An Experiment in Background Check Adjudication & Fair Chance Hiring *

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Abstract

Criminal background checks are a standard feature of hiring in the United States, yet little evidence exists on whether firms use background check data effectively to select candidates. We partner with a large online services marketplace that conducts criminal background checks on all providers seeking entry to the platform. In collaboration with the firm, we implement a randomized experiment that suppresses a set of non-violent criminal charges for randomly selected applicants, increasing platform access by 3.3 percentage points (5%) among applicants with criminal records. Cross-sectional comparisons show that workers with criminal records generate lower customer engagement and higher rates of adverse customer incidents. Yet, local average treatment effect estimates from the experiment show that randomly admitted marginal service providers perform no worse than average on either dimension, meaning that marginal applicants screened out by criminal records are not less productive or riskier than the average service provider admitted onto the platform. Because individual criminal charges are heterogeneous and offer limited statistical power for predicting adverse customer outcomes, fine-grained screening rules will have high rates of both false negatives and false positives at the margin.

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1 Introduction

Criminal background checks are widely used in hiring in the United States. In a 2021 survey, 90 percent of firms reported conducting background checks on all full-time employees and 59 percent reported conducting background checks on all contractors (Professional Background Screening Association, 2021). Firms cite protecting customers, improving quality of hires, and mitigating reputational risk as primary motivations (HR.com, 2018). Criminal histories are commonplace among the US working-age population: over one-third of working-age adults have been arrested (Goggins and DeBacco, 2024). Records are disproportionately concentrated among less-educated men, minorities, and the poor (Shannon et al., 2017; Bucknor and Barber, 2016; Agan and Starr, 2018). Consequently, criminal background screening is likely to affect labor market access among groups that already face substantial barriers to employment.

In practice, firms map detailed criminal histories into relatively simple decision rules, typically based on charge type, severity, and time since offense. But it is unclear whether these rules are well-calibrated to predict on-the-job performance or risk.

We assess the efficacy and distributional consequences of criminal records screening by analyzing the adjudication process at a large online marketplace platform. In partnership with the firm and its background check provider, we implement a randomized experiment that reduces the visibility of selected criminal charges for a subset of applicants. This intervention effectively allows some new applicants with records access to the marketplace who would otherwise have been turned away. Because we observe the full criminal record, the record visible to the adjudicator, and subsequent marketplace outcomes, we can estimate average criminal record characteristics and outcomes for these marginally admitted workers.

Our results show that while applicants with criminal records perform worse in cross-sectional comparisons, marginally admitted service providers do not exhibit worse engagement or safety outcomes, as measured by incident rates, customer ratings, and platform activity. This pattern suggests that commonly used screening rules may over-exclude workers whose realized performance is similar to that of admitted workers.

A large literature documents correlations between criminal records and subsequent outcomes, including recidivism (Blumstein and Nakamura, 2009; Bushway, 2024), non-employment (Mueller-Smith and Schnepel, 2021; Rose, 2021; Agan et al., Forthcoming), and employer callbacks (Pager, 2003; Uggen et al., 2014; Agan and Starr, 2018). Employers also report substantial reluctance to hire individuals with records (Holzer et al., 2006; Cullen et al.,

2022). However, these correlations do not isolate the information content of criminal records for predicting workplace performance or risk. Criminal records covary with a wide range of underlying characteristics, including prior behavior, human capital, and labor market opportunities, making it difficult to determine how much of the observed association is causal.

Recidivism provides one benchmark for how informative criminal records may be. Prior work shows that risk declines sharply with time since last criminal contact and eventually converges toward population averages (Blumstein and Nakamura, 2009; Bushway et al., 2009; Kurlychek et al., 2012). But recidivism is not the only decision-relevant outcome for employers. Firms also care about productivity, reliability, and customer- or coworker-facing risks, and there is comparatively little evidence linking criminal histories to these outcomes. Additionally, firms worry about very rare but salient tail risks like negligent hiring lawsuits.[1]

Existing evidence on workplace performance is limited and largely observational. Lundquist et al. (2018) studies US military enlistees and finds that those with felony records are no more likely to attrit and, if anything, are promoted more quickly than those without. Minor et al. (2018) uses data from a call center and finds that workers with records are less likely to quit and have similar or slightly better performance. While informative, these studies focus on selected samples of hired workers and rely on comparisons conditional on observables, leaving open how criminal records relate to outcomes at the margin of hiring decisions.

Our paper contributes to this literature in three ways. First, we provide experimental evidence on the consequences of relaxing criminal record screening thresholds, allowing us to isolate outcomes for marginally admitted workers rather than relying on comparisons among observationally selected workers. Second, we study a setting with rich measures of on-the-job performance, including engagement, productivity, and safety outcomes that are directly relevant to firm decision-making. Third, because the intervention operates through the firm's existing adjudication rules, we are able to link specific features of criminal records, such as charge type and time since offense, to both hiring decisions and subsequent outcomes.

[1] While Douglas and Maltby (2023) find only 435 cases of employers liable for negligent hiring because they employed someone with a conviction over a 50-year period, Pyle (2023) finds that reducing firms' exposure to negligent hiring legal liability raises the hiring of workers with criminal records.

2 Institutional Setting

We partner with an online marketplace company and an external background check company to run this experiment.

2.1 Online Marketplace Companies

Online marketplace companies are online platforms that connect sellers of products or services with buyers of those products or services. They sit between traditional employment and pure classifieds by intermediating matching, reputation, and sometimes even payment between service providers and customers. In our case, this online marketplace company is a platform that connects service providers with users needing assistance with various short-term/immediate mostly in-person tasks.

Service providers who are allowed onto the platform after a background check may contact potential clients who have posted tasks they need assistance with. Clients then can choose a provider (or no provider, or a provider outside the platform). They can leave a review for the provider on the platform which, similar to most platforms includes a 1-5 star rating and potentially some text. If they have a problem, they can report an incident to the platform which the platform will investigate and may take action on if substantiated (such as removing a service provider from the platform).

2.2 The Background Check Process

The online marketplace company we partner with uses an external company to perform court record searches on applicants before allowing them to access the marketplace.

Broadly, there are two main types of background checks: fingerprint background checks (which can only be performed in certain legally mandated circumstances) and court record searches based on identifying information such as name, date of birth, and location provided by the candidate. To perform a court record search, background check companies conduct comprehensive, automated searches of federal, state, and county records. These companies are consumer reporting agencies (CRAs) and must adhere to the Federal Credit Reporting Act (FCRA) and any relevant state and local regulations regarding use of criminal record information for employment purposes.

Because background check companies are subject to FCRA, they must inform job candidates whenever information from their criminal background is used to make decisions that adversely affect them. The goal is to allow job candidates an opportunity to flag inaccurate

information that appeared on their record or to appeal the decision by providing information about extenuating circumstances.[2] This notice is called an "adverse action" notice.

Manually reviewing high volumes of background checks is a time-intensive process. To codify and streamline their review processes, the marketplace company has pre-specified the relevance of each charge. For certain categories, dispositions, and classifications they find irrelevant and wish to hide from their adjudicators, the consumer reporting agency then removes these charges from new background checks before sending those background check results to that firm's adjudication team. This lightens the burden of costly manual review and allows adjudicators to focus their discretion on the remaining records.

Thus, for our marketplace company and criminal background check agency partners, the standard adjudication process proceeds as follows (described visually in Figure 1):

1. The background check provider retrieves the applicant's full criminal record (and applies any relevant state and federal rules that regulate the reporting of different charges, e.g. FCRA).

2. A rule-based visibility matrix, chosen by the marketplace company, is applied by the background check company and determines which charges are visible.

3. If no visible charges remain, the applicant is automatically passed through the screening process ("allowed" on the marketplace) with no human intervention.

4. If visible charges remain, a human adjudicator reviews the record and either allows the applicant on the marketplace or issues a pre-adverse action notice. Federal guidance suggests waiting five business days for a response before continuing to the post-adverse action stage.[3]

The rule-based visibility matrix is based off of crime type (one of 10 charge categories and 55 charge subcategories), level of crime (felony, misdemeanor, or petty offense), and years since the offense. The full pre-experimental matrix this marketplace company implemented is described in Appendix A.

The firm has a small cadre of part-time adjudicators whose only job for the firm is to make decisions on criminal records. Adjudicators face no binding capacity constraint and have no additional information about the applicant besides their name, date of birth, location,

[2]Criminal histories frequently include information fuzzily matched to an individual through inaccurate entity matching (Lageson and Stewart 2023).

[3]It is rare for candidates to reverse an adverse action process in this five-day period. In our, data, fewer than 5% manage this.

and visible criminal record history. Conceptually, they admit applicants if expected benefits exceed expected costs.

Marketplace companies benefit from having additional individuals providing services on the marketplace because this can lead to faster and more engagement with potential customers, and potentially higher customer satisfaction based on the quality of the applicant. And thus the firm benefits from additional people in the marketplace. At the same time, admitting applicants who do not perform well or cause incidents could lead to harm in terms of fewer customers or reputational risks.

3 Experimental Design

Between July 24, 2023 and September 1, 2024, all applicants who wished to be admitted to the online marketplace and consented to a background check were randomly assigned to:

- **Control matrix**: Status quo charge visibility rules.
- **Treatment matrix**: Additional categories of charges hidden.

Table 1 shows the marginal changes that the treatment matrix made above and beyond the control matrix (Appendix A includes the full "control" matrix). The threshold is measured in years and indicates how long a certain type of charge stays visible on the criminal background check adjudicators see; this is sometimes known as a "look back" window. An individual assigned to the treatment matrix would have a charge hidden if it would be hidden in the control matrix, or if it would be hidden by any of the marginal charges added in the treatment matrix. The treatment primarily reduced visibility of older misdemeanor traffic and drug-related offenses, and certain felonies, by shifting look-back windows based on the age of the charge from 7 years to 3 or 0 years; or from 3 to 0 years. A 0-year look-back window indicates that this type of criminal charge is never visible to adjudicators. The most common types of charges made invisible by the treatment matrix are car-related misdemeanors: most are license & registration and unsafe operation charges, followed by DUIs, speeding, and vehicle equipment charges.

The treatment matrix changes the visibility of applicants' criminal records in heterogeneous ways. We classify applicants with charges by how the treatment matrix impacts their visible criminal record compared to control into the following treatment response types (akin to the principal strata of Frangakis and Rubin 2002, where the stratifying variable is the visibility

of the criminal record under each matrix):

1. Always Clear: the applicants' criminal record is already completely clear (hidden) under the control matrix, and thus is still completely clear (hidden) under the treatment matrix.

2. Newly Clear: the applicant has at least some visible charges under the control matrix, but under the treatment matrix, the record is completely clear (all charges hidden).

3. Reduced Visibility: the applicant has at least some visible charges under the control matrix, and some but not all of these are hidden under the treatment matrix.

4. Same Visibility: the visible criminal record remains the same under both treatment and control matrices.

Randomization was done via computer in code written and implemented at the background check company. The human adjudicators who worked at the firm were not made aware the experiment was being run, and no visual indicator appeared on the background checks they reviewed indicating whether an applicant was assigned to the treatment or control matrices. Randomization was done at the individual level throughout the period with no stratification. Even applicants with no charges were technically assigned to either matrix (though of course not impacted by this assignment).

