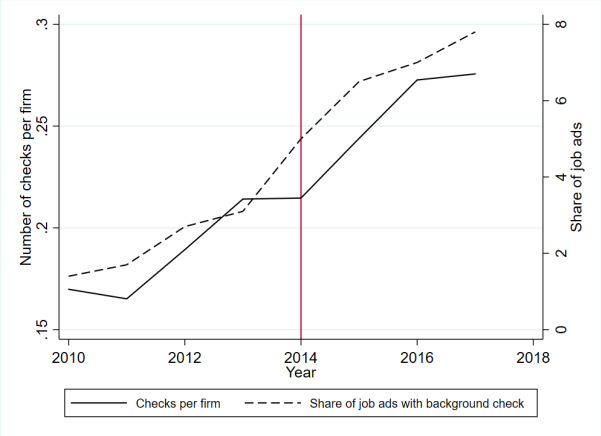
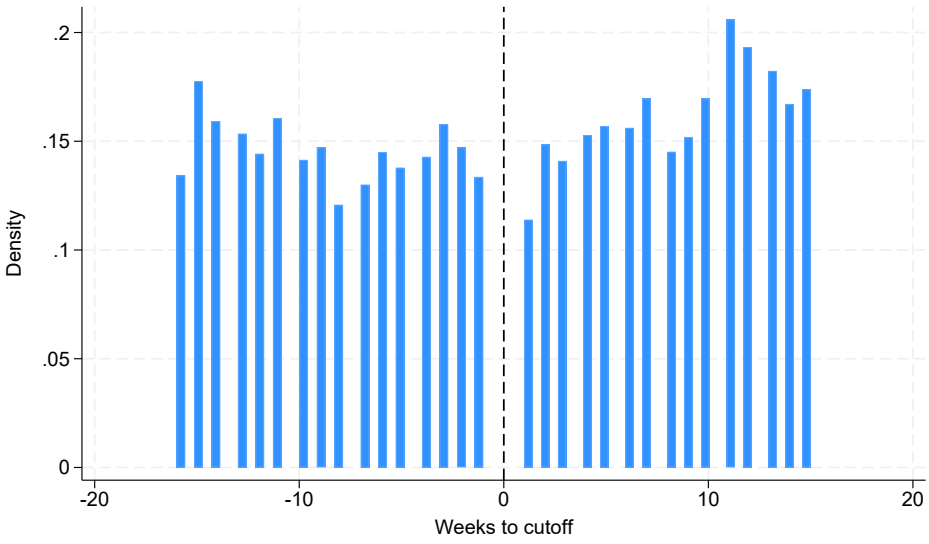


Figure A.7: Official (non-Lexbase) criminal background checks

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 A.8: Density of first-time charges around the cutoff

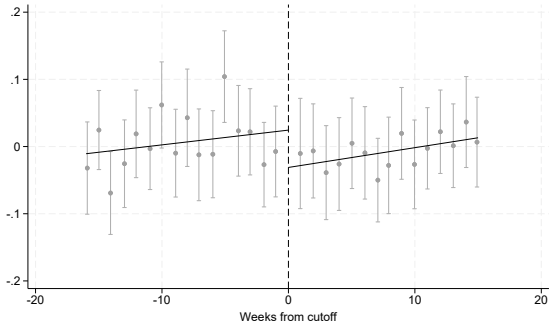


Note: The sample consists of male defendant aged 20 to 60 who were first charged within a 16-week window around the court-specific cutoff at week 0. RDD estimates show no significant change in the (log) number of cases around the cutoff (est. 0.141; se 0.207).

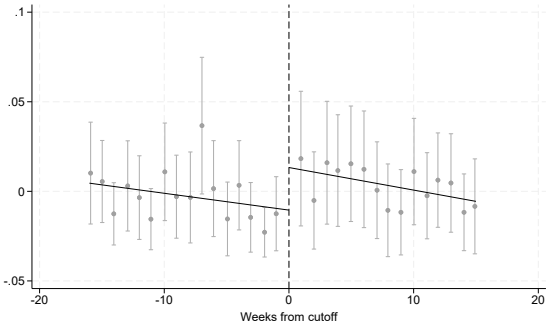
Figure A.9: Reduced form effects on main outcomes and placebo year

Panel A: Main outcomes (2014–2018)

(a) P(Having a partner)

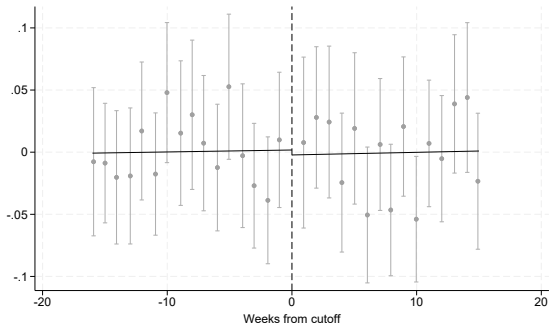


(b) P(Violence against women)

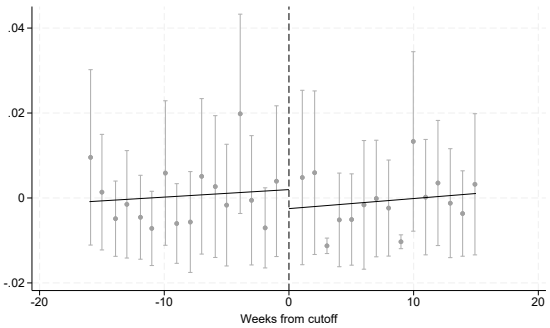


Panel B: Placebo year (2013)

(c) P(Having a partner), 2013



(d) P(Violence against women), 2013



Note: The sample consists of first-time male defendants aged 20 to 60 who were first charged within a 16-week window around the court-specific cutoff. **Panel A** shows the effect on outcomes measured during 2014–2018. **Panel B** shows placebo effects on outcomes measured in 2013, prior to the public listing. Figures plot residualized outcomes after netting out year and court fixed effects and baseline controls (age, immigrant status, high school degree, college degree, and missing education). Ninety-five percent confidence intervals are based on standard errors clustered at the court level.

