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## Assessing Access-to-Justice Outreach Strategies

J. J. Prescott

*University of Michigan Law School, jprescott@umich.edu*

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# Assessing Access-to-Justice Outreach Strategies

by

J.J. Prescott\*

The need for prospective beneficiaries to “take up” new programs is a common stumbling block for otherwise well-designed legal and policy innovations. I examine the take-up problem in the context of publicly provided court services and test the effectiveness of various outreach strategies that announce a newly available online court access platform. I study individuals with minor arrest warrants whose distrust of courts may dampen any take-up response. I partnered with a court to quasi-randomly assign outreach approaches to a cohort of individuals and find that outreach improves take-up, that the type of outreach matters, and that online platform access is itself effective.

*Keywords:* take-up, access to justice, RCT, outreach, warrants, duration models

*JEL classification code:* C41, C93, K14, K41, K42

## 1 Introduction

A core function of government is providing a forum where individuals can resolve their disputes. Access to courts of law has always been considered a valuable publicly provided benefit, if not a fundamental right. Unfortunately, the willingness of individuals to *take up* – i.e., use – these services is almost certainly below what is socially optimal. One key indicator of inadequate take-up in the U.S. is the pervasiveness of default in American courts (Greiner, Jiménez, and Lupica, 2015). In the context of minor disputes with the government (e.g., civil infractions, misdemeanors), physical, psychological, and financial access barriers have led to many millions of arrest warrants. These warrants are not only costly to the named individuals; they also generate outsized social costs in that those affected are unlikely to vote, to report being victimized, or to interact with government officials of any

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kind, out of fear of arrest (Brayne, 2014). Low take-up of court services highlights the fact that the mere existence of a program is insufficient to ensure that society truly benefits from its availability. Understanding how, when, and why individuals decline to use court services is therefore critical to improving them.

In this paper, I investigate strategies to improve beneficiary take-up of court services to resolve disputes, and in particular, minor disputes with the government. As an emblematic deficiency of access to justice for decades and, in recent years, a source of significant social unrest, minor warrants are essentially a failure by millions of people to make use of publicly provided court services. I study take-up behavior by leveraging recent technological innovation in accessing courts. To combat the dynamics that lead to the issuance of these warrants, a number of courts have implemented online platforms that reduce access barriers by allowing litigants to interact directly with prosecutors, judges, and court staff using Internet-based communication technology. These innovations are visible (if not salient) to new litigants, but individuals subject to warrants (usually months old) are typically unaware of these newly available avenues of relief.

I assess the relative effectiveness of outreach strategies by randomly assigning these strategies to a large sample of individuals subject to warrants. Specifically, I evaluate take-up-focused outreach involving phone calls, direct mail, fliers, and working through community partners, including employment and social-service providers and religious institutions. My empirical approach may be underpowered due to practical constraints and the partner court's wishes, and my results may be limited to the geographic setting (poor and urban) and the underlying platform technology. Nevertheless, this research demonstrates that outreach regarding court access opportunities can be successful and that the type of outreach matters. My results have an additional policy-relevant takeaway. When government is faced with uninformed beneficiaries, an experiment that randomly *informs* individuals of a program's availability is equivalent to an experiment that randomizes program availability itself. My findings, framed in this way, can be interpreted as evidence that technology-based access improvements – such as online platforms that allow litigants to communicate with court personnel – are effective.

The organization of this paper is as follows. In section 2, I situate this research in the existing benefit take-up literature. In section 3, I describe access-to-justice challenges and the institutional setting of the interventions. In section 4, I discuss the data and outline the outreach strategies I evaluate. In section 5, I present my identification strategy, main results, extensions, and robustness checks, and I offer a rough measure of the value of the interventions. In section 6, I conclude. Supplementary material, containing appendix tables and figures, is available online at <http://www.mohr.de/prescott>.

## 2 *Benefit Take-Up Research*

Individuals often fail to take full advantage of beneficial government programs and policies. This take-up problem has been studied in numerous contexts (Currie, 2006) and documented with respect to the public's engagement with tax policies (e.g., Smeeding, Phillips, and O'Connor, 2000; Chetty and Saez, 2013; Bhargava and Manoli, 2015), social security (Mastrobuoni, 2011; Liebman and Luttmer, 2015), savings (Madrian and Shea, 2001; Hastings and Tejada-Ashton, 2008; Song, 2015; Karlan et al., 2016), voting rights (e.g., Gerber et al., 2013; Green, McGrath, and Aronow, 2013; Pang, Zeng, and Rozelle, 2013; Ho, 2015), health (Kling, Liebman, and Katz, 2007; Kling et al., 2012), education (Hastings and Weinstein, 2008; Bettinger et al., 2012; Booij, Leuven, and Oosterbeek, 2012), and, most relevant to this paper, legal access and compliance (Greiner and Rubin, 2011; Greiner, Patanayak, and Hennessy, 2013; Archer, Boittin, and Mo, 2016). Take-up issues are thus widespread, and effective policy implementation requires that take-up be a critical design consideration. This idea dovetails well with the recent prominence of choice architecture (Thaler and Sunstein, 2008).

While it may seem surprising that people neglect to capitalize on policies designed to benefit them, researchers have offered several explanations for why these failures might occur. Currie (2006) observes that there are nontrivial costs to learning about and making use of government programs, and that these costs may be especially onerous for those who most need the programs. Other explanations include hyperbolic time discounting and stigma costs. Bhargava and Manoli (2015) emphasize the costs associated with the behavioral-economics literature, including psychological frictions due to cognitive, motivational, and emotional constraints. People may be confused about a program or policy (Liebman and Zeckhauser, 2004) or may even be unaware that a particular program exists (Chetty, Friedman, and Saez, 2013), or they may suffer from other psychological biases or tendencies (e.g., procrastination, inattention, distaste for time-consuming processes) that inhibit their pursuit of a program's benefits (Madrian, 2014; Bertrand, Mullainathan, and Shafir, 2006).

In light of the wide array of potential explanations, a common approach to identifying the mechanisms behind take-up propensity and the eventual effects of a program or policy change has been the use of randomized control trials (RCTs) to allow for rigorous causal inference. Unfortunately, truly randomizing law, legal procedure, or institutions (such as courts) is usually impractical, if not impossible, due to cost, political and ethical issues, or methodological challenges (Ho, 2015). However, individuals are often poorly informed about available programs or their legal rights (Macaulay, 1963; Ellickson, 1991), and quasi-RCTs can be conducted by experimentally altering individuals' awareness of the program under scrutiny (Angrist, Imbens, and Rubin, 1996). Such an approach requires that the treatment improve the targeted beneficiaries' knowledge of the program, which permits the researcher in turn to evaluate the resulting behavioral response.

Conditional on the type of intervention and the program at issue, research has found varying results with respect to take-up. Most studies have shown that outreach interventions (whether they arise from natural experiments or in a more controlled manner) generally increase awareness (or otherwise lower the participation costs) of the policy or program in question. However, whether this knowledge changes behavior may be both intervention- and policy-dependent, as some studies discern changes in awareness without any behavioral consequences (Rigotti et al., 1992; Greiner and Pattanayak, 2012), while other interventions appear to have significant behavioral effects (Ridgeway et al., 2011; Greiner, Pattanayak, and Hennessy, 2013; Liebman and Luttmer, 2015). Given this divergence, comparing the effectiveness of different outreach strategies may be particularly valuable. Not surprisingly, a few researchers have already sought to compare different take-up boosting approaches (Bettinger et al., 2012; Fellner, Sausgruber, and Traxler, 2013; Bhargava and Manoli, 2015). This paper follows in this tradition.

I follow a common theme in the benefit take-up literature by focusing on a recently implemented but potentially underused program: facilitating the resolution of minor arrest warrants by providing better access to courts through a newly available online platform. This study builds on the use of RCTs in both a take-up (Currie, 2006) and a legal context (Green and Thorley, 2014; Greiner and Matthews, 2016) by randomly assigning different outreach interventions (across geographic regions) that seek to inform eligible individuals of the availability of a particular court's online access platform.

While the literature focuses on increasing take-up of an underutilized good (e.g., disbursing additional EITC resources or easing access to voting), I evaluate a policy aimed at alleviating a stigmatizing financial and legal burden by lowering the barriers to discharging it. In this sense, this work follows and complements the research on encouraging the payment of overdue taxes (Hallsworth et al., 2017), on improving legal compliance (Fellner, Sausgruber, and Traxler, 2013), and on resolving debts of the financially distressed (Greiner, Pattanayak, and Hennessy, 2013; Haynes et al., 2013). For one example, Greiner, Pattanayak, and Hennessy (2013) intervene to inform and improve legal access for disadvantaged, financially distressed individuals, a group that shares many characteristics with individuals in my sample. Haynes et al. (2013) build on this theme: they deploy text messaging to lower the costs of compliance and thus the number of warrants issued – and discern a significant behavioral effect.