4 A Model of Criminal Record Screening

This section formalizes the firm's screening problem and clarifies what our experiment identifies.

Applicants differ in a latent type $\theta_i \in \mathbb{R}$ that determines expected productivity and risk on the platform. If admitted, applicant i generates expected benefits

$$B_i = E[b(\theta_i)]$$

and expected costs

$$C_i = E[c(\theta_i)].$$

In the absence of capacity constraints, the firm admits applicant i if expected surplus is

non-negative given available information:

$$E[S_i \mid \mathcal{I}_i] \geq 0,$$

Where $S_i = B_i - C_i$ and $\mathcal{I}_i$ denotes information available to the adjudicator about the applicant at the time of screening.

The firm does not observe θ_i directly. Instead, it observes criminal record characteristics R_i and limited background information X_i (e.g., name, date of birth, and location). We assume

$$S_i = \mu(R_i, X_i) + \varepsilon_i,$$

where ε_i captures unobserved determinants of surplus. The optimal decision rule is therefore:

$$\text{Admit if } \mu(R_i, X_i) \geq 0.$$

In practice, this corresponds to a threshold rule in criminal record space.

Let $V(R_i)$ denote the portion of the criminal record visible under a given screening matrix. The experiment randomizes between

$$V^C(R_i) \quad \text{(control)} \qquad \text{and} \qquad V^T(R_i) \quad \text{(treatment)},$$

where $V^T(R_i)$ hides additional charges relative to control. Adjudicators observe only $V(R_i)$ and are unaware of the experiment, so the decision rule becomes:

$$\text{Admit if } E[S_i \mid V(R_i), X_i] \geq 0.$$

Thus, the treatment coarsens the signal by removing selected information.

Define $D_i(V) = \mathbf{1}\{E[S_i \mid V(R_i), X_i] \geq 0\}$ as the admission decision under visibility rule V. Compliers lie at the margin of the control threshold and satisfy:

$$D_i(V^T) = 1 \quad \text{and} \quad D_i(V^C) = 0,$$

Under standard IV assumptions, including monotonicity, the IV estimand using treatment assignment as an instrument identifies:

$$\beta^{IV} = E[S_i \mid D_i(V^T) > D_i(V^C)]$$

the average realized surplus for applicants at the screening margin induced by criminal record visibility.

If the firm's beliefs are well calibrated and its objective is correctly measured in our data, then marginal applicants should satisfy

$$E[S_i \mid D_i(V^C) = 0, D_i(V^T) = 1] < 0.$$

Empirically, if marginally admitted workers exhibit similar productivity and no increase in safety-related outcomes, then realized surplus among compliers appears non-negative, suggesting that the control group screening threshold was too strict.

Such miscalibration may arise if $\mu(R_i, X_i)$ places excessive weight on certain criminal record characteristics that are not predictive of outcomes, if adjudicators lack feedback on rejected applicants and therefore cannot update beliefs about marginal hires, or if records proxy for unobservables in cross-sectional data.

One potential rationale for strict screening is concern over *tail risk*—rare but high-cost events not well captured by average outcomes. Our estimates are informative about mean realized surplus for marginal applicants, but may underweight low-probability, high-severity risks if these are imperfectly observed in our data or if the firm places disproportionate weight on them. Interpreting our results as evidence of miscalibration therefore requires the implicit assumption that such tail risks are either small or adequately reflected in the outcomes we measure.

A related concern is *reputational risk*. Even in the absence of realized negative events, firms may incur costs if clients who have preferences over interacting with providers with criminal records learn that providers with criminal records are active on the platform, and negatively update their beliefs about platform quality. Such effects operate through perceptions rather than realized performance and may not be captured by observed engagement or safety outcomes. To the extent that the firm places weight on these reputational considerations, optimal screening may rationally exceed what would be implied by average realized surplus alone.

Finally, our interpretation focuses on the firm's private objective and abstracts from broader welfare considerations. Expanding access to work for individuals with criminal records may generate positive externalities if employment reduces recidivism, as suggested by existing research (Denver et al., 2017; Yang, 2017; Schnepel, 2018). At the same time, consumers may have preferences over interacting with providers with criminal records, and reduced

transparency may affect their welfare. Incorporating these forces would require a broader welfare framework beyond the scope of this analysis.

5 Data

The data we have come from both the background check company and the online marketplace company and are linked based on a unique identifier for the applicant who consented to the background check.

5.1 Data: Criminal Background Checks

The data from the background check company includes, importantly, the applicant's full criminal record (except charges not legally reportable due to FCRA or state-specific versions; see Agan et al. (Forthcoming) for more details on these regulations). Information about the criminal history includes, for each legally reportable found charge: the charge disposition (conviction, pending, alternative adjudication, dismissed, etc.); the date of the charge; the charge type (in 10 categories and 55 subcategories); the level (felony, misdemeanor, or petty offense); the assigned matrix outcome (visible or hidden); and the adjudication outcome (whether the applicant was issued an "adverse action"). We know whether the applicant was assigned to treatment or control visibility matrix and for each charge (though this was not visible to adjudicators at the marketplace company). We also know the matrix rules for both treatment and control matrices, so we can create counterfactual visibility for each applicant had they been assigned to a different visibility matrix.

Unfortunately, we do not have information on the race, age, gender, or other characteristics of the applicants. To be fair, the adjudicators are also not directly given this information when they are making their decisions either. However, incidentally background checks do have names and dates of birth on them, which can be used to proxy for race and gender, and to calculate age. Thus while we do have full information about the criminal records that the adjudicators have (plus any legally reportable charges that are hidden by the visibility rules), we do not have everything that might be in their information set at the time the make the decision.[4]

[4]We are working with the background check company to potential add race and gender predictions from names, which we do not have direct access to, in future iterations of this data.

5.2 Data: Marketplace Company Outcomes

For each applicant to the marketplace company we obtain information on their outcomes on the platform. We have this data from July 24, 2023 (the first day of the experiment) to September 1, 2025 (1 year after the end of the experiment), which means we have at least one year of outcome data for all applicants. In our analysis, we measure outcomes for each individual for the 12 months after their background check is completed. Conditional on being allowed on the platform, we have: the existence and date of each time a service provider reached out to a potential client; the star rating of each review left for the service provider (but not the text of any review); and the number and type of incident reported to the platform, if any.

We do not have direct information on whether the client and the provider actually contracted about a task; we do know whether a review was left, which can be a proxy for a contracted task, but a client could contract with a service provider to complete a task and choose not to leave a review. Nor do we know the ultimate price paid by the client in cases where the contracted task was carried out. We do not have consent from clients to access the content of their private conversations to identify when matches were finalized, and the marketplace company does not collect this information in any other way. In addition, most payments happen off of the platform itself, and as a result we cannot observe that behavior. We also know that service provider can import reviews for tasks that were done outside of the platform, and we have information on both review that originated off-platform and those that originated on the platform. Our analysis will mainly focus on "verified" reviews (those attached to usage on the marketplace).

6 Sample Criminal Record Characteristics and Balance

Our sample consists of 253,193 individuals for whom a background check was run between July 24, 2023 and September 1, 2024. 50.15% (126,976) were individually randomly assigned to the treatment matrix. Of these applicants, 14% had a legally reportable criminal record (whether visible to the adjudicators or not) which represents 35,370 individuals who could potentially be impacted by the experimental matrix.[5] In the last two weeks of December 2023 some data anomalies occurred that we cannot fully explain, and thus these two weeks are dropped from the final analysis, leaving a final analysis sample of 246,057 applicants,

[5]Though as described in Section 3, not everyone with a record is actually impacted. The "always clear" group whose criminal records are clear under the control matrix and the "same visibility" are not impacted.

of which 34,553 have a legally reportable criminal record (14%). A full accounting of this data anomaly is available in Appendix B. Those 34,553 with a record will serve as the main sample for our analysis.

Figure 2 shows the number of applications over time by whether applicants have any criminal charges on their record. Application volume is relatively stable over the experimental period, with modest month-to-month variation but no clear trends or discontinuities. Applicants with criminal records make up a consistent share of total applications throughout the period. There is no evidence of differential trends in application behavior by charge status that would coincide with the timing of the experiment. This stability suggests that the composition of applicants by charge status is not changing in ways that would confound comparisons across treatment and control.

Table 2 shows a balance table of some of the characteristics of applicants' full criminal background records both overall (including 0s for anyone without a record) and for record characteristics conditional on having any charges. We note the fidelity of the randomization: approximately 50% of applicants are assigned to the treatment matrix and the criminal record characteristics are all very similar between treatment and control. p-values on F-tests of the null that all listed variables are jointly equal across treatment and control fail to reject that null across panels.[6] In Appendix Tables D.1-D.4, we also break these down into treatment response types (i.e. always clear, newly clear, reduced visibility, and same visibility) among those with charges, and find no significant differences between treatment and control applicants within these subgroups.

Among applicants with criminal records, individuals have multiple charges on average (roughly four) spread across several distinct charge dates, indicating that records are not typically one-off events. Nearly all applicants with records have at least one subfelony (misdemeanor or petty) charge, while about 30 percent have at least one felony. Conditional on having a record, applicants fall into all four treatment response types, with most applicants with records affected by the treatment: 32% have records that are newly clear by the treatment matrix and 17% have records with reduced charge visibility.

[6]Two characteristics have a $p < 0.05$, though neither magnitude represents an economically significant difference. The control group is slightly more likely to have a felony (31% vs 30% in the treatment group); and the control group is slightly more likely to have a drug charge (39% vs 37.8% in the treatment group).

7 Overall Results of the Experiment

The experiment hides some criminal records from adjudicators, potentially allowing more individuals with records access to the platform. To interpret its effects, in this section we first examine how treatment assignment affects admission to the platform. Later in Section 8 we will return to results for marginal applicants.

7.1 First Stage: Allowance onto the Platform

How does being assigned to a more relaxed adjudication matrix impact the probability you get access to the marketplace?

Let: Z_i = assignment to treatment matrix; D_i = indicator for being allowed on platform, the first-stage relationship is

$$D_i = \delta + \pi Z_i + \nu_i$$

Table 3 displays results of these regressions. Among applicants with any charge (N = 34,553), treatment increases the probability of being allowed on the platform by 3.3 percentage points from a baseline of 67 percent allowance in the control group (a 5% increase). This represents approximately 1140 additional applicants with records allowed access to the marketplace due to the experiment who would not have been allowed otherwise.

The next few columns break down this first stage relationship by the treatment response types defined in Section 3. Column (2) shows that among the group whose visible criminal records are already completely clear in control (always clear), 99.9% of them were already being allowed, and there is no room for treatment to have an impact.[7] In Column (5), the same visibility group is similarly not impacted by treatment as their visible criminal records remain the same under both matrices—about 19% of the applicants were being allowed on the marketplace in control and treatment has no statistically significant impact on this allowance rate, as expected. Similarly, in Column (6), those with no charges see no treatment effect, as expected.