Table A.1: Descriptive Statistics

	(1) Full sample	(2) Partnered end 2013	(3) Unpartnered end 2013
Panel A: Background Characteristics			
Foreign born	.	.	.
	(.)	(.)	(.)
Age in 2013	.	.	.
	(.)	(.)	(.)
Partnered in 2013	.	.	.
	(.)	(.)	(.)
Compulsory degree in 2013	.	.	.
	(.)	(.)	(.)
HS degree in 2013	.	.	.
	(.)	(.)	(.)
College degree in 2013	.	.	.
	(.)	(.)	(.)
Missing education	.	.	.
	(.)	(.)	(.)
Panel B: First charge characteristics			
Property charge	.	.	.
	(.)	(.)	(.)
Violent charge	.	.	.
	(.)	(.)	(.)
Drug charge	.	.	.
	(.)	(.)	(.)
DUI charge	.	.	.
	(.)	(.)	(.)
Traffic charge	.	.	.
	(.)	(.)	(.)
Other charge	.	.	.
	(.)	(.)	(.)
Acquitted	.	.	.
	(.)	(.)	(.)
Prison sentence	.	.	.
	(.)	(.)	(.)
Fines	.	.	.
	(.)	(.)	(.)
Probation sentence	.	.	.
	(.)	(.)	(.)
Panel C: Outcomes			
Has partner	.	.	.
	(.)	(.)	(.)
Violence against women	.	.	.
	(.)	(.)	(.)
Obs.	6,160	1,562	4,598

Note: The table shows mean values and standard deviations (in parentheses) for the full analysis sample (1), and subsamples based on partnership status at the end of 2013 (2-3). Standard deviations are reported on the row below the mean values. The sample consists of male defendants aged 20 to 60 who were first charged within a 16-week window around the court-specific cutoff.

Table A.2: First Stage Results by 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 male defendants aged 20 to 60 who who were first charged within 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 control for the other covariates as well as year and court fixed effects and weeks-to-cutoff, allowing for variying slopes above and below the cutoff. The Sanderson-Windmeijer F-statistic in the full sample is 654.9 and the Cragg-Donald F-statistic of 4569.7 The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table A.3: Balancing

	Publicly listed	First charged 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 male defendants aged 20 to 60 who were first charged within a 16-week window around the court-specific cutoff. Column (1) shows results from a regression of an indicator for being publicly listed (endogenous variable) on all baseline covariates. Column (2) shows results from a joint regression of an indicator for first being charged after the cutoff (instrument) on all baseline covariates. The standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table A.4: Effect of public listing on the probability of having a partner among females

	Mean	OLS	2SLS
Panel A: Full sample	(1)	(2)	(3)
Has partner	0.459	-0.056**	0.005
	(0.498)	(0.026)	(0.054)
Panel B: With partner at baseline			
Has partner	0.935	-0.005	0.007
	(0.246)	(0.019)	(0.056)
Panel C: Without partner at basline			
Has partner	0.240	-0.021	0.088
	(0.427)	(0.024)	(0.057)
Court & FE	–	Yes	Yes
Baseline Controls	–	Yes	Yes
Obs.		2,061	2,061
Obs. partnered		648	648
Obs. unpartnered		1,413	1,413

Note: The sample consists of female defendants aged 20 to 60 who were first charged within a 16-week window around the court-specific cutoff. The dependent variable is an indicator for whether the individual had a partner in any year 2014–2018. All regressions control for year and court fixed effects as well as for weeks-to-cutoff, allowing varying slopes above and below the cutoff. Controls include age, immigrant status, high school degree, college degree, and missing education. The first-stage coefficient in column (3) is .782 (.057). Standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent levels.

Table A.5: Robustness results

	Discrete RD	Bandwidth 20 weeks	Bandwidth 8 weeks	Placebo Year
	(1)	(2)	(3)	(4)
Has partner	-0.106***	-0.085***	-0.015	-0.018
	(0.039)	(0.032)	(0.052)	(0.031)
Violence against women	0.034**	0.027**	0.053**	-0.008
	(0.016)	(0.013)	(0.026)	(0.009)
Court & Year FE	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Obs.	6,160	7,717	2,800	6,160

Note: The baseline sample consists of male defendants aged 20 to 60 who were first charged within a 16-week window around the court-specific cutoff. All regressions control for year and court fixed effects as well as age, immigrant status, high school degree, college degree, and missing education. Row (1) controls for the running variable using year-by-month fixed effects. Row (2) increases the bandwidth to 20 weeks. Row (3) shrinks the bandwidth to 8 weeks and estimates a local randomization RD without controlling for the running variable. Row (4) uses as a placebo the dependent variable measured in the year before the launch of Lexbase. Standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent levels.

Table A.6: Information Updating: Search Intensity, Crime Type, and Relationship Length

	High Search Intensity		Low Search Intensity	
	(1)	(2)	(3)	(4)
Panel A: By Crime Severity				
	Felony	Misdemeanor	Felony	Misdemeanor
Treatment	-0.268**	0.016	0.024	0.018
	(0.128)	(0.029)	(0.258)	(0.069)
	[0.939]	[0.982]	[0.935]	[0.951]
Obs	459	394	292	224
Panel B: By Relationship Length				
	Long Rel.	Short Rel.	Long Rel.	Short Rel.
Treatment	-0.076	-0.192***	-0.000	-0.063
	(0.059)	(0.059)	(0.063)	(0.088)
	[0.975]	[0.949]	[0.966]	[0.927]
Obs	442	525	264	331
Court & Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses, p-values in brackets. *p<0.1, **p<0.05, ***p<0.01. Add your additional study-specific notes here.