Overall, this research makes a few specific and well-defined contributions. By comparing the effectiveness of distinct take-up interventions, it provides insight into which outreach methods are most likely to improve take-up of court access innovations and other services. Evidence on the relative success of these interventions offers some understanding of the underlying barriers to court access by illuminating the sources of preintervention failures,<sup>1</sup> and understanding how reducing

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<sup>1</sup> The failure to access a court to resolve an arrest warrant is likely due to many of the standard reasons for low take-up in other contexts, including economic costs such as the

access costs can enhance access to justice (especially for low-income households) is critical to almost all justice-related reforms (Sandefur, 2007; Greiner, 2016). Moreover, one can draw tentative inferences from successful interventions about the social value of online access platforms themselves (Ho, 2015). Finally, this study joins the body of existing RCT work that examines lowering the cost of court access (e.g., Greiner and Pattanayak, 2012) by considering both new program innovations and new outreach interventions.

### 3 Access to Justice

The phrase “access to justice” is usually associated with access to an attorney or resources that will render complicated substantive law comprehensible to a lay person (Rhode, 2001). In the U.S., however, most lawsuits in state courts involve minor legal issues (Flango and Clarke, 2014) – e.g., civil infractions – and, for these cases, access to justice has as much or more to do with common physical, psychological, and financial barriers to appearing in court in person than it does with lack of representation or legal confusion (Bulinski and Prescott, 2016). Millions of individuals a year struggle to appear at a courthouse to address their minor legal issues. For some, these struggles can be rooted in a courthouse’s location or a litigant’s disability or work or child care schedule (Prescott, 2015; Difiore et al., 2015; Cabral et al., 2012; Yegge, 1994). For others, access adversity results from the financial hardship of fines or fees, the fear of speaking directly to a prosecutor or judge or in public, or the prospect of a negative, perhaps prejudiced outcome and its consequences (Sandefur, 2007; Flannery and Kretschmar, 2012).

#### 3.1 Minor Arrest Warrants

As a consequence of courthouse access barriers, millions of people in the U.S. default every year by failing to respond to a government allegation made against them (Heath, 2017). Practically speaking, courts have few useful tools to address these defaulted cases. Many courts proceed by issuing arrest warrants, aiming to induce litigants to turn up at the courthouse. Issuing a warrant increases the pressure on an individual to resolve the issue by magnifying the dispute’s stakes, but it does nothing to mitigate access difficulties. If anything, issuing a warrant exacerbates perceived barriers by creating confusion and fear about the associated process and the range of potential outcomes. Courts are unlike many social institutions in that they represent or are closely affiliated with law enforcement and can fine or even incarcerate noncompliant litigants. Moreover, once someone decides not to address an outstanding arrest warrant, he is likely to engage in socially costly avoidance be-

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time and effort required to appear and understand the process, as well as psychological costs such as fear and embarrassment (Sandefur, 2007; Bulinski and Prescott, 2016). This study, therefore, may be an avenue to gain more general insight into how interventions can smooth these well-understood frictions in other contexts, especially when the direct use of multiple interventions may be impossible.

havior, partially withdrawing from society and eschewing government services and public obligations (Brayne, 2014).

In the wake of recent revelations about the scope of this warrant problem (e.g., U.S. Department of Justice, 2015), a number of state courts have begun to focus on reducing courthouse access barriers. Unfortunately, many traditional reform options are not scalable. State courts have always operated as brick-and-mortar institutions, communicating by written correspondence to a litigant's last known address. While increasing the number of courts or expanding hours of operation would help, such reforms would be at best expensive half measures. They also do not address a core access challenge in the warrant context: the fear of coming to court and effectively surrendering when an ability-to-pay determination might result in arrest. A potentially more attractive approach involves the remote use of online platform technology that allows litigants to communicate and resolve their disputes over the Internet (Bulinski and Prescott, 2016). This approach addresses many of the above-mentioned barriers to court access.<sup>2</sup>

Over the last few years, dozens of state courts have adopted an online platform communication technology called Matterhorn to reduce access-to-justice barriers.<sup>3</sup> A key goal for some of these courts has been reducing their backlog of preexisting arrest warrants, but communicating the platform's availability to those individuals already subject to warrants has proven difficult, leading to low take-up rates and few resolved warrants. Consequently, courts now face their own access issue: how can they make individuals with warrants aware of their new, less intimidating, easier access opportunities? This question is but a species of a larger genus: how can government best make citizens aware of innovations that might benefit them, but of which they may be ignorant?

### 3.2 *Geographic and Institutional Setting*

Highland Park, MI, is a municipality contained within the geographic borders of Detroit, MI. The 30th District Court – serving Highland Park – is small for a trial court in Michigan, but structurally and jurisdictionally, it has much in common with Michigan's 100-plus other limited-jurisdiction trial courts.<sup>4</sup> In 2016, the court

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<sup>2</sup> There are at least two concerns with this approach, however. First, accessing courts online requires Internet access, which is not universal. Second, this form of access may produce different case outcomes, either because parties make different arguments or because decision makers view asynchronous online arguments differently. Online access seems likely to improve decision-making accuracy (Bulinski and Prescott, 2017). To the extent the decision to use the technology turns on a change in the expected outcome, my estimates capture the effect of this change as well as the reduction in access costs.

<sup>3</sup> Information on Matterhorn can be found at [www.getmatterhorn.com](http://www.getmatterhorn.com).

<sup>4</sup> Figure A1 displays a map of district court jurisdictions in Wayne County, MI. There are 276 district court judges in Michigan's 105 district courts. The 30th District Court only has one judge. District courts in Michigan typically handle: (1) civil litigation under \$25,000, excluding equity and small claims under \$3,000; (2) civil infractions, including traffic violations, and misdemeanors with sentences of less than one year; (3) preliminary trials on felony charges; and (4) landlord-tenant and summary proceedings.

disposed of approximately 14,000 cases, roughly 10,000 of which involved traffic violations (District Court of Highland Park, MI, 2016). The court's ratio of traffic disputes to total case load is somewhat higher than average state-wide and nationally, most likely because Highland Park is bounded and crossed by highways carrying commuters (see Figure A2). But the prominence of traffic cases in the court's docket is not unusual. Traffic cases make up the bulk of court cases in the U.S. (Flango and Clarke, 2014).

The 30th District Court's operations affect many of Detroit's citizens. Highland Park is demographically comparable to Detroit overall, but more akin to Detroit's more depressed neighborhoods.<sup>5</sup> According to the American Community Survey's 2015 estimates, over 49 % of the population lives in poverty (U.S. Census Bureau, 2016e). In nearby Hamtramck, the poverty rate is over 47 % (U.S. Census Bureau, 2016d), and for the city of Detroit as a whole, it is just over 40 % (U.S. Census Bureau, 2016c). The combination of Highland Park law enforcement writing thousands of traffic citations yearly and the extent of Detroit's poverty has resulted in the issuance of thousands of minor arrest warrants every year for unpaid traffic fines and fees. While Detroit is not representative of most American cities on many dimensions, the pervasiveness of civil infraction citations, poverty, and the use of warrants as a means of enforcing these fines is common (e.g., U.S. Department of Justice, 2015).

### 3.3 Online Platform Technology

In June 2015, the 30th District Court implemented online platform technology to address its very crowded traffic docket and to enhance access opportunities for the many people who found it too costly, confusing, or otherwise difficult to clear up their traffic ticket at the courthouse.<sup>6</sup> At the same time, the court decided to make the platform available to the many thousands of individuals with failure-to-appear (FTA) and failure-to-pay (FTP) warrants, allowing them to "go to court" over the Internet. To seek to have an arrest warrant withdrawn, a litigant directly petitions a judge through the platform. The application involves answering questions, a free-form statement, and a request for relief, usually one asking for permission to schedule a new appearance or to enter into a payment plan. While the substantive opportunities offered through the platform are similar to what would be available to a litigant meeting a judge face to face, using the platform dramatically reduces a litigant's costs and confusion over the process and eliminates fear of im-

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<sup>5</sup> Highland Park has a higher fraction of African Americans than does Detroit: 92.0 % versus 80.1 % (U.S. Census Bureau, 2016b,a).

<sup>6</sup> People who receive eligible traffic tickets or other civil infractions can locate their case on the court's platform and submit a request for relief, which includes answers to inquiries and a written statement. Requests are directed to law enforcement officials for initial review, and then a judge evaluates any recommendation, along with the litigant's initial submission, the case file, and the litigant's criminal and driving record. If the judge decides to make an offer of some kind, the platform delivers the terms and an opportunity to accept to the litigant's mobile phone or email account.



mediate arrest should the judge conclude the litigant is able to pay. Nevertheless, the court predicted low take-up by individuals with warrants because it anticipated most would remain ignorant of the new means of accessing the court.<sup>7</sup>

In mid-2016, the 30th District Court agreed to study this take-up problem by empirically evaluating potential outreach strategies with the goal of improving awareness and use of its new access opportunity. The court also agreed to exploit the geographic distribution of individuals with warrants and the fact that certain strategies were able to target specific people or areas in order to experiment with different interventions in different places. The randomization of these outreach strategies across Detroit serves as the basis for this paper's empirical strategy.