The newly clear and reduced visibility groups include potential compliers with the treatment matrix are (i.e. those whose visible criminal records differ under the treatment and control matrices). In Column (3), we see that among those who are newly clear due to being assigned to treatment (meaning their visible criminal records under the control matrix had only these

[7] Rarely, individuals are not allowed on the platform due to other non-criminal screenings, such as identity verification.

marginal charges), this group was already being allowed at a very high rate of 95%. The treatment brings that up to 99.9% as the allowance is now essentially automatic. We might call these "computer" or "automatic" compliers—while under the control matrix, a human adjudicator would look at the record and make a decision, under the treatment matrix they are all essentially auto-allowed by the algorithm. They have a very strong first-stage effect of treatment on allowance to the marketplace ($F = 281$) In Column (4), we see that among those whose visible criminal records are reduced but not completely cleared, they similarly see an increase in the probability of allowance, though not as large: a 4.1pp increase over a baseline of 23% allowance ($F = 13$). We might call these "human compliers"—in both treatment and control, a human adjudicator makes the decision about allowance versus adverse action. So the "action" of the experiment is mainly taking place in the newly clear and reduced visibility groups.

7.2 Reduced Form Effects

We estimate reduced form effects of being assigned to the treatment matrix on outcomes on the platform via

$$Y_i = \kappa + \rho Z_i + \eta_i$$

Where Y_i are various platform outcomes, such as making any contact with clients or any incident reported to the platform. Z is random assignment to the treatment matrix as defined previously. These results combine potential impacts of treatment on being allowed on the platform and usage conditional on being allowed.

Table 4 displays these results for unconditional outcomes: any contact, review, or incident, as well as how many days someone was on the platform (measured from date the background check was completed to date of the last client contact we have recorded). Overall they imply that, if anything, the treatment group is more likely to use the platform and has a lower incident rate. The treatment group also tends to stay longer in the marketplace. These effects on engagement are concentrated in the groups actually affected by treatment (the newly clear and reduced visibility groups). Columns (2)-(5) break this down by the treatment response types. Recall that for the always clear and same visibility groups their visible criminal records are not impacted by treatment, and indeed we see little difference in outcomes between treatment and control for these group.[8] The two main compliers groups see more engagement with a higher probability of contact and reviews in treatment.

[8]With the exception of an apparent reduction in incidents for the always clear group in treatment, significant at the 10% level. More on this follows.

While Table 4 shows how the treatment groups differ from the control group on extensive margin platform outcomes on average, Table 5 displays similar results for intensity of platform usage and other conditional outcomes. Because these outcomes are defined conditional on engagement, random assignment no longer guarantees balance in the sample used for estimation; these estimates should therefore be interpreted as descriptive. Even so, they are informative about behavior on the platform among active users. There are not many statistically significant or economically meaningful differences in reviews, incidents, or ratings conditional on platform engagement.[9]

Overall, we interpret these results as suggesting that the treatment and control groups, as a whole, perform very similarly on the platform with the treatment group, if anything, performing slightly better in terms of engagement or incidents.

7.3 Effects Conditional on being Allowed on the Platform

The reduced form effects described in Section 7.2 are unconditional and include zeros for applicants not allowed on the platform. As a result, the control means combine selection into the platform with differences in outcomes among those who participate. Table 6 instead restricts attention to applicants who are allowed on the platform and compares outcomes for those without charges to those with charges in the control group (and by treatment response type). This provides a descriptive view of how individuals with records perform conditional on admission.

We emphasize that these comparisons are observational. We do not observe key characteristics such as demographics, age, education, or geography, and thus cannot adjust for differences in underlying applicant composition. These results should therefore not be interpreted as causal, but rather as descriptive benchmarks.

Overall, applicants with records perform worse along several dimensions. They are less likely to contact clients (64.4% vs. 71.7%) and less likely to receive reviews (19.7% vs. 21.9%). At the same time, they are more likely to experience incidents (2.5% vs. 1.8%). This increase

[9]The Always Clear group in Column 2 is effectively a placebo, as they are not impacted by treatment, yet some coefficients appear statistically significant (ln(contacts), ln(reviews), and any incident), consistent with sampling variation. Number of contacts is a fat-tailed distribution and in the always clear group there are more outliers in the control group (Appendix Figure C.1b). When applying a rank test or restricting results to contacts less than the 95th percentile (Appendix Figure C.1a), there are no longer statistically significant differences in contacts for the treatment group in the always clear subgroup. Similarly, quantile regressions at the median and 75th percentile show no evidence of differences between treatment and control in this group. Reviews and incidents appear to flow downstream of this imbalance in contacts, as seen in Table 5, reviews or incidents per contact are not significantly different. This also explains why we saw a significant decrease in incidents in Table 4. Appendix C explores this further.

appears to be driven primarily by work-related incidents (1.8% vs. 1.3%) rather than criminal incidents, which are both rare and quite similar across groups (0.5% vs. 0.4%). There are also meaningful differences across treatment response types. Applicants in the "reduced visibility" and "same visibility" groups exhibit substantially lower engagement (e.g., contact rates around 0.54) and higher incident rates, while the "always clear" and "newly clear" groups look more similar to each other. This suggests heterogeneity in underlying applicant quality across different parts of the criminal record distribution, even conditional on being admitted.

Table 7 reports conditional outcomes, restricting to individuals who engage on the platform. The same qualitative patterns remain but are attenuated. Applicants with records have fewer levels of contacts and reviews, and slightly higher incident rates per interaction. However, differences in quality measures are modest. For example, average ratings are very similar (4.48 vs. 4.59), and conditional incident rates remain low in absolute terms. This suggests that much of the gap in unconditional outcomes operates through differences in participation and engagement rather than large differences in performance conditional on activity.

8 Identifying Outcomes for Marginal Applicants

The reduced form results suggest that applicants assigned to treatment do not perform worse on the platform. However, these estimates average over all applicants, including a relatively small share of compliers (5.2% in the newly clear group and 3.5% in the reduced visibility group). In addition, these comparisons include both applicants who are allowed on the platform and those who are not, for whom outcomes are mechanically equal to zero.

To isolate the outcomes of interest, we adopt the LATE framework of Imbens and Angrist (1994). This framework allows us to characterize the applicants at the margin of the screening rule—those who would be denied access under the control policy but are allowed under treatment—and to estimate their outcomes when admitted. A key question is whether these marginal applicants perform differently from always takers, who are admitted under both policies. In our setting, compliers correspond to applicants at the margin of the firm's screening threshold.

Table 8 summarizes the standard compliance types for a binary instrument Z (treatment assignment) and binary treatment D (platform access). Given random assignment, the characteristics of always takers and never takers are directly observed:

$$E[X \mid Z = 0, D = 1] \quad \text{and} \quad E[X \mid Z = 1, D = 0].$$

Complier characteristics can be recovered using the first stage and standard IV weighting arguments (equivalently, via 2SLS regressions of $X_i \times \mathbb{1}(D_i = 1)$ on Z_i; see Abadie 2003).

The same approach identifies outcomes for compliers. Letting $D(z)$ and $Y(d)$ be standard potential outcomes notation for treatment (when $Z = 0$ or $Z = 1$) or outcomes (when $D = 0$ or $D = 1$):

$$E[Y(1) \mid D(1) > D(0)].$$

In our setting, this corresponds directly to the 2SLS estimate from

$$Y_i = \alpha + \beta D_i + \varepsilon_i,$$

instrumenting D_i with Z_i. Because $Y_i(0) = 0$ by construction for applicants not allowed on the platform, the IV estimand recovers the average outcome for compliers when admitted.

8.1 LATE Assumptions

Interpreting the estimates above requires the standard LATE assumptions (Imbens and Angrist, 1994):

1. **Independence** ($Z \perp (Y_i(0), Y_i(1), D_i(0), D_i(1))$): This follows from random assignment. Balance on observable criminal record characteristics is shown in Tables 2 and D.1-D.4.

2. **Exclusion** ($Y_i(D_i(Z_i), Z_i) = Y_i(D_i(Z_i))$): Treatment affects outcomes only through platform access. This is plausible in our setting because neither adjudicators nor applicants are aware of treatment assignment, and the intervention operates solely by altering the visibility of criminal records used in the admission decision.

3. **Relevance** ($Cov(Z_i, D_i) \neq 0$): The first stage is reported in Table 3. Treatment significantly increases the probability of platform access, particularly in the newly clear group, where the first stage is large by construction.

4. **Monotonicity** ($\forall i : D_i(1) \geq D_i(0)$): Treatment weakly reduces the set of visible charges and therefore (weakly) increases the probability of admission. In the newly clear group, monotonicity holds mechanically, as treatment clears all visible charges and bypasses adjudication. In the always clear and same visibility groups, treatment

does not change the visible record, implying no change in admission decisions. In the reduced visibility group, treatment removes a subset of charges, weakening the signal observed by adjudicators and thereby (weakly) increasing the probability of admission.

8.2 Who are the Compliers?

The experiment allows some applicants who would have been denied access under the control matrix to instead be admitted to the platform. These compliers are an objects of interest. Who were adjudicators excluding under the status quo? What do their criminal records look like?

Table 9 reports characteristics of compliers, always takers, and (where relevant) never takers, using the approach described above. We present results for the full sample of applicants with charges, as well as separately by the treatment response types with first stage (with compliers): the newly clear and reduced visibility groups.

Compliers have moderately more severe records than always takers, but substantially less severe records than never takers. In the overall sample, never takers have on average 6.3 charges, compared to 4.7 for compliers and 2.9 for always takers. This pattern holds across most dimensions: compliers are more likely to have felony, drug, and pending charges than always takers, but less likely than never takers.

There are also meaningful differences across treatment response types. In the newly clear group, compliers are substantially more likely to have felony or drug charges, suggesting that these charges play an important role in triggering exclusion by humans under the control matrix. When these charges are fully hidden under treatment, these applicants are automatically admitted. In contrast, compliers in the reduced visibility group tend to have more extensive criminal histories—including more convictions and more charges overall—suggesting that adjudicators respond to the cumulative number and severity of records. When some, but not all, charges are hidden, these applicants move across the admission threshold.

Comparing compliers across these two groups is also informative. "Newly clear" compliers are automatic compliers and have fewer charges, fewer convictions, are less likely to have violent offenses, and have older records than compliers in the reduced visibility group (where a human makes the decision). In contrast, "reduced visibility" compliers tend to have more recent and more extensive records. This variation provides additional leverage for understanding which dimensions of criminal history drive screening decisions and how these

dimensions relate to subsequent outcomes.

Overall, the complier analysis suggests that adjudicators are not making arbitrary decisions. Criminal record characteristics clearly matter for screening: applicants with more severe records are less likely to be admitted. At the same time, the experiment induces variation precisely along these dimensions, allowing some applicants with more severe records than the average admitted worker to enter the platform. This variation provides leverage to study how and whether these criminal records map into realized outcomes.

8.3 Average Outcomes for Marginally Allowed Applicants

We now turn to outcomes for compliers—the marginal applicants whose admission status changes under treatment. We distinguish between "newly clear" (automatic) and "reduced visibility" (human) compliers, reported in Table 10.

For the newly clear group, point estimates suggest that compliers are at least as engaged as always takers, and if anything slightly more so. For example, the complier mean for any contact exceeds that of always takers, and there is no evidence of higher incident rates. Estimates for complier means for incident outcomes are imprecise and occasionally negative. While the instrument is strong in this group ($F = 298$), the first stage is still modest in levels (around 5 percentage points), so small reduced form differences translate into noisy IV estimates. The fact that some complier means fall outside the natural range of the outcome (e.g., any contact > 1) likely reflects this noise and the linear IV approximation rather than economically meaningful effects outside of 0 and 1.