Table A.7: Relationship Continuation by Municipality Population Density

Subgroup	2SLS estimate	Standard error	Sample mean	Observations
Below-median population density	-0.147^{***}	(0.049)	0.958	740
Above-median population density	-0.012	(0.038)	0.951	822

Notes: The sample consists of men who had a register-observed partner at the end of 2013. The outcome indicates remaining with that baseline partner during 2014–2018. Entries are 2SLS effects of public listing, estimated separately below and above the sample median of pre-launch municipality population density. Listing is instrumented with an indicator for case closure after the relevant court-specific cutoff. The specifications otherwise follow the main analysis, with standard errors clustered by court. Population density is an indirect and nonexclusive proxy for anonymity, network overlap, and audience reach; it also captures other features of local communities and relationship markets. A pooled test of equality and subgroup first stages were not supplied and should be added before the split is interpreted as statistically heterogeneous. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Search market composition and mobility for men without partners at baseline

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

Note: The sample consists of male defendants aged 20 to 60 without partners at baseline who were first charged within a 16-week window around the court-specific cutoff. The sample in columns (1)-(2) is conditional on having found a partner within five years after Lexbase. The dependent variable in column (1) is an indicator for the partner being from the same high school or neighborhood (at age 16). The dependent variable in column (2) is an indicator for the partner being from the same workplace or neighborhood (three years before Lexbase). The dependent variable in column (3) is an indicator for changing the place of residence. All regressions control for year and court fixed effects as well as age, immigrant status, and educational attainment. Standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table A.9: Alternative interpretations of the violence effect: Changes in reporting and types of violence

	Baseline VAW	Case charged	Case convicted	Violence against partner	Violence against non-partner
	(1)	(2)	(3)	(4)	(5)
	0.056***	0.045*	.	0.041*	0.024*
	(0.021)	(0.025)	(.)	(0.024)	(0.014)
	[0.058]	[0.036]	[.]	[0.038]	[0.021]
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 sample consists of male defendants aged 20 to 60 without partners at baseline who were first charged within a 16-week window around the court-specific cutoff. Column (1) repeats the baseline effect on any violence against women. The dependent variable in column (2) is an indicator for whether the prosecutor brought the case to trial. The dependent variable in column (3) is an indicator for whether the case resulted in a conviction. The dependent variable in column (4) is an indicator for violence against a partner, and in column (5) violence against a woman who is not a partner. All regressions control for year and court fixed effects as well as age, immigrant status, and educational attainment. Standard errors are clustered at the court level. */**/*** denote significance at the 10/5/1 percent level.

Table A.10: Violence against Women by Pre-Lexbase Substance-Related History

	Prior history	No prior history
Panel A. Prior DUI or drug suspicion		
2SLS effect on violence against women	0.110	0.036
	(0.069)	(0.026)
Panel B. Prior inpatient substance-use record		
2SLS effect on violence against women	0.029	0.064***
	(0.128)	(0.024)

Notes: Entries are 2SLS effects of public listing on post-launch recorded violence against women among men without a partner at baseline. Panel A divides the sample by a DUI or drug suspicion observed before Lexbase. Panel B divides it by an inpatient substance-use record during the ten years preceding Lexbase. These are violence-outcome splits; they do not estimate post-launch substance-related diagnoses or offending by prior history. Subgroup observations, means, first stages, and pooled interaction tests were not supplied and should be added before circulation. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.11: Empirical Patterns by Baseline Partnership Status

	Partner at baseline	No partner at baseline
Relationship outcome	Rapid decline in continuation of the baseline relationship	Gradual decline in entry into a register-observed partnership
Heterogeneity and timing	Larger point estimates in newer relationships, for serious index cases, in higher-engagement age groups, where women have greater relative resources, and in below-median-density municipalities; formal equality tests are required	Larger violence point estimate among men with prior recorded violence or sexual offending against women; formal equality test is required
Labor-market and substance-related outcomes	Lower female non-employment, with a larger point estimate in the high predicted-risk subgroup; evidence is suggestive	Increase in recorded alcohol- or drug-related diagnoses and in DUI/drug suspicions; no detectable change in measured labor-market outcomes
Observed match composition	Not applicable to the incumbent-match analysis	Among selected partnerships observed to form, partner characteristics indicate greater observable disadvantage
Recorded offending and case processing	Positive but statistically imprecise estimate for violence against women	Offending response is not broad-based; violence against women rises, survives the trial threshold, and is not confined to current partners

Notes: The table summarizes empirical patterns and does not report a mediation analysis. Baseline relationship status is not randomly assigned, and subgroup 2SLS estimates may identify different complier populations. Population density is an indirect proxy, and the corresponding split does not identify information diffusion or stigma. The data do not reveal individual searches, prior knowledge, which partner initiated separation, potential partners' acceptance decisions, men's search or withdrawal, or the separate contributions of private learning, salience, social diffusion, and audience or associative-stigma effects. Conditional match-composition estimates describe selected observed matches and are not causal effects for a fixed population.

Table A.12: Sample characteristics for men with and without partners at baseline

	With partners	Without partners
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 male defendants aged 20 to 60 who were first charge withing a 16-week window around the court-specific cutoff. Convictions and incarceration are measured from the retention cutoff to the launch of the website.

Online Appendix B. Fuzzy-RD Estimand

B.1 Control complier mean

Following Mueller-Smith and Schnepel (Mueller-Smith and Schnepel, 2021), we benchmark the 2SLS estimate against the untreated outcome mean for the same local population identified by the IV. For a binary outcome, this control complier mean is the natural denominator for expressing τ_{FRD} in relative terms. Define the local listing rates

$$p_- = \lim_{x \to 0^-} E[D_i \mid X_i = x], \qquad p_+ = \lim_{x \to 0^+} E[D_i \mid X_i = x], \tag{4}$$

and the local outcome means among unlisted defendants

$$m_- = \lim_{x \to 0^-} E[Y_i \mid D_i = 0, X_i = x], \qquad m_+ = \lim_{x \to 0^+} E[Y_i \mid D_i = 0, X_i = x]. \tag{5}$$

Under monotonicity, defendants who remain unlisted immediately above the cutoff would remain unlisted on either side. Continuity of their share and untreated outcomes implies that their weight among unlisted defendants just below the cutoff is

$$\omega = \frac{1 - p_+}{1 - p_-}. \tag{6}$$

The below-cutoff unlisted mean can therefore be written as

$$m_- = (1 - \omega)\, CCM + \omega m_+, \tag{7}$$

which gives the control complier mean

$$CCM = \frac{m_- - \omega m_+}{1 - \omega}. \tag{8}$$

The ratio τ_{FRD}/CCM expresses the 2SLS estimate relative to the estimated outcome mean that the affected defendants would have experienced without listing. With a first stage near 0.73, the unlisted population below the cutoff is already dominated by this group, so the adjustment from m_- to the CCM is typically modest. We estimate the required limits using the same local specification as in the main analysis and report conventional observed means alongside the CCM for transparency.

