Screening, Exclusion and Violence in the Partner Market*

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Abstract

We study how making criminal-court records publicly searchable affects intimate relationships and violence against women. Our setting is the unexpected 2014 launch of Lexbase, a Swedish platform that made court involvement immediately and anonymously searchable. We link platform records to population-wide administrative data and exploit court-specific retention cutoffs that generated a 73-percentage-point discontinuity in actual listing. Among men who were partnered at launch, listing reduces continuation of the baseline relationship by 7.8 percentage points. Among men who were unpartnered, listing reduces partnership formation by 8.7 percentage points and increases recorded suspicion of violence against women by 6.1 percentage points. The offending response is not broad-based: we find no statistically detectable increase in violence against others, property crime, or other crime, although DUI or drug offending also increases at the 10-percent level. Listing increases alcohol- or drug-related diagnoses, and the partnerships observed to form involve women with characteristics associated with greater observable disadvantage. The estimate remains positive for cases brought to trial, and the recorded violence extends beyond current partners. The effect is muted for males that underwent treatment for mental health problems at baseline. A complementary event study of a fixed female cohort shows a post-launch decline in realized exposure to listed relative to unlisted convicted men. The same public-disclosure shock can therefore facilitate screening and exit on the incumbent-relationship margin while reducing access to future matches and coinciding with adverse selection and harmful recorded outcomes on the new-match margin.

Keywords: *Criminal records*; *Family formation*; *Violence against women*; *Mental health*; *Stigma*;
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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 straightforward: if information about past criminal conduct is difficult to obtain, making it accessible may allow women to avoid dangerous partners or leave risky relationships. 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. Yet public searchability changes more than a potential partner's private information. It can expose a criminal history to incumbent partners, relatives, friends, co-workers, and other members of the local community. It may therefore precipitate separation, restrict access to future relationships, or affect 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 searches during its first year. According to company data, 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.

The information conveyed by a typical Lexbase search was both salient and incomplete. A free search revealed that an individual had appeared as a defendant in a criminal court case, but generally did not disclose the offense, disposition, or surrounding circumstances. Only 0.16 percent of searches during the first year resulted in purchase of the underlying court documents. For most users, listing therefore provided a coarse binary signal of criminal-court 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. Moreover, even if an incumbent partner already knew about the case, public searchability made it easier for her family, friends, or co-workers to discover it, potentially imposing audience or associative-stigma costs (Goffman, 1963). We consequently refer to Lexbase as a public-searchability or public-visibility shock. Our design identifies the combined causal effect of learning, salience, social diffusion, and potential stigma; it does not separately

identify these 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-court involvement becomes readily searchable while holding fixed the underlying case and its legal consequences.

The main results depend sharply on whether a relationship existed when Lexbase launched. Among men who were partnered 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 unpartnered at launch, listing reduces the probability of forming a register-observed partnership during 2014–2018 by 8.7 percentage points. This deficit develops gradually over the follow-up period. For the same initially unpartnered men, listing increases the probability of a recorded suspicion of physical or sexual violence against an adult woman by 6.1 percentage points. 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 point estimates are larger when the relationship formed after the index trial, when the relationship is relatively recent, and when the underlying case involved a more serious charge. They are also larger when the female partner has greater earnings or education relative to the man, consistent with the practical ability to leave mattering when relationship surplus falls. None of these splits cleanly identifies a mechanism. Relationship duration captures both the opportunity to know about the case and relationship-specific capital, while relative resources may capture outside options, bargaining position, or other differences across couples.

An exploratory split by municipality population density provides suggestive evidence that the public reach of listing may also matter. The estimated decline in remaining with the baseline partner is 14.7 percentage points in municipalities below median population density and 1.2 percentage points in municipalities above the median. Smaller and less

anonymous communities may permit information to diffuse more readily through social networks or impose larger audience costs when a record becomes searchable. Population density also proxies for many other features, including prior knowledge, mobility, social-network structure, and local relationship-market conditions. In the absence of a formal cross-group equality test and subgroup-specific first-stage evidence, we treat this pattern as consistent with, but not a test of, a social-diffusion or stigma channel.

We next investigate why recorded violence against women rises among men who were unpartnered at launch even though fewer register-observed partnerships form. The increase is not part of a broad-based rise in criminal offending. Using a common crime-date definition that pools physical and sexual offenses within each victim-sex category, listing increases violence or sexual offending against women by 7.8 percentage points. This estimate is not directly comparable to the 6.1-percentage-point headline estimate because the outcome construction differs. The corresponding estimates for violence or sexual offending against others, property crime, and other crime are smaller and statistically imprecise. DUI or drug offending increases by 5.7 percentage points and is statistically significant at the 10-percent level. Thus, we do not find a general increase across major offense categories, although the DUI or drug result cautions against describing the response as confined exclusively to violence against women.

Changes in reporting could contribute to the estimated violence effect because the primary outcome records reasonable suspicions rather than latent violence. We therefore examine cases that cross a higher processing threshold. Listing increases the probability that a violence-against-women case is brought to trial by 4.5 percentage points. Requiring a case to proceed to trial reduces the scope for an explanation based solely on marginal reports that fail subsequent prosecutorial screening. It does not rule out reporting changes that generate sufficiently substantiated cases, nor does it exclude changes in investigation or case processing. We therefore interpret this result as evidence against a pure marginal-reporting account, not as proof that reporting behavior is unchanged.

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. This pattern is consistent with listing amplifying risk among men with a history of violence, although the subgroup estimate is imprecise and a formal equality test is not available. At the same time, the violence response is not systematically confined to men with documented pre-launch substance-related problems. 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. The increase in DUI or drug offending provides corroborating criminal-justice-register evi-

dence that the substance-related response is not confined to health-care contact. These estimates document post-launch changes in recorded diagnoses and offending, but do not establish distress or substance use as causal mediators of violence.

Third, listing changes which partnerships are observed to form. Conditional on forming a partnership—an outcome that listing itself affects—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. These estimates describe a selected set of realized matches and should not be interpreted as causal effects on a fixed population of women. 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. This pattern helps reconcile fewer formal partnerships with more violence: the administrative partnership measure does not capture dating relationships, rejected approaches, former relationships, or other interactions with known women.

We complement the male-side analysis with a descriptive event study of realized relationship exposure in a fixed female cohort. We follow women who were single and age 20–35 in 2010 and classify each annual state as having no register-observed partner, having a partner outside the focal convicted-male sample, having an unlisted convicted partner in that sample, or having a listed convicted partner in that sample. 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 -0.009 percentage points. The absolute magnitude is small because both focal partnership states are rare in the full female cohort. Because this exercise classifies men by actual listing rather than cutoff assignment, it is descriptive: it combines formation, persistence, dissolution, re-partnering, and remaining single, and does not isolate women's searches or unilateral screening decisions. It nevertheless shows that the male-side contraction is accompanied by a corresponding post-launch shift in realized relationship allocation from women's side.

Taken together, the results show that making criminal-court involvement publicly searchable changes both existing relationships and subsequent outcomes among men who begin without a partner. For incumbent couples, listing is followed by rapid separation, consistent with women or their networks acting on the newly searchable signal, with increased salience, or with the social cost of continued association. For initially unpartnered men, listing reduces entry into observed partnerships and is accompanied by substance-related diagnoses, selection among the relationships that remain, and more recorded violence against women. Baseline relationship status is not randomly assigned, and the subgroup

estimates need not describe identical complier populations. We therefore do not interpret their difference as a causal effect of being partnered. Rather, baseline status identifies distinct margins available when the visibility shock occurs: continuation of an existing relationship versus access to new relationships and contacts.