#### 4 *The Data and the Outreach Interventions*

##### 4.1 *Data*

I conduct this study using data from the 30th District Court and Court Innovations Inc., which owns and operates the Matterhorn platform. The court furnished a list of all open minor warrants issued in any of the previous three years as of Nov. 1, 2015 (4,150 total warrants).<sup>8</sup> After dropping five duplicate warrants, the data consisted of 3,589 individuals with 4,145 total warrants.<sup>9</sup> Fields include the name of the individual subject to the warrant, the case number, the case type, the warrant type, the litigant's mailing address, and the warrant issue date. Some of these arrest warrants were resolved after the list's creation but before interventions began, and of course the district court continued to issue warrants after generating the November 2015 list. I assume that this sample is broadly representative of minor warrants generally, although the data do not include very old warrants (i.e., those more than three years old).<sup>10</sup>

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<sup>7</sup> The court engaged in limited public promotion of the tool following the platform's adoption. The court and its law enforcement partners added language to their websites and tickets to announce the platform's availability. The police were encouraged to inform motorists of the new access opportunity. Importantly, the court did not attempt to communicate with individuals who had outstanding warrants. In part, the court made this choice because it assumed its contact information would often be out of date and, consequently, individual notices were unlikely to be sufficiently effective to justify the expenditure.

<sup>8</sup> The list of warrants was derived from a paper report generated by the court's case management system. This list was converted to a machine-readable format using optical character recognition software. Hand inspection indicated that the conversion was highly accurate, although some characters proved challenging (e.g., differentiating the letter "O" and the number "0"). I believe a large proportion of these errors were caught and corrected.

<sup>9</sup> The 450 individuals with multiple warrants often changed addresses in between warrants. Assuming an individual was most likely to live at the address of the most recent arrest warrant during the intervention period, all of these litigants' older warrants were assigned to their most recent address's ZIP code's treatment.

<sup>10</sup> Furthermore, because the technology had been available for months before the court generated the warrant list, individuals most likely to learn of the innovation may have

The 30th District Court handles all minor cases that arise within the district, including the civil infractions and crimes of individuals who live elsewhere, many far away from Highland Park and even out of state. My empirical strategy employs interventions at the ZIP code level, and so I used one-stage cluster sampling with ZIP codes as clusters to further refine the sample I analyze. Out of 194 unique ZIP codes, I sampled Highland Park (48203) and 13 nearby ZIP codes with more than 100 open warrants, covering much of the city of Detroit. Limiting the sample to only those individuals whose last known address was within one of these 14 ZIP codes produced a sample with 2,467 litigants.<sup>11</sup> I discuss the specific take-up interventions and my empirical approach of randomly assigning these interventions to ZIP codes below.

My analysis involves three other notable data and sampling choices. First, for individuals with more than one warrant as of Nov. 1, 2015, I relied on the oldest warrant's data when calculating warrant age, warrant type, case type, etc., on the assumption that the oldest arrest warrant marked the individual's entrance into warrant status and hence reflected the type and length of exposure to resolution options.<sup>12</sup> Second, because of the high number of total warrants in Highland Park (48203), I trisected the ZIP code into roughly equal parts by number of warrants using major cross-cutting streets (see Figure A4), and treated each – 48203(1), 48203(2), and 48203(3) – as a distinct ZIP code for intervention purposes.<sup>13</sup> Individuals with arrest warrants listing ZIP code 48203 alongside an inconsistent street address were excluded (27 observations) because I was unable to allocate them to a sub-ZIP code (Figure A5). Third, I assigned individuals to interventions by ZIP code using the address in the court's original arrest warrant list. For phone calls and direct mailers, I verified and updated contact details, which revealed that the ZIP code information in the court's arrest warrant data was sometimes inaccurate. For individual-level treatments, I incorporated any new information, but for the group

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already dropped out of the sample (by taking up), leaving behind potential beneficiaries who were less likely on average to know about or to be willing to use the platform.

<sup>11</sup> Table A1 provides the number of warrants for each ZIP code along with warrant density, population, and area. Not surprisingly, warrant density is highest in Highland Park. Differences across the intervention ZIP codes exist, including differences in arrest warrant density, but I control for fixed differences across ZIP codes in the work below. In Figure A3, I display the intervention geography by ZIP code, along with a sample of the data plotted using the court's information on an individual's ZIP code to assign interventions. Each ZIP code in the analysis sample received a package of interventions.

<sup>12</sup> However, for individuals with two or more warrants, I do incorporate the mailing address and ZIP code of the latest (usually second) warrant for purposes of assigning interventions (on the assumption that it is more up-to-date) when a single individual's warrants are linked to different addresses. When two warrants have the same issue date, I use the case number to infer the order of the cases.

<sup>13</sup> I describe these partial ZIP codes as simply ZIP codes in this paper. Of these 16 "ZIP codes," two received similar packages of interventions, so I study a total of 15 distinct intervention packages in my analysis (see section 5 for more details).

(e.g., fliers) and the organizational interventions, I treat the warrant's ZIP code data as accurate. Thus, exposure to the interventions is measured with error.<sup>14</sup>

My measures of take-up by potential beneficiaries – in this case, the decision to use the 30th District Court's new online Matterhorn platform – come from the platform provider and include whether and when a litigant determined to search for or *access* their case and/or submit a *request* for relief using the platform.<sup>15</sup> The access data run from the implementation of the platform until Apr. 14, 2017, or more than 500 days after Nov. 1, 2015. These data include many searches and requests by individuals not in my sample because many became subject to their warrants after Nov. 1, 2015, or because they resided in a ZIP code excluded by my sampling procedure.<sup>16</sup> Matching these data to the sample of individuals with warrants uncovers that 184 (7.5 % of 2,467) sample individuals used Matterhorn to search for their case, and 84 (3.4 %) individuals (or 46 % of the searchers) made a request over the platform at some point during the study period before Apr. 14, 2017. More than half of these requests for relief (45) resulted in warrants being rescinded following compliance by the litigant.<sup>17</sup>

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<sup>14</sup> There are at least two sources of measurement error. First, the court's information was sometimes internally inconsistent. This can be seen in Figures A4 and A5 in which Google Maps overrides the court's information on an individual's ZIP code and uses the street address to assign a new ZIP code. Based on my calculations from ZIP code 48203, these errors occur in roughly 5 % of cases, and this is probably an overestimate, as the 30th District Court is located in ZIP code 48203, and so clerks seem much more likely to accidentally enter this ZIP code – their "work" ZIP code – than others. Second, the data are sometimes out of date, even if the street address and ZIP code match.

<sup>15</sup> For cases in which litigants submit a request, my data also include whether the court approved the request and the amount owed at the time of the request. Access data and request data arrived in two separate files. I matched the request data to warrants using unique case numbers. I matched the access data to this merged set using the only overlapping identifiers – first, middle, and last names. Names sometimes differed across the files – likely the result of imperfect OCR in processing the warrant data – and so I used the user-written Stata command `reclink2` to merge the two data sources with a fuzzy matching algorithm (Wasi and Flaaen, 2015). I only treated these matches as accurate if the discrepancies were due to omitted characters or if the first and last names matched but the middle name was missing in one of the two data sources. The resulting data showed that out of 2,467 individuals, 210 used Matterhorn to search for warrants and 97 of these used the online platform to submit requests to the 30th District Court between June 29, 2015, and (for access) May 5, 2017, or (for requests) Apr. 14, 2017.

<sup>16</sup> I received data on all the search and request activity that occurred on the platform from June 29, 2015, to (for access) May 5, 2017, or to (for requests) Apr. 14, 2017. During this period, there were 4,189 searches by 1,587 individuals. Of these searches, only a subset made a request in regard to 457 warrants.

<sup>17</sup> The critical factor in the warrant rescission decision is whether the litigant follows through with a required up-front payment of \$50 toward the amount owed on the warrant. The platform provider's staff was unable to verify the reasons for the 39 rejections, but they were also confident that most or all rejections had resulted from the litigants having failed to pay the \$50 up-front fee in the time allotted.

## 4.2 Take-Up Improvement Strategies

The 30th District Court agreed to evaluate outreach strategies that are time-worn and commonsensical, but that vary in scalability and cost and in their underlying assumptions about behavior – in particular, about how individuals become aware of program opportunities and ultimately amenable to participation. The strategies I examine in this paper include (1) targeted outreach to specific prospective beneficiaries (e.g., phone calls), (2) broad outreach to geographic areas that contain potential beneficiaries (e.g., fliers), and (3) outreach to organizations likely to come into contact with potential beneficiaries (e.g., religious, social-service, and employment organizations).