Overall, the key takeaway is that these "newly clear" compliers do not perform worse—either in engagement or safety outcomes—than the always takers the platform was already admitting.

A similar pattern emerges for the reduced visibility ("human") compliers. Point estimates are generally comparable to, and in some cases slightly better than, those for always takers. There is no evidence of reduced engagement or increased incidents among marginally admitted applicants, though estimates are again imprecise.

Table 11 reports conditional outcomes among active users. The same qualitative patterns hold. Differences in engagement intensity are small, and conditional incident rates remain low. For example, incidents per contact and per review are similar across groups, and average ratings are nearly identical.

Taken together, these results show no statistically significant decline in engagement and no increase in safety-related outcomes for marginal applicants. If anything, point estimates for engagement are slightly positive. The evidence therefore suggests that marginally admitted workers perform similarly to those the platform was already admitting.

9 Discussion

We study the effects of hiding certain criminal records for randomly selected applicants from human adjudicators in a large online marketplace. The experiment increases access to the platform among applicants with criminal records. Despite this increase in admission, we find no evidence of worse outcomes among those assigned to treatment. On average, treated applicants are at least as engaged on the platform and do not experience higher rates of incidents. Using a LATE framework, we show that marginally admitted applicants, e.g. those who are excluded under the control policy but admitted under treatment, perform similarly to workers already admitted onto the platform.

These findings contrast with cross-sectional comparisons, where applicants with criminal records perform worse on average. Taken together, the results suggest that while criminal records may be correlated with engagement and incidents, the information used to exclude marginal applicants is not strongly predictive of realized performance in this setting. If adjudicators were optimally setting screening thresholds based on correct beliefs about expected benefits and costs, marginal applicants should have lower net value. Instead, we find no evidence of reduced engagement or increased incidents among marginally admitted workers, suggesting that screening thresholds may be overly strict at the margin.

One explanation is that adjudicators overweight salient features of criminal records, such as charge type, severity, or recency, relative to their true predictive content. This could be because adjudicators lack feedback on the outcomes of rejected or applicants, limiting their ability to learn about performance at the margin and update screening rules. More broadly, when screening relies on high-dimensional and heterogeneous signals, even informative predictors in cross-sectional data may be weakly predictive at the margin relevant for decision-making.

At the same time, our results do not imply that criminal records are uninformative or that screening is unwarranted. Applicants with criminal records perform worse on average, which may partially reflect correlated unobservables. The key distinction is between average differences and marginal decision-making: the applicants excluded at the margin appear

similar, in terms of realized outcomes, to those who are admitted.

Our estimates focus on average outcomes. Firms may rationally place weight on tail risks—rare but high-cost events—or on reputational concerns that are not fully captured by observed engagement or incident measures. To the extent that such risks are important, optimal screening thresholds may be stricter than what would be implied by average realized outcomes alone. Even so, our results suggest that modest expansions in access would not worsen typical performance outcomes.

While our results are specific to one online marketplace, they provide causal evidence on how criminal record information is used in practice and how it maps into realized outcomes. Future work should examine how these patterns generalize across settings and explore how firms can better align screening decisions with realized productivity and risk.

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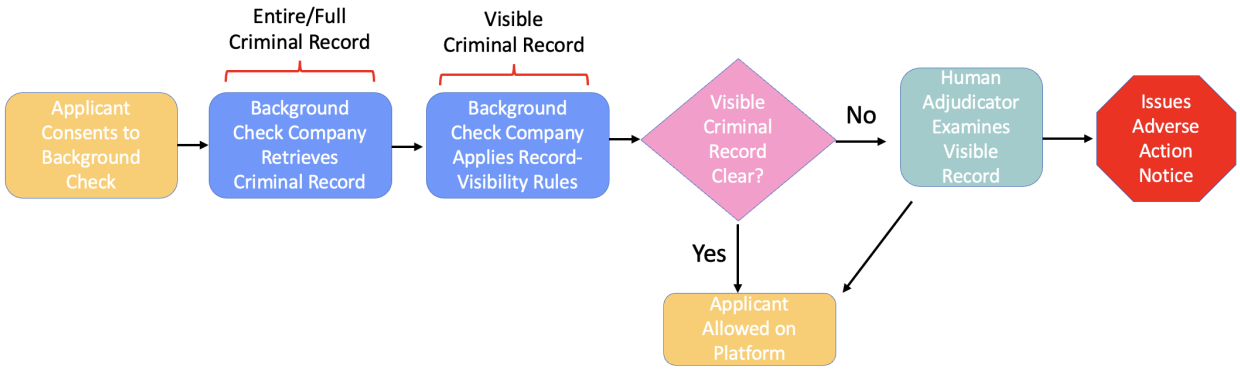
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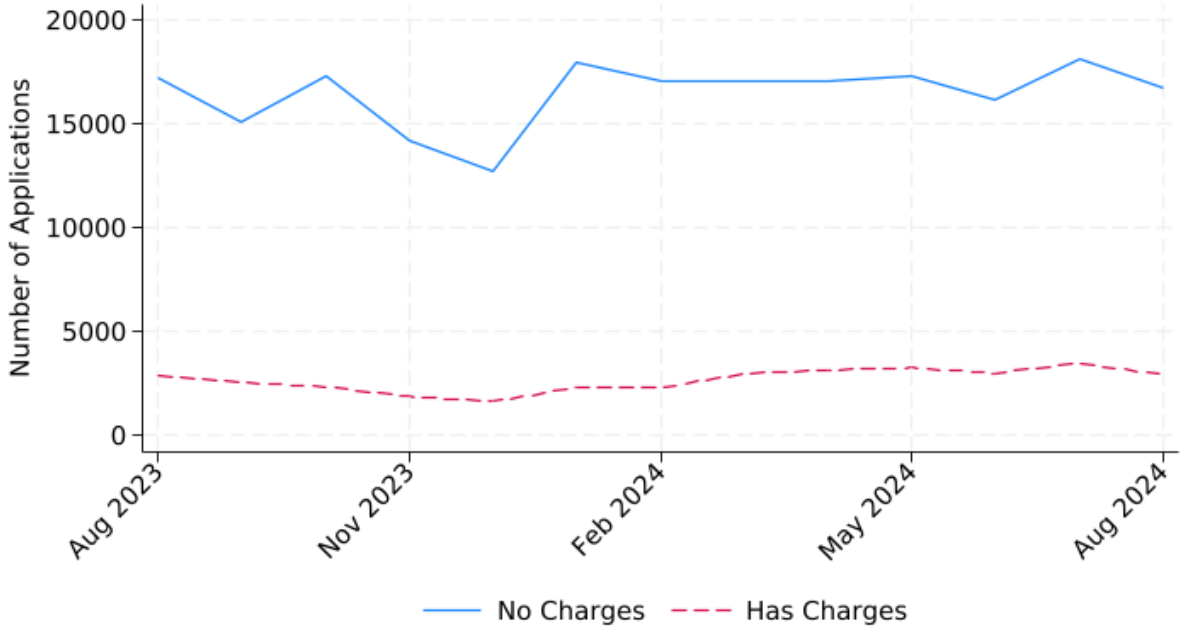
Figures and Tables

Figure 1: Criminal Background Check Process



Note: This figure shows a simplified flowchart detailing the criminal background check process.

Figure 2: Application Volume over Time by Charge Status



Note: The figure shows the monthly volume of background check reports run for applicants to the platform over the course of the experimental period, broken out by whether the applicant has a legally reportable criminal charge on their record.

Table 1: Marginal Changes to Treatment Rule-Based Visibility Matrix

Charge Type	**Subtype**	**Level**	**Threshold**		**# Charges**
			C	**T**	**Newly Hidden**
Vehicles & Traffic	License & Registration	Misd	3	0	11,730
Vehicles & Traffic	Unsafe Operation	Misd	7	0	7,848
Drugs & Alcohol	Driving Under the Influence (DUI)	Misd	7	0	5,743
Vehicles & Traffic	Speeding	Misd	3	0	3,496
Vehicles & Traffic	Vehicle Equipment	Misd	3	0	1,707
Criminal Intent	Court Orders	Misd	7	3	1,626
Drugs & Alcohol	Drugs - Possession/Use	Felony	7	3	1,555
Criminal Intent	Obstruction	Misd	7	3	1,321
Drugs & Alcohol	Driving Under the Influence (DUI)	Felony	7	0	476
Theft & Property	Vandalism & Mischief	Misd	7	3	394
Violence	Disorderly Behavior	Misd	7	3	185
Drugs & Alcohol	Marijuana Possession/Use	Felony	7	3	129
Criminal Intent	Conspiracy	Misd	7	3	62
Criminal Intent	Criminal Tools	Misd	7	3	48
Criminal Intent	Accessory	Misd	7	3	22
Criminal Intent	Organized Crime	Misd	7	3	11
Drugs & Alcohol	Alcohol & Tobacco	Felony	7	3	1

Note: Categories and subcategories of charges whose thresholds differ between the control and treatment matrix are listed above. The threshold is measured in years since the charges adjudication and indicates how long a certain type of charges remains visible, e.g. 7 means charges 7 years and younger at the time of the background check are shown, but charges older than 7 years are not. They are sorted by the number of charges affected during the experimental period. The full control rule-based visibility matrix can be found in Appendix A.

Table 2: Balance and Sample Criminal Record Characteristics

	Mean (T)	Mean (C)	p-value
Full Sample			
Any criminal charge	0.141	0.140	0.305
Number of charges	0.564	0.563	0.959
At least one felony	0.300	0.310	0.045
At least one subfelony	0.989	0.988	0.832
At least one pending charge	0.166	0.159	0.062
At least one violent	0.245	0.252	0.175
At least one property	0.224	0.228	0.310
At least one drug	0.378	0.390	0.023
Number of felonies	0.118	0.120	0.613
Number of convictions	0.455	0.458	0.693
Always Clear	0.039	0.038	0.123
Newly Clear	0.046	0.045	0.117
Reduced Visibility	0.024	0.024	0.269
Same Visibility	0.033	0.033	0.590
Observations	123,343	122,714	
Joint p-value			0.182
Conditional on Any Charge			
Number of charges	3.996	4.033	0.491
Number of felonies	0.836	0.857	0.361
Number violent	0.426	0.439	0.275
Number property	0.493	0.490	0.833
Number drug	0.802	0.839	0.033
Years first to last charge	3.716	3.762	0.485
Years since last charge	4.732	4.756	0.698
Avg Charge Age (Years)	6.560	6.595	0.597
Num distinct charge dates	2.742	2.771	0.339
Always Clear	0.276	0.271	0.234
Newly Clear	0.325	0.319	0.229
Reduced Visibility	0.168	0.175	0.102
Same Visibility	0.230	0.235	0.259
Observations	17,409	17,144	
Joint p-value			0.393

Note: Each variable is based on the full criminal record (whether visible or not). Panel A includes 0s for anyone without a criminal record. The joint p-values report F-tests of the null that all listed variables are jointly equal across treatment and control groups within each panel, based on regressions of treatment assignment on the variables with robust standard errors. A subfelony is either a misdemeanor or petty offense.