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, 2018; 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. Information that is nominally public may have limited practical reach until it becomes immediate, anonymous, and searchable.

Second, the paper contributes to the economics of violence against women. Prior work emphasizes household resources, bargaining power, labor-market conditions, emotional shocks, gender norms, and institutional responses (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). We identify a different shock: public visibility of a man's past criminal-court involvement. The finding that listing facilitates separation in some incumbent relationships but increases recorded violence among initially unpartnered men 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.

Third, the paper contributes 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). 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 rela-

tionship differs from making a coarse criminal-court 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 estimates do not sign aggregate welfare or identify the optimal disclosure regime. They 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 Institutional setting

[1] Criminal-history checks are commonly understood as a screening technology in settings where organizations choose among applicants under safety, trust, and liability concerns. Employers, landlords, educational institutions, licensing bodies, and other organizations use these checks to identify past conduct that may be viewed as relevant to the safety of workers, tenants, students, clients, or the broader public. Screening may also serve a legal function by documenting due diligence and reducing exposure to negligent-hiring or tenant-screening liability.[2] Historically, criminal-history information was supplied mainly by public agencies. Sweden was no exception: access to criminal records was traditionally governed by relatively stringent rules and confined largely to authorized institutional uses, including employer screening in settings where records could be requested from public authorities (Backman, 2012). Increasingly, however, it is assembled and distributed by private background-check firms, especially in the United States.

The resulting reports often provide a highly salient but incomplete signal. A typical report lists the individual's name, the reporting jurisdiction, the date on which the record was created, and a case number or law-enforcement identifier (NCLC, 2019). Some reports include a short offense label, such as "possession of marijuana," but they often omit information about the underlying facts, severity, or surrounding circumstances. Coverage also varies across sources: some public repositories report only convictions, whereas

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

[2] About 94 percent of employers in the United States conduct some form of criminal history check, and about 90 percent of landlords run background checks on prospective tenants (NCLC, 2019). Earlier survey evidence from the United States showed that roughly 50 percent of employers checked the criminal backgrounds of job applicants in 2004 (Holzer et al., 2006).

private firms may include arrests and other non-conviction records, including cases that were later dismissed (Bushway and Sweeten, 2007).

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 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.

2.1 The Lexbase website

Beginning in 2013, the operators of Lexbase assembled a nationwide database of recent Swedish court cases by requesting case information and judgments from individual courts. 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. A separate government decree regulated the courts' automated registers and required information in criminal cases to be removed from the searchable register no later than five years after the year in which the case was decided.[6] This retention rule made recent cases substantially easier for Lexbase to identify and retrieve than otherwise comparable older cases.

Lexbase publicly launched on January 27, 2014. The platform sharply changed access

[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.

[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]See Decree (2001:639). The right to request public court documents followed from the Swedish constitutional principle of public access.

to criminal-history information in Sweden. Before 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. These frictions became especially severe once information about an older case had been removed from the searchable portion of the court's administrative register.[7]

Lexbase replaced this decentralized process with a person-searchable online service. Users could enter a name and, without contacting a court or notifying the person searched, immediately learn whether the individual appeared as a defendant in a criminal case covered by the database. Search results displayed identifying information such as age and place of residence, generally allowing users to distinguish between individuals with the same or similar names.[8] 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.

At launch, Lexbase sought to cover criminal judgments from courts throughout Sweden during the preceding five years. This distinguished it from conventional criminal-record systems that restricted access to authorized institutions or particular uses. A free search revealed whether the person appeared in the database but provided little information about the underlying case. Registered users could pay a small fee—approximately USD 5 at the time—to obtain the relevant judgment or other court documents, which described 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 searches during the first year resulted in a purchase. 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

[7]The underlying court documents remained public and were preserved in court archives. In practice, locating a specific older case could require visiting the relevant court and manually identifying the case number in chronological docket books before requesting the underlying documents.

[8]Users could also search by personal identity number, the unique identifier assigned to Swedish residents at birth or immigration.

involvement, but provided only limited contextual information to most searchers.

The launch thus represented a change in information accessibility rather than in the legal availability of the underlying records. It eliminated the need to identify the relevant court, communicate with public officials, and wait for court personnel to locate and transmit the documents. It also allowed searches to be conducted privately and at very low marginal cost. Information that had previously been public in a formal sense but practically obscure became an immediately searchable signal available to the general population.

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 or collaborate and claimed that easier access to court records could have a crime-prevention benefit.[9] 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 a way to protect herself and her children from dangerous men.[10] 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.[11] Together, these sources indicate that Lexbase reduced information frictions not only in formal institutional screening, but also in decentralized partner markets.

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 a short interruption before relaunching in April 2014 without the original map function.[12]

Use of the platform was extensive. Company logs record 32 million searches during the first year after launch. 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.3 shows that cumulative searches had reached 103 million

[9]See Lexbase's launch announcement, January 27, 2014.

[10]See the media account cited in the text.

[11]Because the survey was conducted or 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.

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

by 2019, when the first close competing platform entered the market. Appendix Figure A.5 documents the accompanying 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."

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.

Lexbase remained a person-searchable court-record database for more than a decade, although its legal position was repeatedly contested. 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. The decisions primarily restricted databases' ability to obtain and redistribute new court records; they did not themselves order Lexbase to remove its existing archive. Lexbase subsequently replaced its original person-search service with a platform based on editorially prepared legal-news articles. In July 2026, the Court of Justice of the European Union further held, in a case arising from Lexbase, that making unedited criminal convictions available online for payment does not generally constitute processing for journalistic purposes and therefore cannot automatically be exempted from the protections and remedies provided by the GDPR.

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

[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 approximately 20 million Lexbase searches per year.

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 relationship status at the end of 2013 and follow post-launch outcomes from 2014 through 2018. 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 measure medication use using the National Prescribed Drug Register, available with individual identifiers from July 2005 onward. The register records prescription drugs dispensed at outpatient pharmacies.[14] We use ATC codes to construct indicators for psychotropic medication and substance-use treatment.

We use the National Patient Register to measure contacts with specialized psychiatric and substance-use care. The register covers both inpatient care and physician encounters in 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. This register measure identifies documented treatment rather than all pre-existing substance use.

3.2 Partnership measures

Our primary relationship measure is based on annual family links in the population registers. At the end of each calendar year, an individual is classified as partnered 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 formal family links. It does not, however, capture most unmarried cohabiting couples without common chil-

[14]It therefore captures medications that were collected rather than prescriptions that were merely written, and it does not include unfilled prescriptions, over-the-counter purchases, or medications administered during inpatient care.

dren 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 register-observed transitions.[15]

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. Being in a relationship is set to one as long as the individual remains with the baseline partner and zero if not. Re-partnering 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. 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.

The complementary woman-level exercise uses a distinct, fixed cohort of women who were single and age 20–35 in 2010 and are followed annually from 2011 through 2018. For each woman-year, we distinguish four mutually exclusive states: no register-observed partner, a partner outside the focal convicted-male sample, an unlisted convicted partner in that sample, and a listed convicted partner in that sample. These states use actual Lexbase listing and describe realized relationship occupancy; they are not assigned by the male-side retention cutoff.

3.3 Criminal-case and violence data

We obtain individual-level criminal-case data from the Swedish Crime Preventive Council (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 observe the relevant court, the date on which the case was closed, the offenses included in the case and any sanction imposed. 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.