The 30th District Court's outreach experiment design takes inspiration from the well-known Fugitive Safe Surrender (FSS) Program's ideas for improving take-up of court services (Flannery and Kretschmar, 2012).<sup>18</sup> FSS sought to encourage people with warrants to surrender by offering court access in non-law-enforcement settings. FSS experimented with various strategies to improve take-up, including partnering with faith-based institutions, TV and radio ads, billboards, Internet and newspaper ads, and fliers and posters.<sup>19</sup> Evaluating certain of these outreach strategies in the warrant context proved impractical or impossible.<sup>20</sup> In the remainder of this section, I describe the 30th District Court's specific interventions.

### 4.2.1 Phone Calls

Targeted live phone calls may establish personal connections with prospective beneficiaries and make it more likely that targets will recognize new opportunities (even if they view them initially with skepticism or fear). Unanswered calls may afford a caller an opportunity to leave a targeted voice mail message, which might be roughly comparable to text messaging (e.g., Karlan et al., 2016). A caller can also answer questions, provide guidance, and overcome concerns about a program (Greiner and Pattanayak, 2012; Greiner, Pattanayak, and Hennessy, 2013). Yet live phone calls are not very scalable as a strategy to improve take-up (contrast robo-calls) and might unintentionally produce shame and fear (Sandefur, 2007). The phone intervention involved organizing a call center staffed mostly by social-work students at the University of Michigan and assembling phone numbers using ar-

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<sup>18</sup> This federal program sought to reduce the number of arrest warrants so as to decrease the risk of serving warrants, to make neighborhoods safer, and to build trust between the police and the community (Flannery and Kretschmar, 2012, p. 438).

<sup>19</sup> FSS did not rigorously evaluate its strategies, but paper-and-pencil survey results shed light on its outreach effectiveness (Flannery and Kretschmar, 2012, pp. 446–449).

<sup>20</sup> For example, although FSS used a prolonged marketing campaign in certain cities, I was unable to pursue radio or TV advertising. Likewise, the institutional interventions were limited because time constraints made it difficult to establish more robust relationships. Consequently, this setting may be a better opportunity to evaluate phone calls, direct mail, and flier-based strategies – all of which can be more easily targeted, which in turn allows for randomization and thus more rigorous examination of their effectiveness.

rest warrant information and skiptracing software.<sup>21</sup> Callers followed scripts (Figure A6) when their calls were answered as well as when they left messages. Students phoned 767 individuals in six ZIP codes over three days, spoke to 144 individuals with warrants, and left messages for another 319 people. Of these 463 people, 22 submitted online requests within 72 hours of the intervention.

#### 4.2.2 Direct Mailers

Mailers may be the most common outreach intervention in take-up research (e.g., Hastings and Tejada-Ashton, 2008; Ridgeway et al., 2011; Mastrobuoni, 2011; Gerber et al., 2013; Fellner, Sausgruber, and Traxler, 2013; Liebman and Luttmer, 2015; Ho, 2015). They are more informal and less obviously targeted than phone calls (and therefore perhaps not as salient). A mailer stores information on how to take advantage of a new program, and a recipient can save the card as a reminder of the take-up opportunity. Yet mailers can contain personal information (Kling et al., 2012), and they necessarily reveal that the sender is aware of the recipient's address, which – in the warrant context – may generate pressure that improves take-up or counterproductive feelings of persecution. The intervention employed a postcard with language calculated to be nonthreatening and comprehensible to individuals below a 7th-grade reading level (Figure A7).<sup>22</sup> Ultimately, 644 of 703 treated individuals appeared to have received mailers, and five submitted requests for relief through the platform within seven days of the mailing date.

#### 4.2.3 Fliers

Unlike targeted outreach – e.g., phone calls – broad messaging like fliering offers the potential to increase program take-up among individuals who are unknown or for whom no useful contact information exists (Chetty, Looney, and Kroft, 2009). Fliers are a classic form of advertising. As with targeted outreach, administrators can control the content of the message and (mostly) where it appears. Fliering is inexpensive, fairly scalable, and much less likely to be perceived as intrusive or threatening. At the same time, publicly posted fliers seem less likely than direct mailing to catch the eye of prospective beneficiaries. The flier intervention involved distributing fliers with tear-off tabs to barber shops, beauty salons, grocery stores, and liquor and party stores in the five treated ZIP codes (Figure A8).<sup>23</sup> Around

<sup>21</sup> The skiptracing procedure (LexisNexis Accurint) produced as many as three phone numbers per person with each number having a matched name (either the individual with a warrant or someone linked by public records to that individual). No phone numbers could be located for some individuals receiving this intervention. Others had more than one. Up to three phone calls were made to reach an individual whenever possible.

<sup>22</sup> Mailing addresses for the 714 people in the five treated ZIP codes were updated using skiptracing software. A printing company (Vistaprint) then sent 703 mailers using these addresses (11 individuals were inadvertently omitted). The company concluded that 20 of the 703 addresses did not exist, and 39 mailers were returned as undeliverable.

<sup>23</sup> Google Search and Maps were used to locate appropriate businesses in intervention ZIP codes. One or two people at a time conducted the fliering. They skipped unexpectedly

30 businesses were flied in each ZIP code,<sup>24</sup> which translates to approximately one establishment per thousand residents or one for every five individuals with an outstanding warrant. Eleven individuals from these ZIP codes tendered requests through the platform within 21 days of the intervention.

#### 4.2.4 Religious Institutions

The overall success of safe surrender proceedings suggests that partnering with religious institutions can improve benefit take-up (Flannery and Kretschmar, 2012), particularly when potential users may be leery of a program or policy. A trusted leader's or counselor's accounting of a program's pros and cons may be especially compelling if the likely beneficiary values the religious affiliation and anticipates fielding follow-up inquiries about benefit use. Outreach mediated through third parties, however, can result in information distortion, and if targeted individuals interact only rarely with the institution, the messaging may miss its mark. Institutions also have their own goals and beliefs, which may conflict with prioritizing take-up. The intervention involved contacting approximately 80 religious institutions (churches, temples, etc.) in seven ZIP codes, explaining the access opportunity, offering training, and providing talking points and resources for the institution to share with its members.<sup>25</sup> In the 21 days following this intervention, six treated individuals submitted requests through the platform.<sup>26</sup>

#### 4.2.5 Social-Service and Employment Service Organizations

Service organizations that help people obtain social services and find employment typically employ one-on-one meetings with individuals who also happen to be relatively likely to benefit from access-to-justice programs (e.g., Bettinger et al., 2012). Arrest warrant status seems highly likely to be positively correlated with government or nonprofit assistance eligibility. For instance, warrants are barriers to securing employment (Goffman, 2009; National Research Council, 2014), and expert employment-related counsel is likely to be both salient and heeded, at least among those seeking out an organization's services. However, these organizations only interact with a fraction of individuals with warrants, and outreach success

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closed businesses (these were rare), and if they discovered an unlisted business while fliering preidentified businesses, they also flied the unlisted business.

<sup>24</sup> The exception was ZIP code 48203(2), which only had four businesses total.

<sup>25</sup> Google Search and Maps were used to locate suitable institutions in each ZIP code, and these were evaluated by their social media footprint to determine whether they were sufficiently large and active to constitute an important community organization. Each institution was initially contacted by phone. Direct communication was established with 26 institutions. If an institution requested more information, it received a memo by email and was offered training. If it indicated no further interest, it still received a memo by email that provided talking points for use with appropriate individuals.

<sup>26</sup> The organizational interventions were only quasi-random, as not all ZIP codes had sufficient numbers of the right type of organization to be feasible choices for treatment. ZIP codes were randomly selected from among those with adequate numbers.

hinges on messengers delivering accurate guidance. Moreover, some advisers may downplay warrant resolution as a strategy if explicitly broaching the subject might humiliate or frighten a client subject to an arrest warrant. Finally, persuading an organization to alter policies and priorities is hard (for instance, one entity outright refused information about the platform), and therefore the strategy may not scale. The interventions involved calling social-service organizations in five ZIP codes and employment service organizations in six ZIP codes, establishing contact with 13 and six entities, respectively. Successful contact resulted in follow-up with (1) information about accessing the platform to be made available to clients and (2) an offer to train employees on how to walk potential users through the online warrant resolution process. One individual in each set of intervention ZIP codes made a platform request within 21 days of the interventions.