Table 3: Impact of Treatment on Access to Marketplace (First Stages)

		Treatment Response Types				
	(1) Any Charge	(2) Always Clear	(3) Newly Clear	(4) Reduced Vis	(5) Same Vis	(6) No Charges
Treatment	0.033***	-0.001	0.050***	0.041***	0.006	0.000
	(0.005)	(0.001)	(0.003)	(0.011)	(0.009)	(0.000)
Constant	0.668***	0.999***	0.949***	0.232***	0.230***	0.999***
	(0.004)	(0.000)	(0.003)	(0.008)	(0.007)	(0.000)
N	34553	9447	11134	5927	8045	211504
Robust F	43.98	2.40	281.14	13.18	0.44	0.05

Note: Each column is a coefficient from a regression of being allowed on the platform on treatment assignment (not issued a post adverse action). Column (1) is restricted to anyone with a charge on their record (whether visible to adjudicators or not), Columns (2)-(5) condition on having any charges and are subgroups by treatment response types defined by charge visibility under treatment and control, Column (6) is a placebo test among people without any charges on their record.
+ p<0.10, * p<0.05, ** p<0.01, *** p < 0.001

Table 4: Reduced Form Effects: Full Sample

		Treatment Response Types			
	(1) Any Charge	(2) Always Clear	(3) Newly Clear	(4) Reduced Vis	(5) Same Vis
Any Contact	0.025***	-0.010	0.055***	0.026**	-0.001
	(0.005)	(0.010)	(0.009)	(0.009)	(0.007)
Control mean	0.430	0.670	0.618	0.126	0.124
Any Review	0.008*	-0.011	0.023**	0.015**	-0.004
	(0.004)	(0.008)	(0.008)	(0.005)	(0.004)
Control mean	0.131	0.202	0.193	0.037	0.036
Any Incident	-0.003*	-0.006*	-0.002	-0.002	-0.002
	(0.001)	(0.003)	(0.003)	(0.002)	(0.001)
Control mean	0.017	0.024	0.024	0.008	0.005
Any Criminal Incident	-0.000	-0.001	-0.001	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Control mean	0.004	0.004	0.006	0.002	0.001
Any Work Incident	-0.002	-0.005	-0.000	-0.001	-0.002
	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)
Control mean	0.012	0.017	0.017	0.006	0.004
Days BGC to Last Contact	2.940*	-4.089	8.138**	3.556	-0.334
	(1.304)	(2.812)	(2.619)	(1.883)	(1.531)
Control mean	62.652	95.517	92.872	17.709	17.296
N	34,553	9,447	11,134	5,927	8,045

Note: Each row is a different regression; each column is a different conditioning set. The values represent coefficients from a regression of the outcome listed in the row on treatment; The control mean is the constant from that regression. All outcomes are measured for each individual for 12 months after the background check is completed. These results are not conditioned on being allowed, outcomes are filled in as 0s for anyone who was not allowed onto the platform.
+ p<0.10, * p<0.05, ** p<0.01, *** p < 0.001

Table 5: Reduced Form Effects: Conditional Outcomes

		Treatment Response Types			
	(1) Any Charge	(2) Always Clear	(3) Newly Clear	(4) Reduced Vis	(5) Same Vis
Log Num Contacts	-0.007	-0.073	0.020	0.109	-0.069
	(0.024)	(0.041)	(0.039)	(0.065)	(0.054)
Control mean	1.763	1.949	1.977	1.116	1.160
Log Num Reviews	-0.039	-0.124*	-0.027	0.256*	-0.060
	(0.033)	(0.055)	(0.049)	(0.109)	(0.101)
Control mean	1.194	1.273	1.275	0.773	0.869
Reviews per Contact	0.001	-0.000	0.002	0.005	-0.001
	(0.002)	(0.002)	(0.002)	(0.007)	(0.006)
Control mean	0.039	0.036	0.038	0.044	0.044
Incidents per Contact	-0.000	-0.001	-0.001	0.003*	-0.001
	(0.000)	(0.001)	(0.001)	(0.002)	(0.001)
Control mean	0.002	0.002	0.002	0.002	0.004
Incidents per Review	-0.001	-0.007	-0.001	0.001	0.013
	(0.004)	(0.006)	(0.006)	(0.013)	(0.012)
Control mean	0.032	0.032	0.032	0.041	0.029
Proportion Reviews 5*	0.003	-0.011	0.007	0.024	0.020
	(0.008)	(0.014)	(0.012)	(0.036)	(0.030)
Control mean	0.837	0.849	0.841	0.773	0.823
Proportion Reviews Low	-0.001	0.010	0.003	-0.044	-0.024
	(0.008)	(0.013)	(0.011)	(0.034)	(0.029)
Control mean	0.132	0.119	0.127	0.202	0.156
Average Rating	0.000	-0.044	-0.003	0.167	0.051
	(0.029)	(0.047)	(0.042)	(0.126)	(0.107)
Control mean	4.473	4.523	4.492	4.190	4.418

Note: Each row is a different regression; each column is a different conditioning set. The values represent coefficients from a regression of the outcome listed in the row on treatment; the control mean is the constant from that regression. Logs are reported conditional on $Y > 0$. Each regression is conditional on having at least contact or review for the "per" values; for the star rating values it is conditional on at least one star rating. Low ratings are defined as a review that is 3 stars or lower. All outcomes are measured for each individual for 12 months after the background check is completed.
+ p<0.10, * p<0.05, ** p<0.01, *** p < 0.001

Table 6: Average Outcomes For Applicants Allowed on the Platform by Charges and Treatment Response Types

		Always Takers (Z=0, D=1)				
	(1) No Charges	(2) Any Charges	(3) Always Clear	(4) Newly Clear	(5) Reduced Vis	(6) Same Vis
Any Contact	0.717	0.644	0.671	0.652	0.541	0.540
	(0.001)	(0.004)	(0.007)	(0.007)	(0.019)	(0.016)
[p (col = NoCharges)]		0.000	0.000	0.000	0.000	0.000
Any Review	0.219	0.197	0.203	0.204	0.159	0.155
	(0.001)	(0.004)	(0.006)	(0.006)	(0.014)	(0.012)
[p (col = NoCharges)]		0.000	0.006	0.007	0.000	0.000
Any Incident	0.018	0.025	0.024	0.026	0.036	0.023
	(0.000)	(0.001)	(0.002)	(0.002)	(0.007)	(0.005)
[p (col = NoCharges)]		0.000	0.007	0.000	0.011	0.328
Any Criminal Incident	0.004	0.005	0.004	0.007	0.007	0.004
	(0.000)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)
[p (col = NoCharges)]		0.036	0.990	0.018	0.302	0.840
Any Work Incident	0.013	0.018	0.017	0.018	0.024	0.017
	(0.000)	(0.001)	(0.002)	(0.002)	(0.006)	(0.004)
[p (col = NoCharges)]		0.000	0.011	0.004	0.043	0.274
Days BGC to Last Contact	111.252	93.800	95.600	97.899	76.174	75.140
	(0.314)	(1.278)	(2.029)	(1.916)	(4.796)	(4.193)
[p (col = NoCharges)]		0.000	0.000	0.000	0.000	0.000
N	211,504	11,451	4,634	5,191	697	929

Note: This table reports average outcomes for applicants who are allowed onto the platform, comparing individuals without criminal records to those with criminal records in the control group ($Z = 0, D = 1$). Reported are means by criminal record status and, for those with records, by treatment response types defined by how the visibility matrix affects their record (always clear, newly clear, reduced visibility, and same visibility). Robust standard errors are reported in parentheses. P-values in brackets test the null hypothesis that the mean outcome in each column is equal to that of applicants without criminal records (Column (1)). All outcomes are measured over the 12 months following completion of the background check.

Table 7: Average Outcomes For Applicants Allowed on the Platform by Charges and Treatment Response Types: Conditional Outcomes

		Always Takers (Z=0, D=1)				
	(1) No Charges	(2) Any Charges	(3) Always Clear	(4) Newly Clear	(5) Reduced Vis	(6) Same Vis
Log Num Contacts	2.091	1.952	1.949	2.004	1.718	1.797
	(0.004)	(0.019)	(0.030)	(0.029)	(0.080)	(0.071)
[p (col = NoCharges)]		0.000	0.000	0.003	0.000	0.000
Log Num Reviews	1.288	1.267	1.273	1.282	1.140	1.220
	(0.006)	(0.025)	(0.040)	(0.037)	(0.113)	(0.100)
[p (col = NoCharges)]		0.416	0.699	0.865	0.189	0.495
Reviews per Contact	0.035	0.038	0.036	0.039	0.042	0.040
	(0.000)	(0.001)	(0.002)	(0.002)	(0.005)	(0.004)
[p (col = NoCharges)]		0.007	0.442	0.020	0.143	0.262
Incidents per Contact	0.001	0.002	0.002	0.002	0.001	0.004
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
[p (col = NoCharges)]		0.001	0.055	0.032	0.834	0.059
Incidents per Review	0.020	0.033	0.032	0.032	0.053	0.032
	(0.001)	(0.003)	(0.004)	(0.004)	(0.015)	(0.010)
[p (col = NoCharges)]		0.000	0.004	0.002	0.029	0.216
Proportion Reviews 5*	0.871	0.837	0.849	0.841	0.750	0.798
	(0.001)	(0.006)	(0.009)	(0.009)	(0.036)	(0.029)
[p (col = NoCharges)]		0.000	0.022	0.001	0.001	0.011
Proportion Reviews Low	0.102	0.132	0.119	0.127	0.230	0.182
	(0.001)	(0.006)	(0.009)	(0.008)	(0.035)	(0.029)
[p (col = NoCharges)]		0.000	0.062	0.003	0.000	0.005
Average Rating	4.593	4.475	4.523	4.493	4.085	4.337
	(0.004)	(0.022)	(0.032)	(0.030)	(0.129)	(0.100)
[p (col = NoCharges)]		0.000	0.031	0.001	0.000	0.011

Note: This table reports average outcomes for applicants who are allowed onto the platform, comparing individuals without criminal records to those with criminal records in the control group ($Z = 0, D = 1$). Logs are reported conditional on $Y > 0$. Each regression is conditional on having at least contact or review for the "per" values; for the star rating values it is conditional on at least one star rating. Reported are means by criminal record status and, for those with records, by treatment response types defined by how the visibility matrix affects their record (always clear, newly clear, reduced visibility, and same visibility). Robust standard errors are reported in parentheses. P-values in brackets test the null hypothesis that the mean outcome in each column is equal to that of applicants without criminal records (Column (1)). All outcomes are measured over the 12 months following completion of the background check.