Online Appendix C. Woman-Level Relationship-State Exposure

This appendix describes the woman-level exercise reported in Section 5.4. It complements the male fuzzy-RD analysis by showing how realized relationship states evolve in a fixed cohort of women. It does not estimate a causal effect on women or a direct effect of women's screening decisions.

C.1 Sample and relationship states

The sample consists of women who were age 20–35 and had no register-observed partner in 2010. We follow this fixed cohort annually from 2011 through 2018. For each woman-year, the identity of the register-linked partner is used to assign one of four mutually exclusive states,

$$S_{it} \in \{0, 1, 2, 3\},$$

where

$$\begin{aligned} &0 = \text{no register-observed partner},\\ &1 = \text{partner outside the focal convicted-male sample},\\ &2 = \text{unlisted convicted partner in the focal sample},\\ &3 = \text{listed convicted partner in the focal sample}. \end{aligned}$$

The residual partner state includes all men outside the focal sample and should not be interpreted as containing only men without criminal-court histories.

The state-occupancy formulation measures total realized annual exposure rather than the initial transition into a partnership alone. It therefore incorporates formation, persistence, dissolution, re-partnering, and remaining single. A transition model restricted to women who were single in the preceding year would answer a narrower flow question and would omit later years of exposure to relationships formed earlier.

The focal states are classified by actual Lexbase listing. Unlike the male fuzzy-RD analysis, cutoff assignment does not define this comparison. Actual listing can be related to other characteristics of the men, so the exercise is descriptive rather than causal.

C.2 Stacked state-occupancy event study

We expand each woman-year into one observation for every destination $d \in \{0, 1, 2, 3\}$ and define

$$Y_{itd} = 1\{S_{it} = d\}.$$

We estimate

$$Y_{itd} = \alpha_{it} + \lambda_d + \sum_{\tau \neq 2013} \Big[\beta_\tau^1 1\{t = \tau\} 1\{d = 1\} + \beta_\tau^2 1\{t = \tau\} 1\{d = 2\} + \beta_\tau^3 1\{t = \tau\} 1\{d = 3\} \Big] + \varepsilon_{itd}. \tag{9}$$

The model includes woman-year fixed effects, α_{it}, and destination fixed effects, λ_d. Woman-year fixed effects absorb all factors common to the four destination indicators in a given year, so the coefficients describe changes in the relative allocation of total annual exposure across the mutually exclusive states. The omitted year is 2013. Standard errors are clustered at the woman level.

The event-study contrast plotted in Figure **??** is

$$\theta_\tau = \beta_\tau^3 - \beta_\tau^2, \tag{10}$$

which is the probability of being partnered with a listed convicted man minus the probability of being partnered with an unlisted convicted man in year τ, relative to the same difference in 2013. It is a contrast in current relationship-state occupancy, not a transition hazard or an estimate of search activity.

C.3 Results and interpretation

The estimated contrast is close to zero in 2011 and 2012, before Lexbase launched. It turns negative in 2014 and becomes progressively more negative through 2018, when it is approximately -9×10^{-5}, or -0.009 percentage points, relative to 2013. The absolute effect is small because partnerships with men in either focal state are rare in the full female cohort.

The post-launch pattern is consistent with a shift in realized relationship allocation away from listed relative to unlisted convicted men. It may combine changes in partnership formation, persistence, dissolution, re-partnering, and remaining single. Because actual listing rather than cutoff assignment defines the focal states, listed and unlisted men may differ in ways that also predict relationship outcomes. The flat pre-launch coefficients are reassuring about pre-existing differential trends but do not eliminate this selection concern. The figure should therefore not be interpreted as a separate causal estimate of listing, as evidence that a particular woman searched Lexbase, or as an estimate of unilateral screening by women.

Online Appendix D. A Simple Framework for Disclosure by Baseline Relationship Status

This appendix develops a deliberately simple framework for organizing the paper's central male-side findings. A public-disclosure shock can change both a partner's private assessment of a match and the social cost of remaining associated with the disclosed individual. The resulting total disclosure penalty operates on different margins depending on whether a man is already in a relationship when disclosure occurs. For a partnered man, disclosure changes the continuation value of an incumbent match. For an unpartnered man, it changes access to new matches, the composition of matches that are observed to form, and the frequency of interactions that do not become formal matches.

D.1 Public visibility and perceived relationship surplus

We treat listing as a public signal that makes an otherwise difficult-to-observe attribute salient and can thereby change participation and selection (Stigler, 1961; Akerlof, 1970; Spence, 1973; Dranove and Jin, 2010). Public visibility can also matter through the reactions of relatives, friends, co-workers, and other audiences, including reputational or associative-stigma costs (Goffman, 1963).