## 5 *Empirical Strategy and Results*

The goal of this research is to evaluate the role that various outreach strategies can play in improving take-up rates of new court access opportunities. Access barriers to using courts to resolve disputes are significant and generate sizable social costs, especially in the context of minor arrest warrants. If reliable evidence can demonstrate that sufficiently simple and scalable outreach strategies linked to a court's adoption of online platform technology can encourage individuals to resolve their arrest warrants (or other disputes) more often and sooner, a persuasive case can be made in support of (1) expanding the use of the most cost-effective outreach approaches and (2) more broadly implementing the underlying platform technology innovation itself (or others of a similar nature) as a necessary ingredient to improving access to justice in U.S. courts (Ho, 2015).

### 5.1 *Identification Strategy*

My identification strategy involves the random assignment of different outreach packages to discrete sets of individuals with warrants. Interventions occurred in all high-warrant-density ZIP codes.<sup>27</sup> I built each of the 15 intervention packages by using different combinations of the six intervention types (Table A2), and then I randomly assigned each package to a ZIP code (Table A3).<sup>28</sup> Interventions occurred between Feb. 1, 2016, and Apr. 3, 2016, or for about two months (Table A4). The

<sup>27</sup> The 30th District Court preferred not to leave out potential beneficiaries in high-warrant-density ZIP codes. Fortunately, the random timing of the interventions allows for a reliable simple differences strategy to identify the consequences of the interventions, using individuals with warrants in the preintervention period as controls.

<sup>28</sup> Again, not all ZIP codes were eligible to receive every intervention. Organizational outreach strategies were assigned only to ZIP codes with minimally sufficient numbers of the relevant organization type. This feature affects the interpretation of any results, giving us something closer to an effect of the treatment on the treated (ETT) instead of the average causal effect (ACE) (Wooldridge, 2002, pp. 603–644). If each strategy had to be applied uniformly to all people, the ACE would be critical to understand. The fact that

timing and exact duration of each intervention were effectively random, although the execution of each by its nature took more or less time to complete.<sup>29</sup>

As a general matter, delays in take-up of court services to resolve disputes are socially costly, and so outreach efforts aim to encourage take-up as soon as possible. More important, however, is that interventions that accelerate take-up also *increase* take-up at any particular point in time. Accordingly, I employ duration analysis to quantify the effects of the various outreach interventions on take-up behavior and study whether the interventions reduce the time it takes for individuals with warrants to access court services. In order to separately identify the effects of the different outreach strategies, I carry out this analysis in a hazard regression framework, and thus compare the conditional probabilities of an individual accessing the court or making a request for relief through the court's online platform over a given period of time.

## 5.2 Summary Statistics and Nonparametric Analysis

I report sample descriptive statistics in Table 1.<sup>30</sup> In Figure 1, I explore the timing of the outreach interventions and changes in take-up by presenting a monthly moving average of the number of individuals accessing the court's online platform per day for the first time over the study period.<sup>31</sup> The figure suggests significant, but short-lived, increases in take-up during the intervention period.<sup>32</sup> Unfortunately, Table 1 and Figure 1 indicate (1) that individuals with warrants very rarely chose to access the court online to resolve their warrants and (2) that, although outreach efforts may significantly increase the take-up rate in percentage terms, the number of disputes resolved may remain low on account of the very low base rate.

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organizational outreach strategies would be ineffective in organization-free areas would need to be taken into account. In this context, however, estimating an ETT seems useful because strategies can be deployed in some places and not others.

<sup>29</sup> The timing of each intervention was determined largely by the ebbs and flows of the university academic calendar. Likewise, each intervention's duration was determined by implementation challenges and students' needs to conduct interventions while satisfying their coursework requirements. To the extent that more routine outreach efforts would be conducted differently, this study's findings should be considered tentative.

<sup>30</sup> The table lists the time-independent and time-dependent covariates available for analysis. The warrants were reasonably old (more than 1.3 years on average) at the outset of the study period, and only a modest number of them were resolved during the 529 days of the observation period, indicating significant right censoring.

<sup>31</sup> Although there is a clear uptick in platform access while interventions were ongoing (Feb. 1, 2016, through Apr. 3, 2016), there is a similar although less dramatic increase in take-up during the same period the following year when no interventions were occurring. I address this potential behavioral seasonality in my robustness checks.

<sup>32</sup> Figure A9 tells a very similar story using nonparametric Nelson–Aalen cumulative hazard functions (access and request), which are robust to right censoring.



*Table 1*  
Summary Statistics and Counts

	(1) All individuals	(2) Individuals accessed	(3) Individuals requested
Average time to access (in days)	508.62 (83.3)	255.78 (155.3)	219.32 (155.3)
Average time to request (in days)	518.31 (64.3)	392.94 (188.0)	225.95 (169.8)
Average time to approval (in days)	522.72 (49.5)	458.45 (147.0)	374.39 (187.1)
<i>Time-Dependent Controls</i>			
Average warrant age (as of Nov. 1, 2015)	502.69 (285.2)	490.76 (290.5)	471.63 (296.5)
<i>Case type (counts)</i>			
Misdemeanor criminal	604	29	11
Misdemeanor traffic	1,823	153	75
Felony criminal	40	2	1
<i>Warrant type (counts)</i>			
Failure to pay	437	35	16
Failure to appear	2,030	149	71
<i>Time-Dependent Variables</i>			
<i>Phone</i>			
Received live phone call	839	98	49
Received voice mail message	144	30	15
	319	57	30
<i>Mailers</i>			
Delivered	714	70	32
	644	66	30
Flier	791	56	23
Religious institution outreach	1,096	98	48
Social-service org. outreach	735	55	25
Employment service org. outreach	1,007	65	27
No. of observations	2,467	184	87

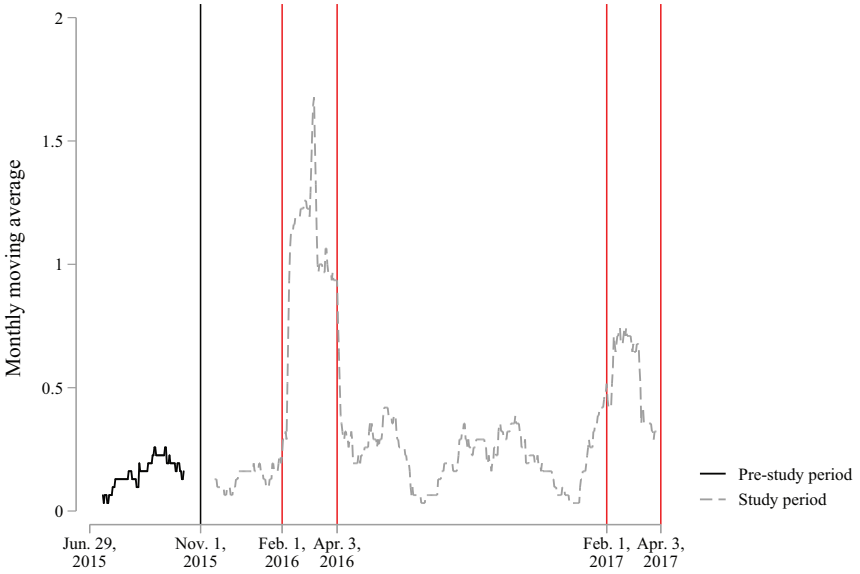
*Notes:* Column (1) displays descriptive statistics and counts for all individuals who had outstanding warrants in the intervention ZIP code areas. Columns (2) and (3) display the statistics for the subgroups of individuals in column (1) who accessed the platform to identify their case (2) and who submitted a request for relief (3). The total number of individuals in each column is reported in the last row, and can be used to calculate percentages. The study period totaled approximately 530 days.

### 5.3 Extended Cox Model with Time-Dependent Covariates

To assess the effect of an outreach intervention on take-up behavior, I use hazard analysis to calculate the conditional probability of an individual litigant declining to access the court's platform over time. I use Cox's partial likelihood method to estimate these proportional hazard models with time-varying or time-dependent covariates. This approach is agnostic about the baseline hazard, but it requires that the survival curves for different strata (as determined by the particular choices of independent-variable values) have hazard functions that are proportional to each other over time – i.e., the model assumes a constant relative hazard (Cox, 1972).

Figure 1

## Monthly Moving Daily Average of Platform Access



This modeling choice, however, does make it very straightforward to account for the rather significant noninformative right censoring in the data.<sup>33</sup>

Specifically, I estimate:

$$\lambda_i(t, Z) = \lambda_0(t) \exp(\beta \times \text{Interventions}(t) + \gamma \times \text{Controls}(t)).$$

The hazard  $\lambda(t, Z)$  is a function of time  $t$  (days) and a matrix  $Z$  of time-dependent and time-independent covariates. The vector of interest  $\beta$  captures the impact of the outreach interventions on the hazard rate, and  $\gamma$  is a vector of coefficients on the controls. Time-independent controls include arrest warrant age as of Nov. 1, 2015, warrant type, case type, and ZIP-code-level (assignment-level) fixed effects to allow for any fixed differences correlated with take-up behavior.<sup>34</sup>

In my baseline specifications, I characterize interventions in three ways. To begin with, I consider short-term, temporary effects on take-up *during* the intervention – i.e., I measure changes in take-up while the intervention is ongoing. These estimates therefore cover a narrow window of time. This representation may make sense for immediately effective interventions (e.g., calls), but with the organizational interventions, which require third parties to pass along information, one

<sup>33</sup> One complication for this strategy is that arrest warrants in my data may be recalled during the sample period through the preexisting, normal process of physically going to court. I address this informative censoring and its implications in section 5.4.