[15]Statistics Sweden can also provide a broader dwelling-based household measure that attempts to identify childless cohabiting couples. The algorithm classifies two co-resident adults as a potential couple using information on sex, age difference, kinship, and household composition. Although this measure captures relationships omitted by our primary definition, it can also classify some non-partner co-residents as couples and is sensitive to errors in registered residence. These concerns are especially relevant for younger adults, among whom shared and temporary housing is more common (Andersson et al., 2023). Our preferred measure is narrower but is consistently available throughout the study period, identifies partners longitudinally, and has high specificity because relationships are established through marriage or a common child. It therefore captures established partnerships with substantial legal, financial, or family consequences. Dating and other non-cohabiting relationships remain outside the scope of the register-based analysis.

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.

Post-launch violence outcomes are constructed from BRÅ's register of suspected persons. This register contains criminal suspicions that reached at least the level of reasonable suspicion (*skäligen misstänkt*) following the criminal investigation and were processed by the relevant investigative authority (either the public prosecutor or the police). Such a registration does not establish guilt and may include suspicions that were subsequently dismissed. Importantly, this measure allows us to infer characteristics of the victim, most importantly the gender, age and from 2016 also offender-victim relationship.

Our principal violence outcome indicates whether a male defendant was registered as reasonably suspected of an offense involving violence against an adult woman during 2014–2018. The cumulative outcome equals one if at least one such suspicion is recorded during the five-year follow-up period; the annual analyses use year-specific indicators. Because the outcome requires that an incident become known to and be processed by the criminal-justice system, it measures recorded suspected violence rather than the total incidence of violence against women.

For the offense-specificity exercise, we construct analogous person-level indicators for violence or sexual offending against women, violence or sexual offending against others, property crime, DUI or drug crime, and other crime. That exercise dates offenses by the date of the alleged conduct and pools physical and sexual offenses within the two violence categories. We also construct a higher-threshold outcome indicating whether any post-launch violence-against-women case was brought to trial. This outcome is defined unconditionally for the full male sample. It requires a recorded incident to survive investigation and a prosecutor to judge the evidence sufficient to proceed, but it is not a conviction. Finally, the prior-substance-history split uses a DUI or drug suspicion recorded before Lexbase.

3.4 Lexbase listing data

Lexbase supplied information identifying the individuals and court cases that were searchable on the platform in the time of its launch. We link these records to the administrative data and define listed as an indicator equal to one if the defendant's index case was searchable on Lexbase at launch. Thus, listing refers to actual public visibility on the platform, not merely to whether the case was formally eligible to be disclosed.

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.

3.5 Defendant sample

The regression-discontinuity sample is constructed from male defendants whose index district-court cases were closed between 2007 and 2010 and lie within 16 weeks of the relevant court-specific retention cutoff. The running variable is the number of weeks between the case's closure date and the estimated cutoff for the court. Online Appendix F describes how we estimate the court-specific cutoffs and explains why crossing a cutoff sharply changes the probability that a case was available on Lexbase at launch.

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. We further restrict the main specification to first-time defendants, defined as individuals with no earlier district-court conviction (since 1973). The first-time restriction ensures that each defendant contributes a single index case to the estimation sample. We report corresponding results for repeat defendants.[16]

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.

The fixed female cohort described above is not another fuzzy-RD estimation sample. Its event study compares relationship states defined by actual listing and is used only as descriptive evidence on realized relationship allocation.

3.6 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.

[16]Moreover, most non-disclosure policies explicitly target first-time defendants.

4 Retention limits and the RDD

4.1 Retention limits and cutoff dates

A critical element of our RDD is the legal retention limit for case information in the courts' searchable administrative records, which, in combination with the Lexbase launch date, determines assignment to treatment. In principle, courts were required to remove criminal-case information from the searchable portion of *Vera* after five years. This requirement applied to the underlying court system rather than to Lexbase itself. Lexbase's requests should therefore, in principle, have yielded readily retrievable criminal cases extending back approximately five years from the date on which each request was processed.

Figure 1 helps to illustrate our design. It depicts the timeline for a court whose records were requested in January 2014 when the Lexbase website was launched. We assume for simplicity that five years of criminal-case records were made available at that time. With a five-year retention limit, cases closed before the cutoff date of January 2009 were unlikely to appear in Lexbase and therefore were unlikely to be available to users at launch. Cases closed after the cutoff were substantially more likely to appear in Lexbase. Because the mapping from cutoff assignment to realized listing is imperfect, the design is fuzzy rather than sharp.

Our treatment-assignment group consists of individuals whose cases closed within a short interval after the cutoff date, while the comparison group consists of individuals whose cases closed within a similarly short interval before it. The figure illustrates the 32-week window used in the main analysis.[17] If the intervals are sufficiently narrow, potential outcomes should evolve smoothly through the cutoff, while the probability that the case is searchable in Lexbase changes discontinuously. The fuzzy RD exploits this discontinuity to identify the effects of public listing over the January 2014–December 2018 follow-up period.

In practice, Lexbase requested records from different courts at different times, so cutoff dates are court-specific. Some vagaries surrounding the retention limits, as well as the massive scale of the Lexbase requests, further contributed to variation among courts in effective cutoff dates. The specific cutoff date for each court was determined by the timing of Lexbase's request for information, the length of time it took the court to process the request, and the exact expungement procedures followed by the court.

To estimate court-specific cutoff dates, we obtained data from Lexbase on the number of cases added to the database for each court in each week. We used these data, together

[17] We use this interval for our main results and vary the bandwidth in robustness checks.

with nationwide register data on the near-universe of all criminal charges in Sweden since 1973, to estimate the share of cases included in the Lexbase data for each court and week.[18] We then computed the change in the share of included cases each week by calculating the difference between the average share of cases in the preceding 16 weeks and the average share in the subsequent 16 weeks. We estimated the cutoff date as the week exhibiting the largest increase in the share of included cases over the entire period. Online Appendix F contains additional details on the construction of the court-specific cutoffs.

4.2 The RDD

The discussion above motivates a fuzzy regression-discontinuity design. We estimate it on first-time defendants aged 20–60 whose cases fall within 16 weeks of the relevant court-specific disclosure cutoff. Let X_{ij} denote the number of weeks between the date on which defendant i's case was closed and the disclosure cutoff for court j, normalized so that $X_{ij} = 0$ at the cutoff. Let

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

indicate that the case closure falls on the post-cutoff side of the threshold. The treatment D_{ij} equals one if the defendant appears in Lexbase 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 cutoff-year fixed effects, and B_i contains predetermined covariates. The coefficient α_1 is the discontinuity in the probability of appearing in Lexbase and β_1 is the reduced-form discontinuity in 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}. Equivalently, in the just-identified specification, $\tau = \beta_1/\alpha_1$.

Because cutoff assignment does not determine listing perfectly, the design is fuzzy. The reduced-form coefficient β_1 captures the effect of falling on the post-cutoff side of the threshold, while τ scales that discontinuity by the first stage. Under continuity, exclusion,

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

and monotonicity, τ identifies the effect of public listing for defendants whose listing status is changed by the retention cutoff. The estimated first stage of 0.732 implies that this group constitutes approximately 73 percent of the local population at the cutoff. The estimand therefore pertains to a large share of nearby defendants, although it need not equal the effect for defendants whose listing status would be unchanged by cutoff assignment.

Equations (1)–(3) display the baseline local-linear specification. Robustness checks vary the bandwidth and the order of the running-variable polynomial and assess sensitivity to additional controls constructed from the population registers. The identifying assumptions are that potential outcomes and baseline characteristics evolve smoothly through the cutoff, crossing the cutoff affects outcomes only through public listing, and moving to the post-cutoff side does not reduce any defendant's probability of being listed.