Table 8: Compliance Types by Treatment Assignment and Platform Access

	Treatment Assignment	
Platform Access	$Z = 0$ (Control)	$Z = 1$ (Treatment)
$D = 0$ (Not Allowed)	Never Taker / Complier	Never Taker
$D = 1$ (Allowed)	Always Taker	Always Taker / Complier

Table 9: Characteristics of (Full) Criminal Records by Compliance Type

	Overall			Newly Clear		Reduced Visibility		
	(1) AT	(2) C	(3) NT	(4) AT	(5) C	(6) AT	(7) C	(8) NT
Number of charges	2.944	4.726	6.265	2.792	3.663	7.197	10.042	9.584
At least one felony	0.115	0.382	0.703	0.040	0.549	0.280	0.412	0.719
At least one misdemeanor	0.806	0.656	0.792	0.748	0.599	0.915	0.814	0.952
At least one petty	0.250	0.249	0.137	0.368	0.086	0.359	0.198	0.268
At least one pending charge	0.110	0.239	0.285	0.156	0.248	0.294	0.239	0.369
At least one violent	0.122	0.181	0.527	0.033	-0.024	0.429	0.499	0.543
At least one property	0.116	0.148	0.473	0.049	0.004	0.253	0.334	0.518
At least one drug	0.337	0.455	0.462	0.331	0.687	0.491	0.415	0.676
Number of felonies	0.280	0.633	2.102	0.073	0.895	0.862	1.473	2.521
Number of convictions	2.502	3.210	4.829	2.246	1.855	5.595	7.979	7.469
Number violent	0.170	0.215	1.023	0.042	-0.055	0.650	0.820	1.148
Number property	0.201	0.491	1.145	0.072	0.287	0.515	1.452	1.486
Number drug	0.604	0.900	1.232	0.573	1.270	1.241	1.132	2.035
Years first to last charge	2.931	5.245	5.300	2.777	2.911	6.290	8.550	7.362
Years since last charge	5.624	2.616	2.975	2.212	3.620	1.728	1.720	1.544
Avg Charge Age (Years)	7.098	5.533	5.470	3.594	5.379	4.662	6.119	4.830
Num distinct charge dates	2.241	2.978	3.833	2.199	2.060	4.773	5.711	5.771

Note: Sample is restricted to applicants with charges. AT is Always Takers, C is Compliers, NT is Never Takers. Always takers means are averages for control applicants allowed on the platform. Never taker means are averages for treatment applicants not allowed on the platform. Complier means are calculated as described in Section 8.

Table 10: Average Outcomes By Treatment Response and Compliance Type

	Newly Clear				Reduced Visibility			
	(1) Always Takers (AT)	(2) Treatment (AT+C)	(3) Compliers (C)	(4) p (AT=C)	(5) Always Takers (AT)	(6) Treatment (AT+C)	(7) Compliers (C)	(8) p (AT=C)
Any Contact	0.652	0.674	1.087	0.014	0.541	0.556	0.639	0.545
	(0.007)	(0.006)	(0.177)		(0.019)	(0.018)	(0.161)	
Any Review	0.204	0.217	0.463	0.088	0.159	0.189	0.354	0.138
	(0.006)	(0.005)	(0.152)		(0.014)	(0.014)	(0.131)	
Any Incident	0.026	0.023	-0.034	0.298	0.036	0.024	-0.045	0.162
	(0.002)	(0.002)	(0.057)		(0.007)	(0.005)	(0.058)	
Any Criminal Incident	0.007	0.005	-0.018	0.386	0.007	0.005	-0.007	0.562
	(0.001)	(0.001)	(0.028)		(0.003)	(0.002)	(0.025)	
Any Work Incident	0.018	0.017	-0.008	0.595	0.024	0.019	-0.013	0.425
	(0.002)	(0.002)	(0.048)		(0.006)	(0.005)	(0.047)	
Days BGC to Last Contact	97.899	101.099	161.250	0.221	76.174	77.762	86.769	0.797
	(1.916)	(1.863)	(51.674)		(4.796)	(4.564)	(40.901)	
N	5,191	5,657	11,134		697	801	5,927	

Note: Columns (1)-(4) are restricted to the Newly Clear treatment response group; (5)-(8) the Reduced Visibility Group. Always taker outcomes are estimated among applicants in the control group ($Z = 0$) who are allowed on the platform ($D = 1$). Columns (2) and (6) are the average for the whole treatment group ($Z = 1$). And Column (3) and (7) estimate average outcomes for compliers via methodology in Section 8. Columns (4) and (8) are p-values testing the null that the mean for always takers and compliers is the same. Ns represent the number of observations used to estimate the value in the column; complier means are estimated from a 2SLS regression of outcomes on allowed using treatment as an instrument conditional on having charges and being in that treatment response group and hence the Ns are higher. Robust standard errors in parentheses.
+ p<0.10, * p<0.05, ** p<0.01, *** p < 0.001

Table 11: Average Outcomes Treatment Response and Compliance Type: Conditional Outcomes

	Newly Clear				Reduced Visibility			
	(1) Always Takers (AT)	(2) Treatment (AT+C)	(3) Compliers (C)	(4) p (AT=C)	(5) Always Takers (AT)	(6) Treatment (AT+C)	(7) Compliers (C)	(8) p (AT=C)
Log Num Contacts	2.004	1.998	1.924	0.873	1.718	1.884	2.612	0.148
	(0.029)	(0.027)	(0.501)		(0.080)	(0.081)	(0.614)	
Log Num Reviews	1.282	1.247	1.114	0.439	1.140	1.284	1.612	0.277
	(0.037)	(0.033)	(0.215)		(0.113)	(0.094)	(0.420)	
Reviews per Contact	0.039	0.041	0.067	0.331	0.042	0.051	0.092	0.236
	(0.002)	(0.002)	(0.029)		(0.005)	(0.006)	(0.041)	
Incidents per Contact	0.002	0.002	-0.007	0.319	0.001	0.004	0.020	0.216
	(0.001)	(0.000)	(0.009)		(0.000)	(0.002)	(0.015)	
Incidents per Review	0.032	0.031	0.026	0.819	0.053	0.034	-0.009	0.244
	(0.004)	(0.004)	(0.027)		(0.015)	(0.010)	(0.051)	
Proportion Reviews 5*	0.841	0.847	0.874	0.532	0.750	0.809	0.942	0.147
	(0.009)	(0.008)	(0.052)		(0.036)	(0.025)	(0.127)	
Proportion Reviews Low	0.127	0.129	0.140	0.790	0.230	0.157	-0.009	0.074
	(0.008)	(0.008)	(0.049)		(0.035)	(0.024)	(0.129)	
Average Rating	4.493	4.490	4.477	0.936	4.085	4.366	5.074	0.073
	(0.030)	(0.029)	(0.189)		(0.129)	(0.089)	(0.536)	

Note: Columns (1)-(4) are restricted to the Newly Clear treatment response group; (5)-(8) the Reduced Visibility Group.. Always taker outcomes are estimated among applicants in the control group ($Z = 0$) who are allowed on the platform ($D = 1$). Column (2) and (6) is the average for the whole treatment group ($Z = 1$). And Column (3) and (7) estimate average outcomes for compliers via methodology in Section 8. Columns (4) and (8) are p-values testing the null that the mean for always takers and compliers is the same. Robust standard errors in parentheses. Logs are reported conditional on $Y > 0$. Each regression is conditional on having at least contact or review for the "per" values; for the star rating values it is conditional on at least one star rating. Low ratings are defined as a review that is 3 stars or lower.
+ p<0.10, * p<0.05, ** p<0.01, *** p < 0.001

Appendix

A Description of Rule-Based Visibility Matrices

The company's pre-experiment and control list of hidden charges is below. The columns show the year thresholds at or above which a charge with a particular severity would be hidden. For example, an animal cruelty petty offense would be hidden if it had been 7 or more years since that charge at the time of the background check. Where there is no number (such as under violent felonies), these offenses would never be hidden, regardless of the time since the charge. For example, homicide charges are never hidden.

Table A.1: Pre-Experiment and Control List of Hidden Charges

Charge Category	Charge Subcategory	Year Threshold		
		Petty Offense	**Misdemeanor Offense**	**Felony Offense**
Violence	Animal Cruelty	7	7	—
	Disorderly Behavior	7	7	—
	Weapons & Endangerment	7	7	—
	Abduction & Restraint	7	7	—
	Harassment & Threats	7	7	—
	Child & Elder Abuse	7	7	—
	Assault & Battery	7	7	—
Vehicles & Traffic	Unsafe Operation	7	7	7
	Parking	0	0	7
	Vehicle Equipment	3	3	7
	License & Registration	3	3	7
	Speeding	3	3	7
Theft & Property	Trespassing	0	0	7
	Vandalism & Mischief	7	7	7
	Possession of Stolen Property	7	7	7
	Petty Theft	7	7	7
	Theft	7	7	7
	Arson	7	7	—
	Burglary	7	7	7
	Robbery	7	7	—
Statutory	Miscellaneous Citations & Violations	0	0	7
	Gambling	0	0	7
	Animal Ordinances	0	0	—
	Fish & Game	0	0	7
	Safety & Zoning	0	0	7
	Public Nuisance	0	0	7
	Custody & Support	0	0	7
Sexual	Prostitution	7	7	—
	Sexual Abuse	7	7	—
	Lewd Behavior	7	7	—

Charge Category	Charge Subcategory	Year Threshold		
		Petty Offense	**Misdemeanor Offense**	**Felony Offense**
Security	Treason	7	7	—
	Terrorism	7	7	—
	Immigration	7	7	—
Homicide	Unintentional Killing	—	—	—
	Attempted Homicide	—	—	—
	Intentional Homicide	—	—	—
Fraud & Deception	Business & Tax	7	7	7
	Worthless Check	7	7	7
	Forgery	7	7	7
	Identity Theft & Impersonation	7	7	7
	Bribery & Corruption	7	7	7
	Cyber Crimes	7	7	7
	Embezzlement	7	7	7
	Fraud	7	7	7
Drugs & Alcohol	Driving under the Influence (DUI)	7	7	7
	Marijuana Possession/Use	0	0	7
	Alcohol & Tobacco	0	0	7
	Drugs-Sale & Manufacture	0	0	7
	Drugs-Possession/Use	0	0	7
Criminal Intent	Court Orders	7	7	7
	Obstruction	7	7	—
	Organized Crime	7	7	—
	Criminal Tools	7	7	7
	Accessory	7	7	—
	Conspiracy	7	7	—

B Description of December 2023 Anomaly

B.1 Description of the Issue

In the last two weeks of December 2023, there are data oddities affecting applicants with records theoretically unaffected by treatment. Specifically, the share of individuals with no charges hidden under the control matrix is higher in the control group (49% of candidates with charges) than in the treatment group (42% of candidates with charges) in this two-week period.

Additionally, those with no difference in the visibility of their record between the control and treatment matrices are more likely to be issued an adverse action notice in the control group during this same period. This group of 54 applicants in the control group was issued adverse action notices at a rate of 94% versus 63% among the 49 applicants in the treatment group.

A similar phenomenon is visible, but with smaller magnitudes and only borderline significance, for individuals with some charges hidden in the control matrix and the same visible charges under the treatment matrix.

B.2 Ruling Out Potential Explanations

We confirm that there is general treatment and control group balance across covariates during this time. Visible records look qualitatively similar, showing no significant differences in number of charges or severity of charges across several metrics in Table B.1.

We check these two weeks against other placebo two-week periods in the months before and after this window, and find no other significant differences, in Figure B.1 and in Table B.2. The two-week anomaly period in late December 2023 is a rare event relative to nearby placebo windows. Again, this suggests a temporary shock confined to this period of time rather than a stable structural change differentially affecting treatment and control.