<sup>34</sup> I consider other controls, outcomes, and specifications in section 5.4.

might expect a lag of many weeks. For this reason, and to capture the long-term effects of interventions as information disseminates or becomes more salient over time, I also consider *exposure* to an intervention. If an individual is assigned to a ZIP code with a particular intervention, the exposure variable takes on a value of one from the start of the intervention through success or the end of the study period. Finally, targeted outreach methods permit three additional measures: phone call received, voice mail received, and direct mailer delivered.<sup>35</sup>

I primarily study two behavioral outcomes. First, on the theory that failure to take up publicly provided services is in significant part a function of potential beneficiary ignorance, I examine the determinants of *access*, meaning take-up as measured by whether individuals use the court's platform to search for their outstanding warrants. In my sample data, 184 individuals search for their warrants, sometimes more than once. Second, on the theory that access alone generates little benefit to the prospective beneficiary (although not none – at a minimum, litigants acquire option value), I evaluate the effects of the outreach interventions on the propensity to *request* relief. In my sample, 87 individuals sought relief through the platform. Requests take various forms, but they usually relate to rescheduling a missed hearing date or seeking payment plan approval.

Table 2 displays access take-up results. Column (1) shows the estimated effects of exposing individuals to the ZIP-code-level outreach interventions. Individuals who live in ZIP codes that received the phone call intervention were on average between two and three times more likely to access the court through the online platform. Individuals exposed to the direct mailer strategy were also more likely to access the court through the platform. However, this first cut of the data implies that the employment services strategy was counterproductive, reducing the relative likelihood of take-up in the form of access by half.<sup>36</sup>

In column (2) of Table 2, *exposure* is replaced with *during* to capture the short-term effects of the outreach interventions. The results are dramatic. I find little dependable evidence on estimated take-up with respect to the flier or the organization-related interventions, but the hazard ratios for the phone call and the mailer interventions are high – perhaps implausibly so. However, the extreme magnitudes of

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<sup>35</sup> A phone call is considered “received” when the caller reports having spoken to the correct person (according to court records). An individual is taken to have “received” a voice mail if the caller was able to leave a message, whether truly received in the end or not. A mailer is “delivered” if the printing company delivered at least one mailer directly to an individual's address. More than 90% of mailers register as “delivered” under this definition. With all three of these targeted outreach interventions, selection may play a role in any estimated relationship with take-up behavior. Individuals who are easier to reach by phone, who have voice mail service, or who reside at stable addresses may differ on many other dimensions relevant to warrant resolution. Care should be taken when interpreting these results.

<sup>36</sup> It may be hard to imagine that outreach could actually *discourage* access – raising the possibility that omitted variables are creating a spurious negative correlation – but enhanced outreach may frighten people with warrants if they believe the shift indicates courts are desperate for resources or if the messaging is otherwise off-putting.

Table 2  
Outreach Interventions and Court Access

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Phone</i>							
Exposure	2.782*** (0.00)		1.903*** (0.00)	0.951 (0.85)	0.955 (0.87)	0.739 (0.60)	1.925 (0.47)
During		49.142*** (0.00)	25.423*** (0.00)	23.787*** (0.00)	22.175*** (0.00)	24.002*** (0.00)	10.096** (0.03)
Received call				2.993*** (0.00)	2.983*** (0.00)	3.010*** (0.00)	3.024*** (0.00)
Received voice mail				2.652*** (0.00)	2.641*** (0.00)	2.643*** (0.00)	2.645*** (0.00)
<i>Mailers</i>							
Exposure	1.792** (0.02)		1.547*** (0.01)	0.556 (0.37)	0.562 (0.35)	0.618 (0.43)	1.181 (0.76)
During		21.675*** (0.00)	14.079** (0.02)	13.521** (0.02)	13.441** (0.02)	11.364** (0.03)	4.855 (0.21)
Delivered				2.722 (0.12)	2.563 (0.13)	2.538 (0.13)	2.521 (0.13)
<i>Fliers</i>							
Exposure	1.202 (0.25)		1.178 (0.22)	1.179 (0.27)	1.123 (0.37)	1.156 (0.71)	0.587 (0.47)
During		0.738 (0.79)	0.703 (0.76)	0.691 (0.74)	0.704 (0.76)	0.678 (0.71)	0.870 (0.90)
<i>Religious Institutions</i>							
Exposure	0.907 (0.70)		1.147 (0.53)	1.009 (0.97)	0.984 (0.94)	0.839 (0.62)	0.570 (0.20)
During		1.512 (0.63)	1.101 (0.90)	1.125 (0.88)	1.103 (0.90)	0.955 (0.95)	1.021 (0.97)
<i>Social-Service Org.</i>							
Exposure	1.565 (0.18)		1.658** (0.03)	1.577** (0.04)	1.623** (0.02)	3.373*** (0.01)	3.625 (0.23)
During		1.015 (0.99)	0.759 (0.82)	0.747 (0.81)	0.748 (0.81)	0.774 (0.84)	0.583 (0.68)
<i>Employment Service Org.</i>							
Exposure	0.574* (0.09)		0.701 (0.16)	0.676 (0.13)	0.661* (0.08)	0.921 (0.83)	2.396 (0.23)
During		0.843 (0.88)	1.142 (0.91)	1.094 (0.94)	1.144 (0.91)	1.260 (0.85)	0.479 (0.58)
<i>Controls</i>							
Case type effects					✓	✓	✓
Warrant type effects					✓	✓	✓
Warrant age controls					✓	✓	✓
ZIP code effects						✓	✓
ZIP code trends							✓
No. of observations	1,254,770	1,254,770	1,254,770	1,254,770	1,249,675	1,249,675	1,249,675

Notes: This table presents extended Cox hazard model estimates with “success” occurring when an individual accesses the court’s online platform to conduct a case search. Estimates are shown as hazard ratios with *p*-values (calculated by the delta method) in parentheses. The number of observations (equal to the number of individuals  $\times$  the number of days at risk) is higher in the first four columns because some covariate observations are missing in later columns. Results in columns (1), (2), and (3) are not sensitive to these observations. \*, \*\*, \*\*\* represent significance at the 10 %, 5 %, and 1 % level, respectively.

these estimates may simply be the result of the very low base rate of online court access among the comparison groups.

In column (3) of Table 2, I include the *exposure* and *during* intervention measures together to disaggregate the long-term effects of exposure (recall that *exposure* in column (1) includes the intervention period). The reported results suggest a reduction in the likelihood of court access following the phone call and the mailer intervention periods, although lingering effects on access through the platform are still economically and statistically significant and in the expected direction.<sup>37</sup> In column (4), I analyze the fact that the phone and mailer outreach strategies targeted specific individuals with warrants. Importantly, assuming that changes in take-up behavior should be detected only among the targeted ignores the fact that other “at risk” individuals (with unknown contact details) might nevertheless learn of the platform’s availability from those who were known and treated.<sup>38</sup> In general, treated people actually reached by phone were three times as likely to access the court through the platform. A similar pattern exists for mailer receipt, but the estimates are not statistically significant at conventional levels.

In columns (5), (6), and (7) of Table 2, I add controls to the specification in column (4). Specifically, I control for case type, warrant type, and warrant age in column (5),<sup>39</sup> and add further controls to allow for the fact that randomization was across a relatively small number of ZIP codes: I add ZIP-code fixed effects in column (6) and ZIP-code-specific linear and quadratic trends in column (7). Even in the most saturated model in Table 2, the phone call interventions show significantly higher relative likelihoods of individuals taking advantage of the court’s new online access opportunity.

In Table 3, I examine the decision of individuals not just to access the court via the online platform, but to follow through and submit a request for relief to the judge – i.e., to use the court’s services to attempt to resolve their arrest warrant. Unfortunately, too few relief requests occurred during the span of the outreach interventions themselves to include the *during* measure in these models, and so I concentrate instead on the longer-term *exposure* measure.<sup>40</sup> In addition, no one

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<sup>37</sup> I note that the relationship goes the other way with the social-service organizational intervention. Although I do not wish to put too much weight on these results before incorporating further controls, it is worth observing that the hazard ratio for *exposure* is consistent with an outreach intervention taking some time to sink in before it affects take-up when mediated by a third party – i.e., no effect during the intervention, but some measurable effect down the road, perhaps as an institution’s practices change.