4.3 Validating the research design

4.3.1 First-stage and monotonicity

Estimating equation (1), we find that having a case close after the cutoff substantially increases the probability of appearing in Lexbase. In the first-stage validation specification reported in column (2) of Table A.2, the estimate of α_1 is 0.732 (0.033). This indicates that the likelihood of being publicly listed in Lexbase is about 70 percentage points higher for the treatment group than the comparison group. This is a very strong first stage, as indicated by the Sanderson-Windmeijer F-statistic of 654.9 and the Cragg–Donald F-statistic of 4569.7.

Figure 2 provides a visual illustration of the first stage. It displays the average share of cases included in the database around the estimated cutoff for the courts in our sample. As expected in a fuzzy design, the listed share does not jump from zero to one at the estimated cutoff. Roughly 20 percent of defendants whose cases closed before the cutoff are nevertheless listed, partly because some reoffended between the cutoff and launch. Conversely, about 10 percent of defendants whose cases closed after the cutoff are not observed in the database, reflecting incomplete recovery, matching error, or imprecision in the estimated cutoff. The company behind Lexbase continued to add cases to the database—collected during the pre-period—even after its public launch in January 2014; however, our analysis focuses on the roughly 200,000 cases that were available at the time of launch. In addition, some courts provided information covering periods extending further back in time than the retention period.[19]

[19]One possible explanation for the slight downward slope is that defendants whose cases closed closer

Monotonicity is supported primarily by the institutional mechanism: moving a case to the more recent side of the retention boundary made it easier, not harder, for Lexbase to retrieve, and crossing the cutoff did not trigger any countervailing rule that removed an otherwise available case. Consistent with this mechanism, Table A.2 reports positive and similar first-stage estimates across the principal prespecified subgroups. These subgroup estimates are supportive diagnostics rather than a test of the individual-level no-defiers condition.

4.3.2 Independence and exclusion

Table A.3 provides evidence consistent with the independence assumptions. Column (1) reports estimates from a regression of treatment status, D_{ij}, on the covariates listed in Table A.1. Several of the displayed variables significantly predict being listed in Lexbase, including age, education, charge type, and selected sentence outcomes. Jointly, the covariates are highly predictive: the p-value on the F-test is effectively zero. This is consistent with the fact that actual listing status is endogenous and reflects both underlying defendant characteristics and subsequent criminal behavior.

Column (2) of Table A.3 instead assesses the exogeneity of the instrument by regressing the assignment variable, Z_{ij}, on the same set of covariates. In contrast to column (1), the covariates are jointly uninformative about assignment: the p-value on the F-test is 0.234. These results are consistent with the view that whether a defendant falls just before or just after the cutoff is as good as random. This interpretation is further supported by the fact that observable baseline characteristics evolve smoothly around the cutoff, and that both crime type and sentence severity are balanced on either side.

Of course, the exclusion restriction could still fail if falling after the case-closure cutoff affected later outcomes through channels other than database inclusion. This assumption is fundamentally untestable, and our estimates should be interpreted with that caveat in mind. Even so, we believe it is plausible in our setting. Most importantly, the court-specific cutoffs were determined several years before the launch of the platform, which was entirely unknown at the time.[20] It is therefore unlikely that courts, defendants, or the general public adjusted behavior in response to the cutoffs. Moreover, because the cutoffs are temporally distant from the launch, the estimates are unlikely to be contaminated by general deterrence effects operating through changes in the composition of defendants after Lexbase was introduced. Put differently, the platform could not have affected pre-launch case timing because it did not yet exist. Consistent with this interpretation,

to the cutoff had less time before launch in which to incur a subsequent charge that would independently place them in Lexbase.

[20] Recall that the company behind Lexbase did not register until 2012/13.

Appendix Figure A.8 shows no visible evidence of manipulation around the threshold.[21]

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.

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

[21] The McCrary test is not statistically significant at conventional levels.

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.[22]

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 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

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

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.[23] 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 provided 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

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

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 couples' 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 de-

tect 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.[24]

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

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

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.[25] 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.

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

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

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 prac-

tically 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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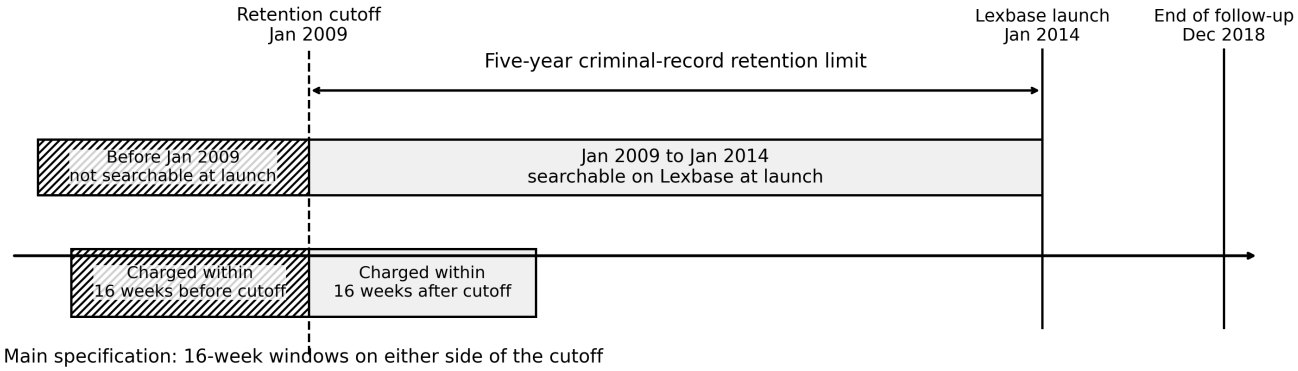
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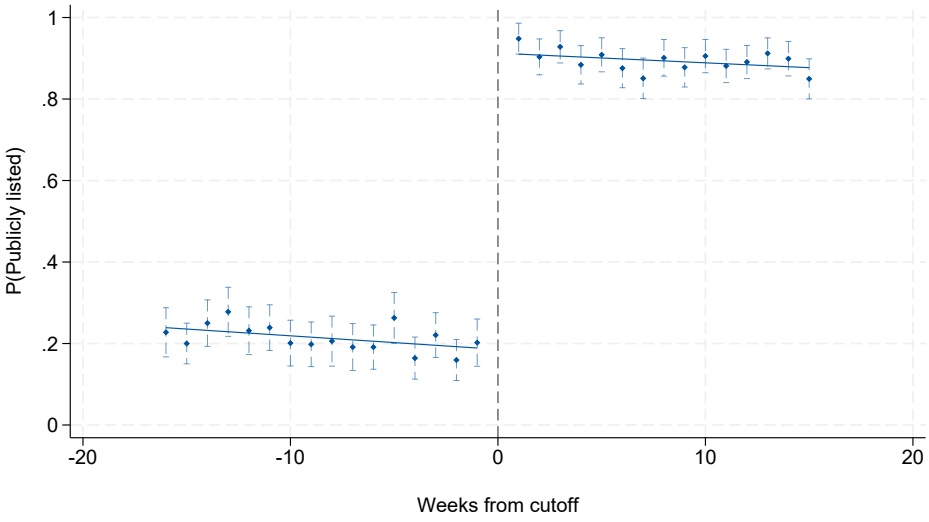
Figures and Tables