Let $L_i \in \{0, 1\}$ indicate whether man i is publicly listed. The underlying offense and formal sanction are taken as fixed. Listing creates a total effective disclosure penalty

$$\Delta_{ijk} = \underbrace{\rho_{jk}\kappa_i}_{\Delta_{ijk}^{\text{belief}}} + \underbrace{\sigma_{jk}\chi_i}_{\Delta_{ijk}^{\text{audience}}} \geq 0, \tag{11}$$

where $k \in \{I, N\}$ denotes an incumbent relationship or a potential new match. The first component captures direct learning, reinterpretation, or increased salience: κ_i is the adverse content of the signal and $\rho_{jk} \in [0, 1]$ governs its reach and weight in context k. The second captures audience effects: χ_i is the potential reputational or associative-stigma cost of public visibility and $\sigma_{jk} \in [0, 1]$ governs exposure to, or concern about, those audiences. Thus $\Delta_{ijk}^{\text{audience}}$ can be positive even when the partner already knows the underlying criminal history and $\Delta_{ijk}^{\text{belief}}$ is small.

This reduced-form representation accommodates several sources of heterogeneity in the data. A more serious charge can raise the adverse content of the signal or the reputational cost of association. The effective penalty can be larger in a newer relationship if the information is more novel, but it can also vary with the likelihood that wider audiences discover the record. Lower local anonymity or greater overlap in social networks could raise σ_{jk}, but population density is not a direct measure of either object. These sources of heterogeneity are not channel-identifying: the empirical design estimates the effect of listing, not the separate components of equation (11). The model is an organizing device and is distinct from the fuzzy-RD framework used to identify the causal effect of listing.

D.2 Incumbent relationships: public visibility and exit

The separation rule follows marriage-market and household-bargaining models in which relationship continuation depends on match surplus, outside options, and bargaining or threat points (Becker, 1973, 1974; Manser and Brown, 1980; McElroy and Horney, 1981; Lundberg and Pollak, 1993).

Consider a man i and his incumbent partner j. Let a_{ij} denote the woman's pre-disclosure utility from continuing the relationship, o_j her outside option, and $c_j \geq 0$ the private cost of separation. Define the pre-disclosure continuation surplus

$$m_{ij} = a_{ij} - o_j + c_j. \tag{12}$$

After disclosure, the woman remains if

$$m_{ij} \geq L_i \Delta_{ijI}. \tag{13}$$

The one-sided decision rule is expositional. The registers reveal whether the relationship continues, but not which partner initiated a separation; the same reduced-form relationship-surplus condition could incorporate a response by either partner. Let G_I denote the distribution of m_{ij} among incumbent couples. For a given effective penalty Δ_I, the separation probability for a listed man is

$$P^{\text{sep}}(1) = G_I(\Delta_I), \qquad P^{\text{sep}}(0) = G_I(0), \tag{14}$$

so that

$$P^{\text{sep}}(1) - P^{\text{sep}}(0) = G_I(\Delta_I) - G_I(0) \geq 0. \tag{15}$$

The comparative statics correspond to the partnered-male results. Separation rises when the total disclosure penalty is larger, whether because the listing changes beliefs or salience, creates greater audience costs, or both. A stronger outside option raises o_j, while a lower practical cost of exit reduces c_j; both lower m_{ij} and make separation more likely. The density of m_{ij} near the disclosure threshold determines how many couples change status, which explains why the response may be concentrated among relationships already close to separation.

The model does not mechanically predict improved labor-market outcomes for women who separate. That result requires an additional condition. Let Y_j^E and Y_{ij}^S denote a woman's economic outcome after exit and if she remains, respectively. The women induced to separate by listing satisfy

$$0 \leq m_{ij} < \Delta_{ijI}.$$

If

$$E\Big[Y_j^E - Y_{ij}^S \mid 0 \leq m_{ij} < \Delta_{ijI}\Big] > 0, \tag{16}$$

then exit raises the average measured economic outcome among women in the relationships whose status is changed by listing. Equation (16) is a condition on affected relationships, not a general implication of public disclosure and not a sufficient condition for a welfare conclusion.

D.3 New matches: exclusion and selection among observed partners

The new-match margin builds on assignment and search models of marriage in which acceptance thresholds and search frictions shape both match rates and sorting patterns (Burdett and Coles, 1997; Shimer and Smith, 2000; Choo and Siow, 2006).

Now consider a man who is unpartnered at launch. We normalize male willingness to enter a feasible match and focus on the potential partner's acceptance decision. For tractability, write Δ_i^N for the effective new-match disclosure penalty. Potential female partners are of two types, $t \in \{v, h\}$. Type v has a relatively weak outside option and is labeled "vulnerable," while type h has a stronger outside option:

$$o_h > o_v.$$

Let the population shares be π_v and π_h, with $\pi_v + \pi_h = 1$. A potential match generates baseline surplus $a_i + \varepsilon_{ij} - o_t$, where a_i is the man's common match value and $\varepsilon_{ij} \sim U[0, \bar{\varepsilon}]$ is idiosyncratic compatibility. Woman j accepts the match if

$$a_i + \varepsilon_{ij} - o_t - L_i \Delta_i^N \geq 0. \tag{17}$$

Assuming the acceptance thresholds are interior, the acceptance probability for type t is

$$m_t(L_i) = 1 - \frac{o_t - a_i + L_i \Delta_i^N}{\bar{\varepsilon}}. \tag{18}$$

The aggregate probability that a potential interaction becomes a match is

$$M_i(L_i) = \pi_v m_v(L_i) + \pi_h m_h(L_i). \tag{19}$$

Because $\pi_v + \pi_h = 1$, listing reduces match formation by

$$M_i(1) - M_i(0) = -\frac{\Delta_i^N}{\bar{\varepsilon}} < 0. \tag{20}$$

This is the model's partner-acceptance channel: a negative public signal lowers the probability that an initially unpartnered man forms a register-observed partnership. A prospective partner has no relationship-specific capital to lose and can therefore reject a possible match when even a coarse binary hit moves expected surplus below zero. By contrast, the incumbent condition in equation (13) includes the private cost of separation, so only a larger effective disclosure penalty may overturn continuation. This threshold logic can accommodate a formation response to relatively minor underlying cases and an incumbent response concentrated among more serious cases. It does not imply that users observed offense severity: the free Lexbase result did not disclose it, and the data do not show who searched or obtained the full judgment. Empirically, the decline in formation could also reflect changes in men's search effort, withdrawal, or willingness to form a registered partnership; the design does not separate the two sides' decisions.