<sup>38</sup> This argument also implies that litigants receiving interventions in their ZIP codes might pass along information and encouragement received from any outreach to others in other ZIP codes. I suspect that any spillovers would be more significant within a ZIP code than outside of it, but regardless, this dynamic would likely create a bias against my detecting any increase in take-up as a result of these interventions.

<sup>39</sup> Adding these controls reduces the sample size, and drops the number of individuals accessing the platform during the study period from 184 to 182.

<sup>40</sup> Thus, the interpretation of the effect of *exposure* differs in this context, including both the *during* and *exposure* effects explored in Table 2.

*Table 3*  
Outreach Interventions and Relief Request

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Phone</i>						
Exposure	3.951*** (0.00)	1.689 (0.12)		1.606 (0.14)	1.202 (0.76)	4.481** (0.01)
Received call		3.301*** (0.00)	3.973*** (0.00)	3.379*** (0.00)	3.713*** (0.00)	3.688*** (0.00)
Received voice mail		3.062*** (0.01)	4.089*** (0.00)	3.003*** (0.00)	3.166*** (0.00)	3.130*** (0.00)
<i>Mailers</i>						
Exposure	2.361* (0.09)					
Delivered		2.530* (0.06)	2.558** (0.04)	2.245* (0.10)	3.541** (0.02)	7.577*** (0.00)
<i>Fliers</i>						
Exposure	1.180 (0.66)	1.246 (0.57)	1.270 (0.54)	1.142 (0.71)	1.734 (0.46)	3.052 (0.22)
<i>Religious Institutions</i>						
Exposure	0.898 (0.76)	0.794 (0.53)	0.868 (0.69)	0.771 (0.47)	0.465 (0.15)	0.514 (0.13)
<i>Social-Service Org.</i>						
Exposure	2.067 (0.19)	2.053 (0.20)	2.055 (0.17)	2.107 (0.15)	4.130** (0.03)	4.875** (0.02)
<i>Employment Service Org.</i>						
Exposure	0.369* (0.06)	0.370* (0.07)	0.414 (0.13)	0.378* (0.07)	0.539 (0.44)	1.191 (0.78)
<i>Controls</i>						
Case type effects				✓	✓	✓
Warrant type effects				✓	✓	✓
Warrant age controls				✓	✓	✓
ZIP code effects					✓	✓
ZIP code linear trends						✓
No. of observations	1,278,678	1,278,678	1,278,678	1,273,275	1,273,275	1,273,275

*Notes:* This table presents extended Cox hazard model estimates with “success” occurring when an individual submits a request for relief to the court through the online platform. Estimates are shown as hazard ratios with *p*-values (calculated by the delta method) in parentheses. The number of observations (equal to the number of individuals  $\times$  the number of days at risk) is higher in the first three columns because some covariate observations are missing in later columns. Results in columns (1), (2), and (3) are not sensitive to these observations. \*, \*\*, \*\*\* represent significance at the 10%, 5%, and 1% level, respectively.

who experienced the direct mailer intervention but did not receive a mailer actually submitted a relief request (roughly 10% of the individuals in the treatment). Consequently, in most of Table 3’s regression specifications, I forgo including the mailer *exposure* measure in the model altogether, examining only the effect of the intervention on those who in fact received a mailer (i.e., those who had a valid mailing address and for whom nothing was returned to sender).

Table 3 tells a story very similar to Table 2. The evidence shows that phone and mailer outreach not only enhanced individuals’ awareness of the platform and their

willingness to experiment with the technology by searching for their existing arrest warrants, but also encouraged these litigants to take up the opportunity for a judge to review their case. Thus, I detect evidence of behavioral change. This need not have been true. Others have found that outreach strategies that improve understanding do not inevitably change take-up behavior (Rigotti et al., 1992; Mastrobuoni, 2011; Booij, Leuven, and Oosterbeek, 2012; Greiner and Pattanayak, 2012). Moreover, the data suggest that outreach strategies that seem to increase access behavior also seem to boost request behavior. Although this scenario is intuitive, the data could have shown it to be false. Theoretically, strategies that convince people to search for their cases on the court's platform could have been counterproductive when it came to encouraging requests for relief. The opposite account is also easy to imagine. Certain outreach strategies might discourage search among those who are uncertain about whether they wish to resolve their warrant, but encourage search among those who are highly likely to submit a request conditional on searching. Thus, *ex ante*, one should not have necessarily anticipated similar patterns with respect to search and request behavior.<sup>41</sup>

As to Table 3's results, individuals who received calls or voice mail messages appear more than three times as likely to submit a request through the platform. The fact that individuals who lived in ZIP codes undergoing the phone call intervention but who did not actually receive a call or a message were more likely to submit requests is consistent with a localized spillover of this outreach – perhaps by word of mouth.<sup>42</sup> My estimate of the effect of using mailers is also large, and suggests that individuals who received a postcard were seven times more likely to submit a request after the intervention. Outreach through social-service organizations seems to have had a long-term effect on litigant request behavior as well. No such effect is evident for search behavior when the intervention is disaggregated into *during* and *postintervention exposure* treatments, but the combined *postintervention* effect on take-up reported in Table 3 hints at potential significant long-term benefits from working with at least this set of organizations.

A number of provisional conclusions follow from this evidence. First, outreach in this domain can improve take-up. Second, individually targeted interventions (e.g., phone calls and direct mailers) are pricier, but they are also more effective. Individualized outreach may still generate shame or fear of persecution, but such

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<sup>41</sup> I chose not to test whether the interventions improve request take-up conditional on the decision to search for one's case, which might be a more natural way to address this question and would allow further exploration of how certain outreach strategies operate to change behavior. Instead, I present the overall effect of the strategies on request behavior on the theory that this evidence is more policy-relevant and may be more robust when transplanted to other access-to-justice domains.

<sup>42</sup> On the other hand, the fact that it was not possible to locate working phone numbers for these individuals may mean that they differed from their counterparts in some critical way, and although my prior is that any difference between the groups is likely to be one correlated with lower take-up, it might be that individuals who are highly mobile or who often change phone numbers are more likely to be in the process of improving their situation, including perhaps taking care of any outstanding warrants.

outreach seems to induce individuals to take up court services at higher rates in spite of this – or perhaps because of it. Third, take-up outreach through organizations is largely ineffective, although this may be a consequence of the short-term and limited nature of the specific interventions I study. Social-service organizations appear to offer at least some potential, but there is little support for the notion that outreach through religious institutions augments take-up – a finding that is at odds with an FSS Program premise.<sup>43</sup>

#### 5.4 Extensions and Robustness Checks

In this subsection, I outline extensions and robustness checks capable of refining, expanding, and deepening the analysis above, and in so doing, I implicitly plot a course for future research. Space limitations require that I review these ideas and findings very briefly in these pages. Results from this subsection are available in this paper's online appendix or from the author upon request.

I begin by discussing extension concepts. First, outreach strategies may interact with each other in ways that magnify or dampen their effects on take-up behavior. Empirically analyzing this possibility seems feasible and even straightforward in theory because each ZIP code received a package of interventions. Unfortunately, there are sufficiently few “successes” in the data that even estimating two-way interaction coefficients asks too much if one wishes to interpret the results with confidence. That said, including two-way interaction outreach indicator variables as controls has little effect on the story told by the estimates of the main effects. Second, outreach may have heterogeneous effects (1) with respect to whether a litigant has an old warrant – more than three months old, say – and also (2) with respect to a potential beneficiary's gender.<sup>44</sup> I approach these two propositions by interacting the outreach intervention measures with an old-warrant indicator and a gender indicator in independent analyses.<sup>45</sup> Examining estimates on the interaction terms reveals little of interest with respect to warrant age. However, men do appear to be more responsive to calls, and yet somewhat less responsive to mailers and voice mails. Third, outreach strategies may vary smoothly in their effects over time – beyond what is likely to be captured by the blunt *during* and *exposure* variables. Certain categories of outreach may take time to sink in, effectiveness rising over time, while others may make an initial splash, followed immediately by a

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<sup>43</sup> The religious-institution results may be due to selection if individuals in those ZIP codes with enough active churches, mosques, etc., to be eligible to receive the treatment were also systematically less able to access the platform after the intervention. However, my attempts to control for ZIP-code fixed effects and ZIP-code-specific linear trends (and quadratic trends with respect to access) make this possibility seem less likely.

<sup>44</sup> The warrant data from the court did not include gender information. To fill this gap, I employed Gender-API.com's gender-matching algorithm, which combines government data, social-network information, and machine learning techniques to make a prediction about an individual's gender using the individual's first name and country of residence.

<sup>45</sup> Men and those with old warrants have lower hazards on average at any point in time, and therefore their prospective warrant durations are longer.



steady decline in effectiveness. Adding implementation indicators interacted with continuous measures of time produces a negligible impact on the estimated main effects, and estimates for the interaction coefficients are not sufficiently precise to draw reliable conclusions, although any such effects are likely to be small.