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



Note: The figure shows the probability of being publicly listed on the Lexbase launch date by the week in which the individual was first charged with a crime. The probability is greater than zero to the left of the cutoff because some individuals first charged prior to the cutoff are charged again after the cutoff. The probability is below unity after the cutoff because some courts delivered less than five full years of records by the launch date, as discussed in section 4.3. 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. The 95 percent 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

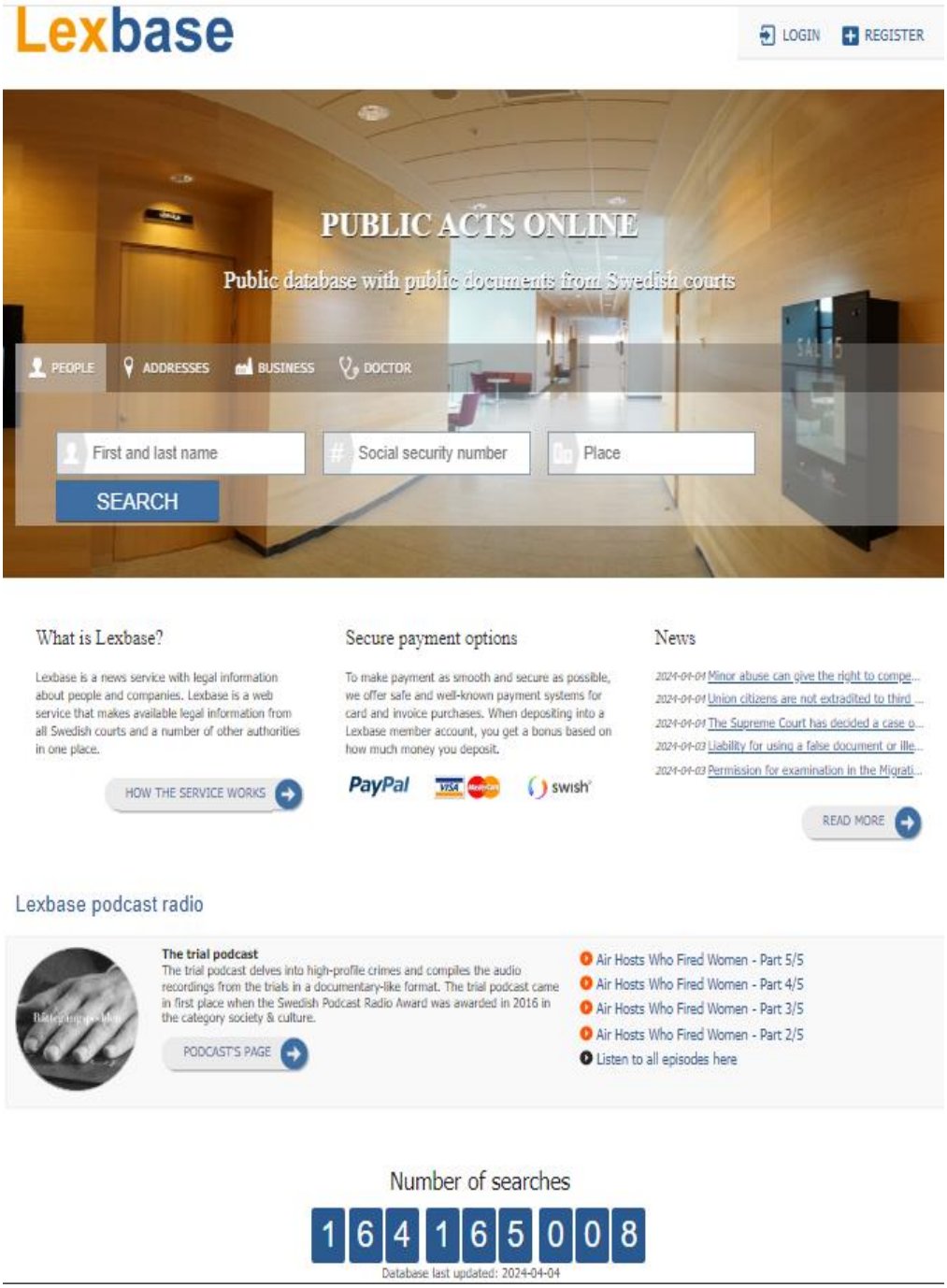
Notes: The sample and specification are the same as in Table 5. Outcomes indicate whether the man experienced the stated outcome during 2014–2018. The welfare-receipt mean is suppressed pending verification of the coding in the source data. Table 5 reports the corresponding estimate for recorded DUI/drug 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

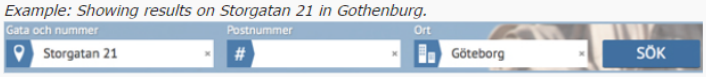


In the search fields, you can use asterisk (*) in your search text to replace part of a search term. You also don't need to search for an exact name of a person or company. Parts of the name in question are sufficient.

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

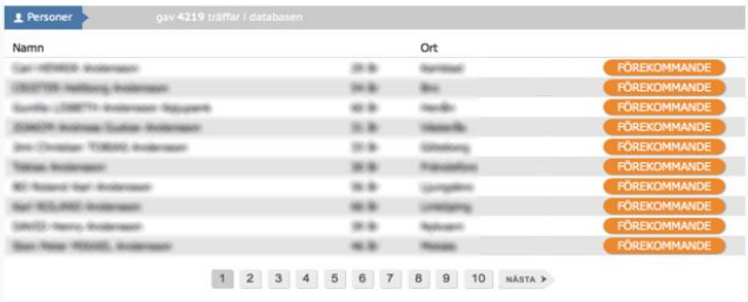
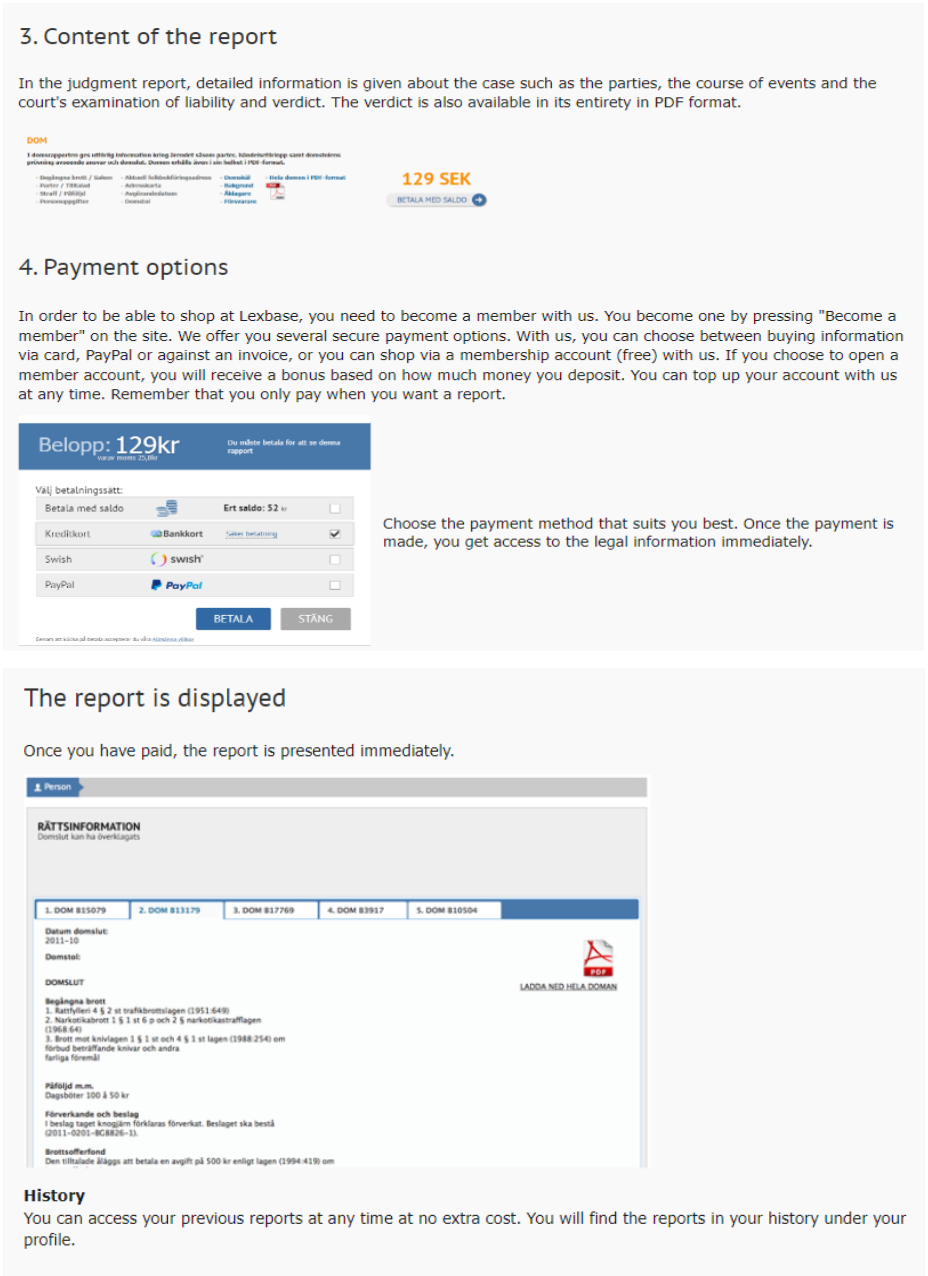