The same threshold rule changes the composition of matches that are observed to form.

Since $o_h > o_v$, type-v women accept at a higher rate than type-h women. Let

$$s_v(L_i) = \frac{\pi_v m_v(L_i)}{M_i(L_i)} \tag{21}$$

be the share of vulnerable women among accepted matches. Define $c = L_i \Delta_i^N / \bar{\varepsilon}$, so that $m_t(L_i) = m_t(0) - c$. Then

$$\frac{\partial s_v}{\partial c} = \frac{\pi_v \pi_h \left[m_v(0) - m_h(0) \right]}{M_i(L_i)^2} > 0. \tag{22}$$

Listing therefore reduces the number of matches while increasing the share of matches involving women with weaker outside options. This is a selection result: the model does not imply that disclosure changes a woman's characteristics. It also mirrors the empirical caution that estimates conditional on partnership formation describe the composition of observed matches and are not causal effects for a fixed population of men.

The two-type formulation is intentionally stylized. More generally, the same conclusion follows whenever women with stronger outside options have a larger proportional response to the negative signal than women with weaker outside options. The latent types in this subsection should not be identified one-for-one with any single observed characteristic. They summarize differences in outside options and acceptance thresholds that may be correlated with several measured and unmeasured attributes.

The framework makes no sharp prediction for time to match conditional on a partnership being observed within the follow-up window. Listing changes who forms a partnership, so conditioning on formation compares selected groups of men under the two listing states. Search effort and the number of potential encounters may also adjust. The statistically insignificant conditional time-to-match estimate is therefore compatible with the framework, but it is not a clean test of the match-probability prediction in equation (20).

D.4 Rejection, distress, substance use, and violence

The reduced-form violence channel is motivated by evidence that violence against women can respond to bargaining conditions, stress, and emotional shocks (Aizer, 2010; Card and Dahl, 2011; Anderberg et al., 2016, 2023).

The decline in formal partnership formation need not imply fewer interactions with women. Let $b \in \{P, U\}$ denote whether the man was partnered or unpartnered at launch, and let n_b be the number of potential relationship interactions or approaches during the follow-up period. Initially unpartnered men are immediately active on the new-match margin, so assume

$$n_U > n_P. \tag{23}$$

The expected number of interactions that do not become formal matches is

$$R_i(L_i, b) = n_b \left[1 - M_i(L_i) \right]. \tag{24}$$

Combining equations (20) and (24), and holding the number of potential interactions

fixed, listing raises expected non-matching interactions by

$$R_i(1,b) - R_i(0,b) = \frac{n_b \Delta_i^N}{\bar{\varepsilon}}. \tag{25}$$

The same disclosure signal therefore creates a larger cumulative set of non-matching interactions for initially unpartnered men when $n_U > n_P$. Some may involve explicit rejection, but the data do not observe the interaction or the reason it failed to become a registered partnership.

Let distress or substance use be

$$H_i(L_i,b) = h_i + \gamma R_i(L_i,b) + \omega_i L_i, \qquad \gamma, \omega_i \geq 0, \tag{26}$$

where $\omega_i L_i$ allows public labeling, marginalization, or audience exposure to affect distress directly, even without an observed rejection. Let violence risk satisfy

$$\Pr(V_i = 1 \mid L_i, b) = \mathcal{G}(\nu_i + \phi H_i(L_i,b) + \psi z_i(L_i)), \qquad \phi, \psi \geq 0, \tag{27}$$

where $\mathcal{G}(\cdot)$ is increasing, ν_i is baseline violent propensity, and $z_i(L_i)$ captures the vulnerability or conflict intensity of the contacts and matches that remain. Distress arising from non-matching interactions or public labeling can raise violence when $\phi > 0$; selection toward more vulnerable or conflict-prone interactions can provide an additional channel when $\psi > 0$.

Equations (25)–(27) show why lower partnership formation and higher violence are internally consistent. Rejected approaches, dates, former partners, acquaintances, and other known women remain possible contacts even when they do not become register-observed partners. The framework permits a larger behavioral response among men who are unpartnered at disclosure because they face the new-match and non-matching-interaction margin immediately. Baseline differences in h_i, ν_i, γ, or ω_i can further amplify this heterogeneity, but are not required for the status-based mechanism.

For men who are partnered at launch, the net violence effect is ambiguous. Disclosure may create distress, but listing-induced separation can also reduce continued contact and conflict within the incumbent relationship. The absence of a statistically detectable violence increase among partnered men is therefore consistent with the framework, while the non-matching-interaction channel permits a positive response among men who are already active on the new-match margin.

Equation (27) is deliberately reduced-form. The empirical analysis does not identify rejection, distress, substance use, or partner vulnerability as complete causal mediators of violence. The equation only demonstrates that the observed combination of fewer formal matches, more recorded substance-related diagnoses, different composition among observed matches, and more recorded violence can arise from a common disclosure shock.

D.5 Comparative statics and empirical counterparts

The framework yields the following comparative statics:

Model implication	Empirical counterpart
The total disclosure penalty raises separation in incumbent relationships near the continuation threshold.	Partnership dissolution rises quickly among men who were partnered at launch.
The separation response is larger when public visibility has a larger private-belief, salience, or audience effect and when the partner has a stronger outside option or a lower exit cost.	Separation point estimates are larger in newer relationships, for more serious charges, in higher-engagement age groups, and when the woman has relatively more education or earnings; these splits do not identify the underlying disclosure channel.
Listing lowers the probability that a potential interaction becomes a new match.	Partnership formation falls among men who were unpartnered at launch.
A higher acceptance threshold selects the observed matches toward women with weaker outside options.	Among partnerships that are observed to form, the partners observed for listed men have characteristics associated with greater observable disadvantage.
Non-matching interactions, direct public-labeling costs, and selected remaining contacts can raise distress, substance use, and violence even as formal matches decline.	Alcohol- or drug-related diagnoses and violence against women rise among initially unpartnered listed men; the violence increase extends to women outside current partnerships.