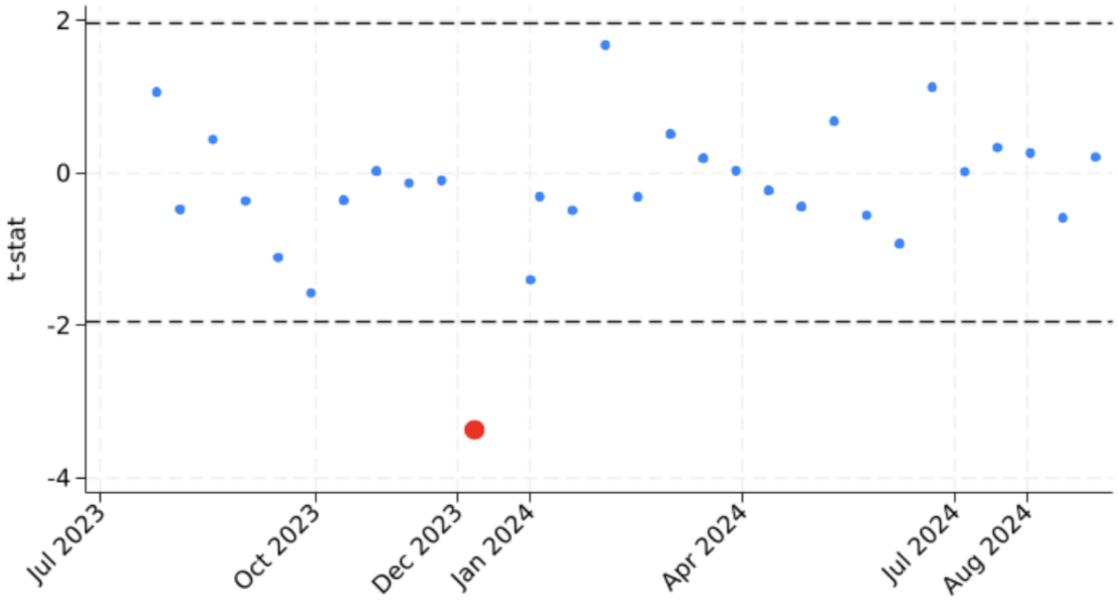
We also investigate whether this can be explained by either assignment to particular adjudicator(s) or odd behavior limited to a specific adjudicator, conditional on assignment, which also does not seem to be the case. Adjudicator assignment probabilities and adverse action probabilities conditional on adjudicator are in Table B.3. Within the adjudicators working on the holiday period reports, adverse action rates are higher for both adjudicators in the control group relative to the treatment group.

One potential explanation we cannot currently directly test for is some kind of adjudication

backlog after the holidays affecting the groups differentially (per the company, this kind of backlog is a common occurrence around winter holidays). However, in order for this to result in adjudication differences across treatment and control groups, the backlog list would have to be sorted somehow on treatment/control matrix assignment, leading to sequencing effects. This may be theoretically possible, though unlikely, due to the user interface looking exactly the same for treatment and control assigned applicant; however, on the back end, there are tags that differentiate between treatment and control assignment for the researchers' use. Given that this does not seem to have happened in any other time period, it would have to interact with the backlog to make sense as an explanation. Unfortunately, we don't have any data that capture adjudication timing, so we cannot test for this directly.

Since we cannot account for a singular explanation for the anomaly with the information we currently have access to, and since we have no reason to believe there were similar issues in any other period of the data, we have decided to drop this two-week period from the main analysis for all groups.

Figure B.1: Placebo 2-Week Window T-Statistics of the Treatment Effect on Platform Allowance



Notes: Each dot represents a t-statistic from a 2-week window, testing the null that P(Allowed | Treatment)=P(Allowed | Control) for the Same Visibility group.

Table B.1: Covariate Balance in Same Visibility Groups by Time Period

Group	Period	Metric	Mean C	Mean T	Diff (T-C)	P-value	N C	N T
NCH	Non-Anomaly	N reports	2105	2119	14	0.829	2105	2119
NCH	Non-Anomaly	N charges	1.955	1.954	-0.001	0.986	2105	2119
NCH	Non-Anomaly	Any felony	0.572	0.578	0.006	0.710	2105	2119
NCH	Non-Anomaly	Any conviction	0.671	0.652	-0.019	0.202	2105	2119
NCH	Non-Anomaly	Any pending	0.386	0.432	0.046	0.291	2105	2119
NCH	Non-Anomaly	Time since most recent charge	1428.382	1373.421	-54.961	0.337	2104	2118
NCH	Anomaly	N reports	54	49	-5	0.622	54	49
NCH	Anomaly	N charges	2.352	2.245	-0.107	0.810	54	49
NCH	Anomaly	Any felony	0.611	0.469	-0.142	0.152	54	49
NCH	Anomaly	Any conviction	0.685	0.673	-0.012	0.900	54	49
NCH	Anomaly	Any pending	0.870	0.347	-0.523	0.073	54	49
NCH	Anomaly	Time since most recent charge	1333.444	1189.204	-144.240	0.642	54	49
SCH	Non-Anomaly	N reports	1932	1890	-42	0.497	1932	1890
SCH	Non-Anomaly	N charges	6.157	6.101	-0.056	0.743	1932	1890
SCH	Non-Anomaly	Any felony	0.715	0.685	-0.030	0.042	1932	1890
SCH	Non-Anomaly	Any conviction	0.938	0.932	-0.007	0.403	1932	1890
SCH	Non-Anomaly	Any pending	0.497	0.590	0.094	0.154	1932	1890
SCH	Non-Anomaly	Time since most recent charge	1786.390	1815.601	29.211	0.664	1932	1890
SCH	Anomaly	N reports	52	46	-6	0.544	52	46
SCH	Anomaly	N charges	5.308	5.478	0.171	0.867	52	46
SCH	Anomaly	Any felony	0.558	0.674	0.116	0.243	52	46
SCH	Anomaly	Any conviction	0.885	0.913	0.028	0.647	52	46
SCH	Anomaly	Any pending	0.462	0.196	-0.266	0.183	52	46
SCH	Anomaly	Time since most recent charge	1582.327	1530.522	-51.805	0.887	52	46

Note: Group NCH refers to the same visibility group with No Charges Hidden in both treatment and control. Group SCH refers to the same visibility group with Some Charges Hidden in both treatment and control. Period Anomaly refers to December 15-31, 2023, and Period Non-Anomaly refers to the remaining sample.

Table B.2: Two-week placebo-window comparison around Dec 15–31 anomaly window.

Window	Group	N windows	Target N	Target diff	Target t	Tail share	Tail %	[Min t, Max t]
14-day	Same visibility (all)	392	180	0.262	4.239	0.003	0.26	[-2.597, 4.239]
14-day	Same visibility NCH	392	98	0.308	3.860	0.020	2.04	[-2.164, 4.377]
14-day	Same visibility SCH	392	82	0.209	2.148	0.064	6.38	[-2.345, 2.937]
17-day	Same visibility (all)	389	201	0.244	4.242	0.013	1.29	[-2.549, 4.562]
17-day	Same visibility NCH	389	103	0.312	4.083	0.021	2.06	[-2.317, 4.271]
17-day	Same visibility SCH	389	98	0.172	1.997	0.103	10.28	[-2.757, 3.262]

Note: Each row represents a separate rolling-placebo exercise (window length × subgroup). *N windows* is the number of placebo windows/regressions estimated when sliding that window around nearby dates. *Target diff* is the treatment–control adverse-action (AA) difference in the focal holiday window (Dec 15–31 for the 14-day spec; analogous centered 17-day target for the 17-day spec). *Target t* is the t-statistic on the treatment effect from that focal-window regression. *Tail share* is the fraction of placebo windows with $|t_{\text{placebo}}| \geq |t_{\text{target}}|$; smaller values mean the focal-window result is more unusual relative to placebo windows.

Table B.3: Adjudicator assignment balance and conditional adverse-action balance (AA | adjudicator) in the Same Visibility group

Group	Period	Metric	Mean C	Mean T	Diff (T-C)	P-value	N C	N T
NCH	Non-Anomaly	Assignment: adj389	0.343	0.328	-0.015	0.302	2105	2119
NCH	Non-Anomaly	AA | adj389	0.843	0.827	-0.016	0.412	721	694
NCH	Non-Anomaly	Assignment: adj829	0.239	0.234	-0.005	0.682	2105	2119
NCH	Non-Anomaly	AA | adj829	0.881	0.869	-0.012	0.566	503	495
NCH	Non-Anomaly	Assignment: adj4df	0.373	0.394	0.021	0.167	2105	2119
NCH	Non-Anomaly	AA | adj4df	0.794	0.806	0.012	0.543	785	834
NCH	Non-Anomaly	Assignment: adjother	0.046	0.045	-0.000	0.963	2105	2119
NCH	Non-Anomaly	AA | adjother	0.875	0.896	0.021	0.652	96	96
NCH	Anomaly	Assignment: adj389	0.574	0.571	-0.003	0.979	54	49
NCH	Anomaly	AA | adj389	1.000	0.643	-0.357	0.000	31	28
NCH	Anomaly	Assignment: adj829	0.000	0.000	0.000		54	49
NCH	Anomaly	AA | adj829					0	0
NCH	Anomaly	Assignment: adj4df	0.426	0.429	0.003	0.979	54	49
NCH	Anomaly	AA | adj4df	0.870	0.619	-0.251	0.057	23	21
NCH	Anomaly	Assignment: adjother	0.000	0.000	0.000		54	49
NCH	Anomaly	AA | adjother					0	0
SCH	Non-Anomaly	Assignment: adj389	0.314	0.304	-0.010	0.498	1931	1890
SCH	Non-Anomaly	AA | adj389	0.807	0.794	-0.013	0.591	606	574
SCH	Non-Anomaly	Assignment: adj829	0.279	0.261	-0.018	0.217	1931	1890
SCH	Non-Anomaly	AA | adj829	0.798	0.777	-0.020	0.422	539	494
SCH	Non-Anomaly	Assignment: adj4df	0.367	0.398	0.032	0.043	1931	1890
SCH	Non-Anomaly	AA | adj4df	0.763	0.725	-0.038	0.100	708	753
SCH	Non-Anomaly	Assignment: adjother	0.040	0.037	-0.004	0.533	1931	1890
SCH	Non-Anomaly	AA | adjother	0.821	0.783	-0.038	0.567	78	69
SCH	Anomaly	Assignment: adj389	0.635	0.565	-0.069	0.489	52	46
SCH	Anomaly	AA | adj389	0.848	0.692	-0.156	0.156	33	26
SCH	Anomaly	Assignment: adj829	0.000	0.000	0.000		52	46
SCH	Anomaly	AA | adj829					0	0
SCH	Anomaly	Assignment: adj4df	0.365	0.435	0.069	0.489	52	46
SCH	Anomaly	AA | adj4df	0.842	0.650	-0.192	0.179	19	20
SCH	Anomaly	Assignment: adjother	0.000	0.000	0.000		52	46
SCH	Anomaly	AA | adjother					0	0

Note: Group NCH refers to the same visibility group with No Charges Hidden in both treatment and control. Group SCH refers to the same visibility group with Some Charges Hidden in both the treatment and control matrices. Period Anomaly refers to December 15-31, 2023, and Period Non-Anomaly refers to the remaining analysis sample.