Figure A.2: How the Search Engine at Lexbase Works



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.

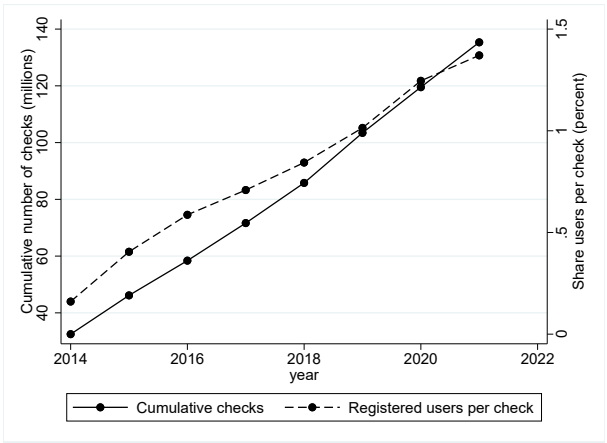


Figure A.3: Background checks using Lexbase

Notes: This figure plots the cumulative number of searches for criminal background using the platform. The figure also plots the number of registered users per background check conducted. The figure uses data provided by Lexbase. WHAT IS ADDED BY THE NUMBER OF REGISTERED USERS PER BACKGROUND CHECK?

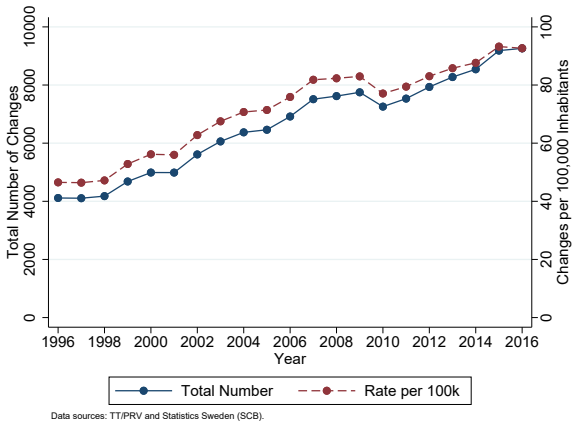


Figure A.4: Surname changes in Sweden

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.

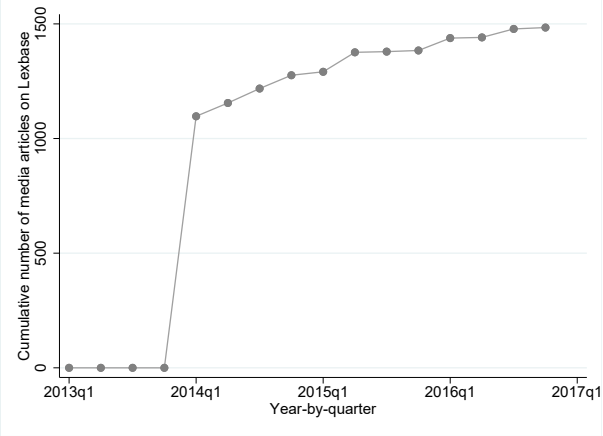


Figure A.5: Cumulative number of media articles covering Lexbase, 2013-2016

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.

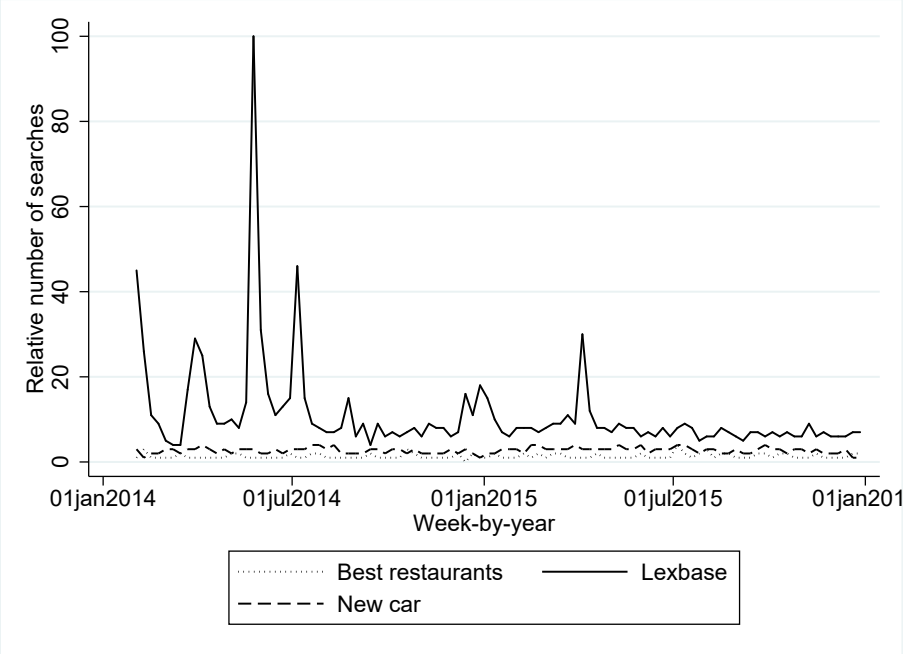


Figure A.6: Relative number of searches on Google

Notes: This figure plots the relative number of Google searches for the terms "Lexbase," "Best restaurants," and "New car." The data were collected from Google Trends on January 20, 2024. Google Trends normalizes search data to facilitate comparisons across terms. Search intensity is normalized by dividing each observation by the total number of searches in the relevant geography and time period, and then scaling the resulting values from 0 to 100. To improve readability, we omit the days immediately following the launch of the database, when searches spiked sharply, and instead focus on the period beginning one week after the introduction and ending in the last week of 2015.