The framework organizes the results without asserting that one mechanism fully explains every estimate. Its main contribution is to show how baseline relationship status indexes different feasible margins of adjustment. For partnered men, the immediate observed margin is continuation and exit. For unpartnered men, it is access to matches and selection among the matches that remain, alongside non-matching or otherwise conflictual interactions. The empirical design identifies the total effect of public listing and does not decompose direct information, salience, stigma, men's responses, or potential partners' responses.

Online Appendix E. Definitions of Variables

Table A.13: Definitions of variables

Variable	Definition
Design and treatment variables	
Publicly listed	Indicator equal to one if the defendant appears in Lexbase on the launch date, and zero otherwise.
Case closed after the cutoff	Indicator equal to one if the defendant's index case closed within the 16-week window after the relevant court-specific cutoff, and zero if it closed within the 16-week window before the cutoff.
Main outcomes	
Has partner	Indicator equal to one if the individual had a partner at year-end in any year during 2014–2018. Partnership status follows the Statistics Sweden definition of being married or cohabiting with shared children.
Violence against women	Indicator equal to one if the defendant was registered as reasonably suspected of physical or sexual violence against an adult woman in any year during 2014–2018.
Baseline demographic and socioeconomic variables	
Immigrant (foreign born)	Indicator equal to one if the defendant was born outside Sweden. The terms "immigrant" and "foreign born" refer to the same variable.
Compulsory degree	Indicator equal to one if the defendant's highest completed education in 2013 was compulsory schooling.
HS degree	Indicator equal to one if the defendant had completed high school by 2013.
College degree	Indicator equal to one if the defendant had completed college or university by 2013.
Missing education	Indicator equal to one if education is missing.
Employment	Indicator for whether the defendant is employed on Nov 1st in a given year.
Earnings	Annual earnings. Nominal values are deflated to 2015 and represented in U.S. dollars using the exchange rate SEK/$ = 9.25.
Initial charge and case characteristics	
Property charge	Indicator equal to one if the defendant's first charge is classified as a property offense according to the Swedish penal code BRB.
Violent charge	Indicator equal to one if the defendant's first charge is classified as a violent offense according to the Swedish penal code BRB.
Sexual charge	Indicator equal to one if the defendant's first charge is classified as a sexual offense.
Drug charge	Indicator equal to one if the defendant's first charge is classified as a drug offense according to the Swedish penal code BRB.
DUI charge	Indicator equal to one if the defendant's first charge is classified as a driving-under-the-influence offense according to the Swedish penal code BRB.
Traffic charge	Indicator equal to one if the defendant's first charge is classified as a traffic offense according to the Swedish penal code BRB.
Other charge	Indicator equal to one if the defendant's first charge falls outside the property, violent, drug, DUI, and traffic categories.
Acquitted	Indicator equal to one if the first charge resulted in acquittal.
Prison sentence	Indicator equal to one if the disposition of the first charge included a prison sentence.

Continued on next page

Table A.13 continued

Variable	Definition
Fines	Indicator equal to one if the disposition of the first charge included fines.
Probation	Indicator equal to one if the disposition of the first charge included probation.
Felony	Indicator equal to one if the first charge belongs to a crime type for which at least one first-time defendant in the population receives a prison sentence.
Misdemeanor	Indicator equal to one if the first charge belongs to a crime type for which no first-time defendant in the population receives a prison sentence.
Additional outcomes and groupings	
Short (Long) relationship	Indicator equal to one if relationship length is at or below (above) the sample median of 6 years.
High (Low) search intensity	Indicator equal to one if the woman belongs to an age group accounting for at least 18 percent (less than 18 percent) of searches among registered users on the platform.
Below- (Above-) median population density	Indicator equal to one if the defendant lived before Lexbase in a municipality with population density below (at or above) the sample median.
Partner has higher education	Indicator equal to one if the female partner has more years of schooling (imputed from highest completed level of education) than the male partner.
Partner has same or lower education	Indicator equal to one if the female partner has the same or fewer years of schooling than the male partner.
Partner's earnings share $>$ median	Indicator equal to one if the female partner's earnings share exceeds the sample median. Earnings share is defined as the partner's earnings divided by household earnings and is measured in 2013.
Partner's earnings share $\leq$ median	Indicator equal to one if the female partner's earnings share is at or below the sample median. Earnings share is defined as the partner's earnings divided by household earnings and is measured in 2013.
High (Low) predicted risk of separation	Indicator equal to one if the couple's predicted separation risk is above (at most) the sample median. The prediction is based on a model in which separation during 2014–2018 is regressed on predetermined covariates.
Non-employed	Indicator equal to one if the relevant individual (the male defendant or the female partner, depending on the analysis) is non-employed on November 1 in any year during 2014–2018.
Welfare use	Indicator for receiving welfare in any year during 2014–2018.
Psychotropic medication	Indicator equal to one if the individual filled at least one prescription for a psychotropic medication at an outpatient pharmacy in any year during 2014–2018. The measure is constructed from the National Prescribed Drug Register.
Substance-use medication	Indicator equal to one if the individual filled at least one prescription for medication used in the treatment of alcohol- or drug-related disorders at an outpatient pharmacy in any year during 2014–2018. The measure is constructed from the National Prescribed Drug Register.
Psychiatric care	Indicator equal to one if the individual received inpatient or specialized outpatient psychiatric care in any year during 2014–2018. The measure is constructed from the National Patient Register.
Alcohol- or drug-related diagnosis	Indicator equal to one if the individual received an alcohol- or drug-related diagnosis in inpatient or specialized outpatient care in any year during 2014–2018. The measure is constructed from the National Patient Register.