C Distribution and Outliers in Number of Contacts

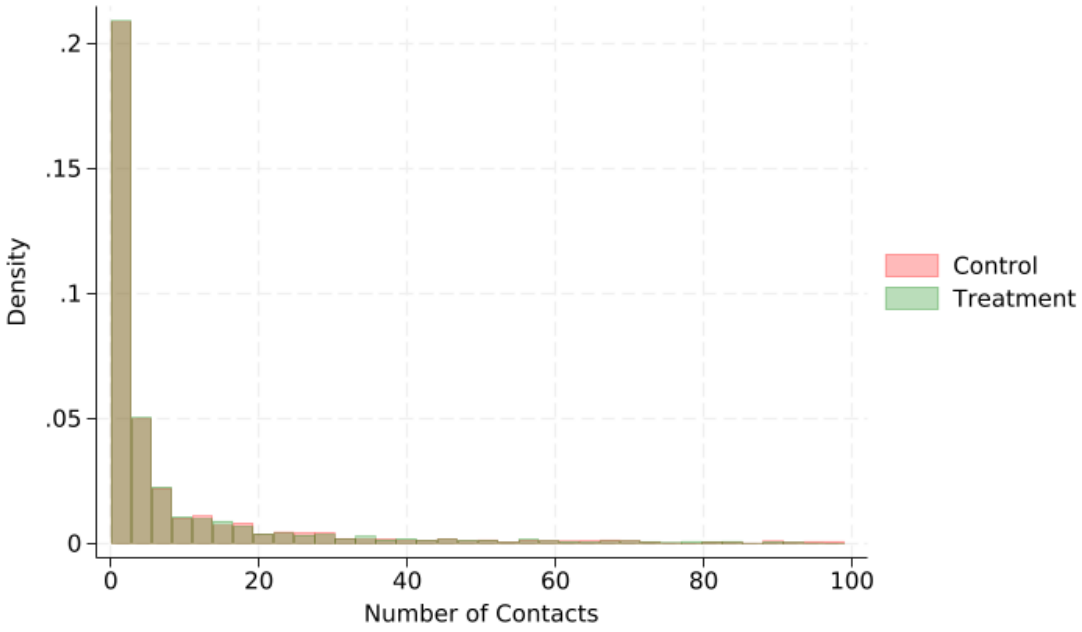
Table 4 and 5 displayed some apparent differences between those assigned treatment and those assigned control within the always clear group, in particular in ln(contacts) conditional on contacting anyone which flowed downstream to apparently fewer reviews and incidents in the treatment group. The always clear group is a placebo group in that treatment does not directly impact this group.

Figure C.1 illustrates the issue. It displays the distribution of number of contacts in the always clear group, separately for contacts < 100 (about the 95th percentile) and contacts > 100. The distribution has a lot of 0s and is also fat tailed. With contacts < 100 the treatment and control distributions lie on top of each other and it's hard to see much difference. However, with contacts > 100 there does appear to be some right skew to the contact distribution in favor of the control group. We seem to have randomly gotten some very high contact individuals in the control group. These high contacts do not seem to represent data errors as far as we can tell, the pattern tends to be high contact over time, not just a single-day spike, the most extreme cases have about 4 contacts/day on active days, with a few very high-frequency accounts reaching 12 contacts/day, consistent with active contact behavior and not data issues (TABLE??).

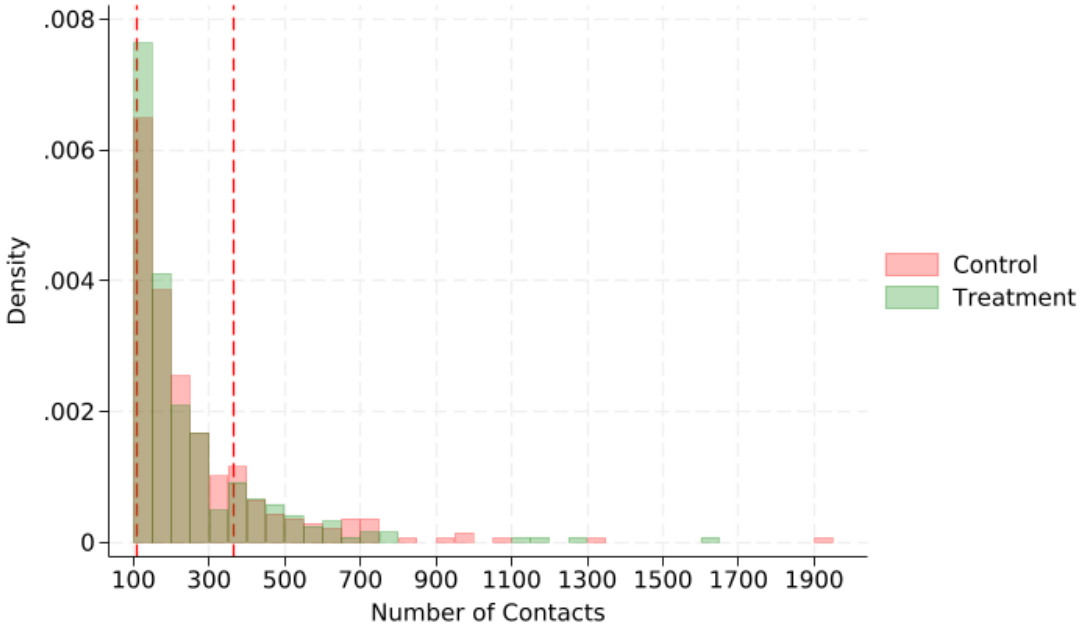
Table C.1: Outlier Robustness by Visibility Group

	Always Clear	Newly Clear	Reduced Visibility	Same Visibility
Number of Contacts	-3.595**	1.003	2.209***	0.668
	(1.564)	(1.433)	(0.775)	(0.802)
Number of Reviews	-0.390***	0.090	0.137*	0.001
	(0.151)	(0.131)	(0.075)	(0.080)
Number of Contacts	-83.317	300.434***	91.468	-41.795
	(66.729)	(72.676)	(57.895)	(67.851)
Number of Reviews	-109.986	211.718***	86.435**	-25.444
	(68.969)	(75.756)	(39.258)	(43.416)
Number of Contacts	-0.380	0.690*	0.630**	-0.333*
	(0.408)	(0.377)	(0.250)	(0.197)
Number of Reviews	-0.046	0.077*	0.049	-0.034
	(0.044)	(0.041)	(0.030)	(0.022)
N	9,446	11,134	5,927	8,046

Figure C.1: Distribution of Number of Contacts in Always Clear Group by Treatment



(a) Conditional on less than 100 Contacts



(b) Conditional on More than 100 Contacts

Note: Vertical dashed lines represent the 95th and 99th percentile in the contact distribution respectively among the always clear group.

D Additional Figures and Tables

Table D.1: Balance By Treatment Response Type: Always Clear

	Mean (T)	Mean (C)	p-value
Number of charges	2.355	2.342	0.809
At least one felony	0.117	0.115	0.767
At least one misdemeanor	0.844	0.859	0.044
At least one petty	0.147	0.136	0.113
At least one pending charge	0.034	0.032	0.512
At least one violent	0.102	0.096	0.284
At least one property	0.142	0.139	0.658
At least one drug	0.326	0.328	0.811
Number of felonies	0.246	0.252	0.727
Number of convictions	2.189	2.190	0.996
Number violent	0.130	0.126	0.671
Number property	0.231	0.221	0.545
Number drug	0.535	0.536	0.967
Years first to last charge	2.440	2.308	0.184
Years since last charge	9.859	10.008	0.300
Avg Charge Age (Years)	11.134	11.208	0.623
Num distinct charge dates	1.872	1.866	0.866
Observations	4,809	4,638	
Joint p-value			0.523

Notes: Each variable is based on the full criminal record (whether visible or not). The joint p-values report F-tests of the null that all listed variables are jointly equal across treatment and control groups within each panel, based on regressions of treatment assignment on the variables with robust standard errors.

Table D.2: Balance By Treatment Response Type: Newly Clear

	Mean (T)	Mean (C)	p-value
Number of charges	2.837	2.844	0.910
At least one felony	0.065	0.063	0.649
At least one misdemeanor	0.741	0.751	0.217
At least one petty	0.354	0.356	0.852
At least one pending charge	0.161	0.155	0.376
At least one violent	0.030	0.034	0.234
At least one property	0.047	0.053	0.145
At least one drug	0.349	0.349	0.963
Number of felonies	0.115	0.118	0.806
Number of convictions	2.228	2.279	0.377
Number violent	0.037	0.044	0.184
Number property	0.083	0.081	0.780
Number drug	0.608	0.623	0.508
Years first to last charge	2.786	2.824	0.704
Years since last charge	2.284	2.282	0.959
Avg Charge Age (Years)	3.686	3.684	0.975
Num distinct charge dates	2.193	2.220	0.496
Observations	5,662	5,472	
Joint p-value			0.110

Notes: Each variable is based on the full criminal record (whether visible or not). The joint p-values report F-tests of the null that all listed variables are jointly equal across treatment and control groups within each panel, based on regressions of treatment assignment on the variables with robust standard errors.

Table D.3: Balance By Treatment Response Type: Reduced Visibility

	Mean (T)	Mean (C)	p-value
Number of charges	9.047	8.909	0.495
At least one felony	0.604	0.616	0.339
At least one misdemeanor	0.938	0.940	0.736
At least one petty	0.286	0.272	0.221
At least one pending charge	0.347	0.331	0.213
At least one violent	0.515	0.523	0.546
At least one property	0.449	0.455	0.623
At least one drug	0.622	0.643	0.088
Number of felonies	2.092	2.038	0.539
Number of convictions	7.054	6.932	0.507
Number violent	1.019	1.039	0.671
Number property	1.259	1.165	0.187
Number drug	1.814	1.884	0.276
Years first to last charge	7.162	6.985	0.324
Years since last charge	1.594	1.651	0.265
Avg Charge Age (Years)	4.844	4.770	0.421
Num distinct charge dates	5.537	5.479	0.598
Observations	2,929	2,998	
Joint p-value			0.523

Notes: Each variable is based on the full criminal record (whether visible or not). The joint p-values report F-tests of the null that all listed variables are jointly equal across treatment and control groups within each panel, based on regressions of treatment assignment on the variables with robust standard errors.

Table D.4: Balance By Treatment Response Type: Same Visibility

	Mean (T)	Mean (C)	p-value
Number of charges	3.909	3.967	0.561
At least one felony	0.628	0.640	0.258
At least one misdemeanor	0.716	0.708	0.442
At least one petty	0.053	0.055	0.746
At least one pending charge	0.200	0.182	0.038
At least one violent	0.525	0.525	0.967
At least one property	0.408	0.401	0.541
At least one drug	0.305	0.330	0.020
Number of felonies	1.646	1.678	0.580
Number of convictions	3.061	3.168	0.259
Number violent	0.898	0.891	0.807
Number property	0.827	0.850	0.558
Number drug	0.656	0.701	0.151
Years first to last charge	4.044	4.308	0.074
Years since last charge	4.334	4.383	0.687
Avg Charge Age (Years)	6.386	6.597	0.140
Num distinct charge dates	2.517	2.548	0.579
Observations	4,009	4,036	
Joint p-value			0.365

Notes: Each variable is based on the full criminal record (whether visible or not). The joint p-values report F-tests of the null that all listed variables are jointly equal across treatment and control groups within each panel, based on regressions of treatment assignment on the variables with robust standard errors.