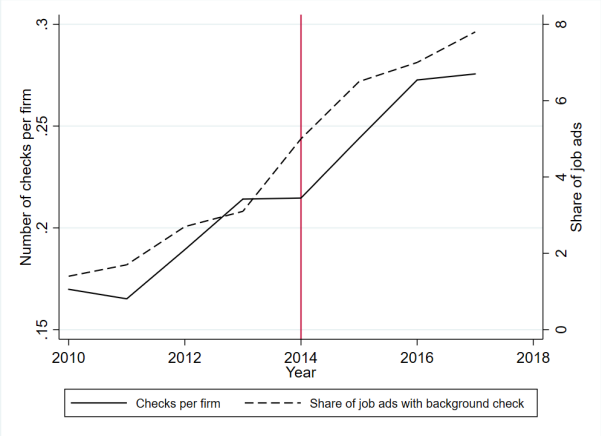
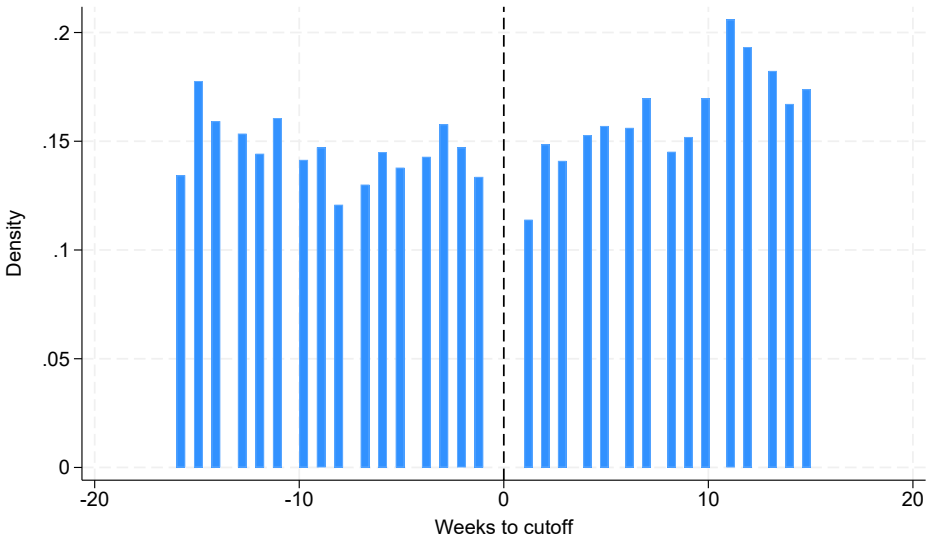


Figure A.7: Official (non-Lexbase) criminal background checks

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

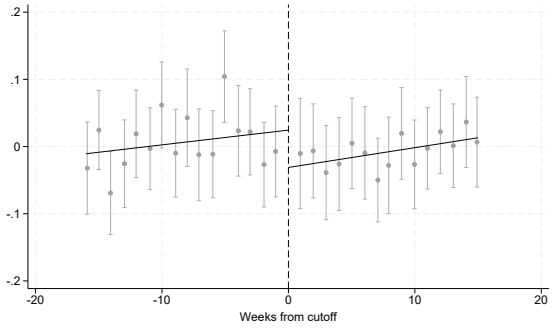


Note: The sample consists of male defendant aged 20 to 60 who were first charged within a 16-week window around the court-specific cutoff at week 0. RDD estimates show no significant change in the (log) number of cases around the cutoff (est. 0.141; se 0.207).

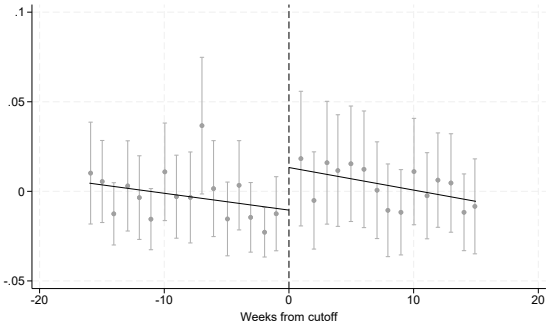
Figure A.9: Reduced form effects on main outcomes and placebo year

Panel A: Main outcomes (2014–2018)

(a) P(Having a partner)

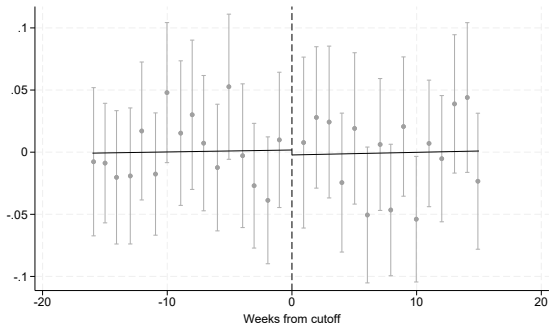


(b) P(Violence against women)

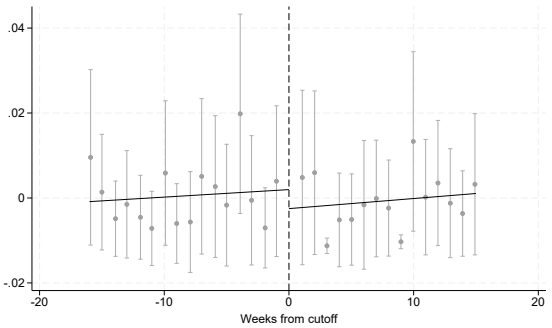


Panel B: Placebo year (2013)

(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 Fuzzy-RD estimand

Let $D_i(z)$ denote whether defendant i would be listed if the index case fell on cutoff side $z \in \{0, 1\}$, and let $Y_i(d)$ denote the corresponding potential outcome under listing state d. Define the local reduced-form and first-stage discontinuities as

$$\Delta_Y = \lim_{x \to 0^+} E[Y_i \mid X_i = x] - \lim_{x \to 0^-} E[Y_i \mid X_i = x],$$
$$\Delta_D = \lim_{x \to 0^+} E[D_i \mid X_i = x] - \lim_{x \to 0^-} E[D_i \mid X_i = x].$$

Under continuity at the cutoff, exclusion, and monotonicity, the fuzzy-RD Wald ratio identifies

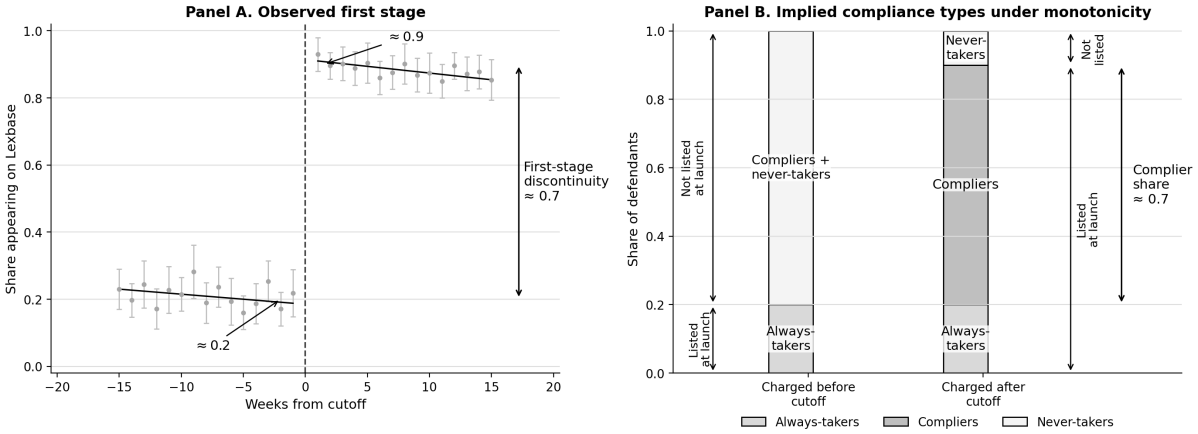
$$\tau_{\text{FRD}} = \frac{\Delta_Y}{\Delta_D} = E[Y_i(1) - Y_i(0) \mid D_i(1) > D_i(0),\, X_i = 0]\,. \tag{4}$$

The condition $D_i(1) > D_i(0)$ defines defendants whose listing status is changed by the retention cutoff. The estimated first-stage discontinuity of 0.732 implies that this group constitutes approximately 73 percent of the local population. The estimand is therefore relevant for a large share of nearby defendants, although it need not equal the effect for defendants whose listing status would be unchanged by cutoff assignment.