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Table A.13 continued

Variable	Definition
Brought to trial	Unconditional person-level indicator equal to one if at least one post-launch violence-against-women case was brought to trial after investigation and prosecutorial review, and zero otherwise.
Post-launch DUI or drug suspicion	Indicator equal to one if the defendant was registered at the reasonable-suspicion threshold for a DUI or drug offense during 2014–2018. This is not a conviction measure.
Prior DUI or drug suspicion	Indicator equal to one if a DUI or drug suspicion was recorded before the launch of Lexbase.
Prior inpatient substance-use record	Indicator equal to one if the individual had an inpatient substance-use record during the ten years preceding the launch of Lexbase.
Time to find partner	Time to partnership formation, measured in years.
High risk of IPV	Indicator equal to one if the new partner belongs to the top quartile of the predicted IPV-risk distribution. The prediction uses predetermined covariates.
Age-gap $\geq$ 10 years	Indicator equal to one if the age difference between the male and his new partner is at least 10 years.
High education	Indicator equal to one if the individual has completed at least high school education.
Woman-level listing relationship state	Mutually exclusive annual state for the fixed female cohort: no register-observed partner; partner outside the focal convicted-male sample; unlisted convicted partner in the sample; or listed convicted partner in the sample.
Partner from childhood social network	Indicator equal to one if the partner attended the same high school or lived in the same neighborhood at age 16.
Partner from adulthood social network	Indicator equal to one if the partner worked at the same workplace or lived in the same neighborhood three years before the launch of Lexbase.
Change place of residence	Indicator equal to one if the individual changed place of residence.
Victim was a current partner	Indicator equal to one if the female victim was a current partner of the defendant.
Victim was known but not a current partner	Indicator equal to one if the female victim was known to the defendant but was not his current partner, for example a former partner, friend, co-worker, or classmate.

Online Appendix F. Construction of Court-Specific Retention Cutoffs

This appendix provides additional detail on the construction of the court-specific cutoffs used in the regression-discontinuity design and on the institutional mechanism linking cutoff position to public listing on Lexbase. The procedure uses only court and listing information; no outcome variable is used to select a cutoff.

F.1 Institutional source of the discontinuity

Swedish courts managed criminal cases in the digital administrative system *Vera*. Under Decree (2001:639), information about a criminal case was to be removed from the searchable register no later than five years after the year in which the case was decided. The underlying court documents remained public and were retained in court archives, but once the searchable metadata had been removed, locating a particular older case generally required prior knowledge of the relevant court and case number or a manual search of archival records. The retention rule therefore affected the practical retrievability of a case rather than its formal status as a public document.

Lexbase assembled its launch database by submitting large requests for court records. Because the requests were made and processed at different times and courts did not implement deletion from the searchable register in exactly the same way, the effective boundary between readily retrievable and older cases varied across courts. For a given court, cases closed on the recent side of this boundary were substantially more likely to be located and supplied to Lexbase, whereas cases closed just before it were substantially less likely to be included. Crossing the boundary did not alter the underlying offense, disposition, or sanction; it changed the probability that the case was practically available when Lexbase assembled the records displayed at launch.

F.2 Estimating the cutoff for each court

We combine the Lexbase launch records with nationwide criminal-case data to construct, for each court j and case-closure week w, the share of district- court cases that appeared in Lexbase at launch. Let

$$p_{jw} = \frac{\text{number of cases closed in court } j \text{ in week } w \text{ that appear in Lexbase}}{\text{number of cases closed in court } j \text{ in week } w \text{ in the administrative data}}. \tag{28}$$

For each candidate week w, we compare the mean inclusion share in the following 16 weeks with the mean inclusion share in the preceding 16 weeks:

$$\Delta_{jw} = \frac{1}{16}\sum_{h=0}^{15} p_{j,w+h} - \frac{1}{16}\sum_{h=1}^{16} p_{j,w-h}. \tag{29}$$

The estimated cutoff for court j is the week at which this increase is largest within the admissible search interval,

$$\widehat{c}_j = \arg\max_w \Delta_{jw}. \tag{30}$$

Averaging over 16 weeks on each side reduces the influence of idiosyncratic weekly variation and targets the persistent change in inclusion generated by the effective retention boundary.

The main analysis uses the 32 courts for which the procedure identifies a clear cutoff before 2010. Restricting the cutoffs to this period helps ensure that the relevant court records had been incorporated by the public launch of Lexbase. We exclude four courts—Umeå, Gällivare, Haparanda, and Värmland—for which the inclusion series does not exhibit a sufficiently clear break. These courts account for approximately 7.5 percent of cases in the underlying court-case data.

F.3 Assignment, running variable, and fuzziness

For defendant i with a case in court j, the running variable is the number of weeks between the case-closure date and the estimated court cutoff,

$$X_{ij} = \text{closure week}_{ij} - \widehat{c}_j, \tag{31}$$

and the assignment indicator is $Z_{ij} = 1\{X_{ij} \geq 0\}$. The main estimation sample contains cases within 16 weeks on either side of the cutoff. The retention mechanism predicts a discontinuous increase in actual listing at $X_{ij} = 0$, while case characteristics and potential outcomes should otherwise evolve smoothly over this narrow interval.

The design is fuzzy rather than sharp. Some cases closed before the estimated cutoff appear in Lexbase because, for example, the defendant accumulated a newer case before launch or a court supplied records extending further back than the nominal retention period. Conversely, some cases closed after the cutoff are absent because of incomplete record recovery, linkage error, or imprecision in the estimated effective boundary. The first stage therefore uses cutoff assignment as an instrument for realized listing rather than equating eligibility with treatment.

F.4 Interpretation

The cutoff-construction procedure is based exclusively on the court-specific relationship between case timing and Lexbase inclusion. It does not use partnership, health, labor-market, or violence outcomes and therefore cannot select thresholds to maximize discontinuities in the outcomes studied in the paper. The identifying comparison is between defendants whose cases were closed only a few weeks apart in the same court but whose records differed sharply in their probability of being retrievable when Lexbase assembled its launch database. Under the continuity and exclusion assumptions discussed in Section 4, the resulting fuzzy-RD estimand identifies the effect of actual public listing for defendants whose visibility was changed by the retention boundary.