One possible account of the smaller OLS coefficients is treatment-effect heterogeneity in the consequences of public visibility. Let τ_C denote the effect for defendants whose listing is induced by crossing the cutoff and τ_A the effect for defendants who would be listed on either side. For the cutoff-induced group, Lexbase makes an aging and otherwise practically obscure record newly searchable. Defendants who would be listed regardless often have newer criminal-justice contact that independently makes them visible. Their partners may already know about that contact, their behavior may convey similar adverse information, or the newer case may already create the relevant audience and stigma costs. If the effects have the same sign and $|\tau_A| < |\tau_C|$, heterogeneity could contribute to an OLS coefficient closer to zero than the fuzzy-RD estimate. Approximately nondifferential misclassification of realized listing could also attenuate OLS, although its precise implications for 2SLS depend on the measurement-error structure. Because the OLS comparison may additionally reflect selection in untreated potential outcomes, the OLS–IV gap does not identify treatment-effect heterogeneity or any particular disclosure channel.

Figure A.10 illustrates the compliance-type interpretation. The local listing rate is approximately 0.2 immediately below the cutoff and 0.9 immediately above it. Under monotonicity, these rates correspond approximately to a 0.2 share that would be listed on either side of the cutoff, a 0.7 share whose listing is induced by crossing the cutoff, and a 0.1 share that remains unlisted on either side. The figure emphasizes both the strength of the first stage and the fact that the 2SLS estimand applies to a large majority of defendants near the threshold.

Figure A.10: First stage and compliance types



Notes: Panel A shows the first-stage discontinuity in Lexbase listing at the court-specific cutoff. Panel B gives the corresponding compliance-type interpretation under monotonicity. The listing rate immediately below the cutoff approximates the local always-listed share, the discontinuity approximates the local complier share, and one minus the listing rate immediately above the cutoff approximates the local never-listed share. Compliers are defendants whose appearance on Lexbase is changed by whether their index case falls within the retention window.

B.2 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

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

and the local outcome means among unlisted defendants

$$m_- = \lim_{x \to 0^-} E[Y_i \mid D_i = 0, X_i = x], \qquad m_+ = \lim_{x \to 0^+} E[Y_i \mid D_i = 0, X_i = x]. \tag{6}$$

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

$$\omega = \frac{1 - p_+}{1 - p_-}. \tag{7}$$

The below-cutoff unlisted mean can therefore be written as

$$m_- = (1 - \omega)\, CCM + \omega m_+, \tag{8}$$

which gives the control complier mean

$$CCM = \frac{m_- - \omega m_+}{1 - \omega}. \tag{9}$$

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

$$Y_{itd} = \alpha_{it} + \lambda_d + \sum_{\tau \neq 2013} \Big[\beta_\tau^1 1\{t = \tau\} 1\{d = 1\} + \beta_\tau^2 1\{t = \tau\} 1\{d = 2\} + \beta_\tau^3 1\{t = \tau\} 1\{d = 3\} \Big] + \varepsilon_{itd}. \tag{10}$$

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

$$\theta_\tau = \beta_\tau^3 - \beta_\tau^2, \tag{11}$$

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

$$\Delta_{ijk} = \underbrace{\rho_{jk}\kappa_i}_{\Delta_{ijk}^{\text{belief}}} + \underbrace{\sigma_{jk}\chi_i}_{\Delta_{ijk}^{\text{audience}}} \geq 0, \tag{12}$$

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 (12). 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

$$m_{ij} = a_{ij} - o_j + c_j. \tag{13}$$

After disclosure, the woman remains if

$$m_{ij} \geq L_i \Delta_{ijI}. \tag{14}$$

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

$$P^{\text{sep}}(1) = G_I(\Delta_I), \qquad P^{\text{sep}}(0) = G_I(0), \tag{15}$$

so that

$$P^{\text{sep}}(1) - P^{\text{sep}}(0) = G_I(\Delta_I) - G_I(0) \geq 0. \tag{16}$$

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

$$E\Big[Y_j^E - Y_{ij}^S \mid 0 \leq m_{ij} < \Delta_{ijI}\Big] > 0, \tag{17}$$

then exit raises the average measured economic outcome among women in the relationships whose status is changed by listing. Equation (17) 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

$$a_i + \varepsilon_{ij} - o_t - L_i \Delta_i^N \geq 0. \tag{18}$$

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

$$m_t(L_i) = 1 - \frac{o_t - a_i + L_i \Delta_i^N}{\bar{\varepsilon}}. \tag{19}$$

The aggregate probability that a potential interaction becomes a match is

$$M_i(L_i) = \pi_v m_v(L_i) + \pi_h m_h(L_i). \tag{20}$$

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

$$M_i(1) - M_i(0) = -\frac{\Delta_i^N}{\bar{\varepsilon}} < 0. \tag{21}$$

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 (14) 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

$$s_v(L_i) = \frac{\pi_v m_v(L_i)}{M_i(L_i)} \tag{22}$$

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

$$\frac{\partial s_v}{\partial c} = \frac{\pi_v \pi_h \left[m_v(0) - m_h(0) \right]}{M_i(L_i)^2} > 0. \tag{23}$$

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 (21).

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

$$n_U > n_P. \tag{24}$$

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

$$R_i(L_i, b) = n_b \left[1 - M_i(L_i) \right]. \tag{25}$$

Combining equations (21) and (25), and holding the number of potential interactions

fixed, listing raises expected non-matching interactions by

$$R_i(1,b) - R_i(0,b) = \frac{n_b \Delta_i^N}{\bar{\varepsilon}}. \tag{26}$$

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

$$H_i(L_i,b) = h_i + \gamma R_i(L_i,b) + \omega_i L_i, \qquad \gamma, \omega_i \geq 0, \tag{27}$$

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

$$\Pr(V_i = 1 \mid L_i, b) = \mathcal{G}(\nu_i + \phi H_i(L_i,b) + \psi z_i(L_i)), \qquad \phi, \psi \geq 0, \tag{28}$$

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 (26)–(28) 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 (28) 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.

Continued on next page

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

$$p_{jw} = \frac{\text{number of cases closed in court } j \text{ in week } w \text{ that appear in Lexbase}}{\text{number of cases closed in court } j \text{ in week } w \text{ in the administrative data}}. \tag{29}$$

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:

$$\Delta_{jw} = \frac{1}{16}\sum_{h=0}^{15} p_{j,w+h} - \frac{1}{16}\sum_{h=1}^{16} p_{j,w-h}. \tag{30}$$

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

$$\widehat{c}_j = \arg\max_w \Delta_{jw}. \tag{31}$$

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,

$$X_{ij} = \text{closure week}_{ij} - \widehat{c}_j, \tag{32}$$

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.