Another extension opportunity – and limitation – of the empirical work above lies in the fact that it does not take account of any arrest warrants that were recalled through the traditional in-court process. This informative right censoring has two chief implications: (1) the outreach interventions may *also* improve traditional in-court warrant resolution rates – i.e., there may be additional unmeasured take-up that occurred at courthouses; and (2) platform availability may lead litigants merely to substitute online process for traditional process, with no net reduction in the number of outstanding warrants.<sup>46</sup> The 30th District Court retains data on arrest warrants recalled by any process,<sup>47</sup> and so one can examine these possibilities explicitly. My analysis finds, first and foremost, that Tables 2 and 3 are robust to censoring by traditional warrant recalls, but it also hints that outreach may have had beneficial consequences for traditional court access as well.<sup>48</sup>

One threat to the validity of the findings reported in Tables 2 and 3 is that the 30th District Court offered an amnesty on fines and fees related to traffic tickets during much of the outreach intervention period. This amnesty ran from Feb. 16, 2016, through Mar. 31, 2016, and received press (CBS Detroit, 2016). However, it also applied uniformly to all ZIP codes, and therefore it does not explain the variation in take-up I observe across outreach strategies – although, at least with respect to my *during* estimates, I may be computing a combined effect of amnesty and outreach. Also critical is that this amnesty explicitly did *not* apply to warrants.<sup>49</sup> Nevertheless, individuals may have mistaken the amnesty as available to them and discovered the court’s platform as a consequence. In general, amnesty messaging and availability ought to be considered indirect outreach and may have reinforced the warrant-related interventions, altering the interpretation of my estimates. Controlling for the amnesty is not strictly possible in an extended Cox model because the amnesty was available to all individuals simultaneously and is thus perfectly collinear with analysis time. In contrast, the additional structure of a parametric (Weibull) hazard model makes it possible to control for the amnesty explicitly by

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<sup>46</sup> Online process is much less burdensome on litigants than in-person process, and so substitution in and of itself would presumably be socially beneficial on net.

<sup>47</sup> The court had considerable difficulty collecting these data internally, and there are some indications in the data that they may be unreliable, with potential mismeasurement occurring both as to the warrant recall decision itself and as to the timing of any recall. For this reason, I chose to include this analysis in the extensions and robustness checks section of the paper.

<sup>48</sup> Reconsidering the analysis in Tables 2 and 3 in light of the fact that the traditional recall of a warrant means it is no longer “at risk” of being resolved online produces nearly identical results. The estimated effects of the outreach interventions on broader measures of warrant resolution are similar to those in Tables 2 and 3 – indicating no substitution.

<sup>49</sup> Although the online platform is mentioned in the 30th District Court’s amnesty press release, the release states, “civil infractions only” (see Figure A10).

including a time period indicator. Doing so, I find that, although the amnesty is itself associated with better take-up, the findings in Tables 2 and 3 are robust to controlling for the policy as a potential confounder. I also investigate whether outreach is differentially effective during the amnesty. I observe some suggestive evidence that direct mailers may have been more effective during the amnesty, but in general, I am unable to discern reliable evidence in either direction on this question.

I also use Weibull hazard models with month-of-the-year fixed effects to analyze the role that the early months of the year may play in explaining the results reported in Tables 2 and 3. Seasonality may be important in warrant-clearing behavior given the apparent uptick in the hazard rates during the late winter months in Figures 1 and A9 (i.e., in both 2016 and 2017). Hazards increase across the board in February, and controlling for this and any other month-of-the-year patterns does reduce somewhat the magnitude of my estimates of the *exposure* coefficients.<sup>50</sup> Even so, the baseline findings persist, and the targeted outreach intervention coefficient estimates remain fundamentally unchanged relative to Weibull model estimates without the month-of-the-year controls.

### 5.5 Assessing the Benefit of the Interventions

Translating hazard ratios into something useful for policy design is difficult but critical when programs are costly and making trade-offs is necessary. To offer some estimate of how much the various outreach approaches can reduce warrant durations, I compare survival curves (equivalent to Kaplan–Meier product limit estimates) by postulating a representative individual with time-invariant litigant and warrant characteristics set equal to their sample means.<sup>51</sup> I calculate a baseline survival curve and then compare it with a curve that incorporates the outreach treatment’s estimated effects,<sup>52</sup> and I interpret any difference between the curves as solely the result of the intervention. Integrating the two curves and subtracting produces a

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<sup>50</sup> One potential explanation for the uptick in take-up in the early months of the year was highlighted by the 30th District Court’s administrator during a discussion of the 2016 amnesty: “We’re thinking that, you know, it is tax time, and folks who didn’t have the money and now probably have a little extra money and want to take care of that business will be able to do it now” (CBS Detroit, 2016). This explanation probably carries more weight with respect to late fees on tickets than with long-term arrest warrants, but the fact that litigants may have more disposable cash in these months suggests there may be a regular and reliable uptick in warrant resolution in the early months of the year.

<sup>51</sup> Note that these values do not represent any particular individual with a particular warrant in the sample as most of the variables are binary in nature. I also control for the age of the warrant and its square at the outset of the study period.

<sup>52</sup> To simplify matters, I consider implementing just one intervention at a time – i.e., multiply the baseline survival function by the treatment hazard rate, i.e., the exponent  $I$  of the linear prediction (where the linear prediction sets the intervention being considered to one, the remaining interventions to zero, and the other covariates to their sample means). To keep these survival curves interpretable, I work with a model with no time-dependent covariates – i.e., I assume the treatment occurred over the entire sample period. Figures A11 and A12 display the curves built for my change-in-duration calculations.

measure of the average reduction in the number of days it takes for an individual to access the platform or make a request.<sup>53</sup>

Using this approach and estimates from a basic model similar to column (3) in Table 3 (adding fixed effects for warrant age and type, case type, and geography), I calculate that phone call receipt results in a 26-day reduction in the time it takes individuals to access the court through the platform. Voice mail receipt (22 days) and social-service organization exposure (25 days) also produce large duration reductions. Somewhat smaller reductions result from direct mailers (9 days) and fliers (3 days). Other treatments point to small increases. Outreach intervention effects on platform requests are qualitatively similar, with phone call receipt (14 days), voice mail receipt (13 days), direct mailers (12 days), and social-service organization exposure (16 days) all leading to significant reductions.

The goal of these back-of-the-envelope calculations is simply to speculate on the measurable benefits of court access outreach strategies. Once we are sufficiently confident that outreach can change take-up behavior, the question becomes whether the effort is worth its expense. Resolving arrest warrants a few weeks earlier may seem minor from a court's perspective, but this estimate is an average, and there may be economic spillovers and knock-on effects in terms of increased employment and tax revenue and fewer social-benefit expenditures. Society also profits from mitigating socially wasteful avoidance behavior. Courts issue millions of minor arrest warrants each year, usually to those least able to deal with their consequences, so inexpensive interventions that produce even a slight benefit per warrant may still be of great social value.

## 6 Conclusion

In this paper, I evaluate the relative effectiveness of litigant outreach strategies on take-up of publicly provided court services. Leveraging the recent adoption of an online platform to promote the resolution of outstanding warrants by the 30th District Court in Highland Park, MI – a court that serves a mostly poor, urban population facing typical access-to-justice challenges – I collaborated with court staff to develop and deploy a handful of familiar outreach strategies (phone calls, mailers, fliers, etc.) to improve court services take-up. To facilitate a causal interpretation of any findings, I randomly assigned outreach strategies to different geographic areas. This identification strategy not only allows the effectiveness of different outreach strategies to be compared, but permits inference regarding the consequences of

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<sup>53</sup> This calculation ignores any difference in the survival function after the 529 days of the study period. Thus, I ignore any long-term take-up effects, including the possibility that some individuals who access the court as a result of an intervention might never have addressed their warrant, leading to a very long tail. Accordingly, this approach likely understates the impact of the intervention. Consider how the calculation handles someone who accessed the platform on day 100 but would have otherwise *never* gone to court: the intervention is calculated to lower the individual's duration until access by  $529 - 100 = 429$  days, while in reality, the reduction would be much greater.

courts adopting platform technology itself, at least with respect to similar courts in similar communities. My empirical analysis demonstrates that a range of outreach strategies can work to improve take-up in the court services context, although there is wide variation in effectiveness. Targeted approaches – including phone calls, voice mail messages, and mailers – appear most potent and imply that concerns about such approaches counterproductively reducing take-up by inducing fears of persecution are unfounded. Rough duration tabulations suggest that interventions can reduce take-up times by weeks. Finally, to the extent that failure to take up new opportunities is often the result of ignorance, these results serve as evidence of the value of platform technology itself.

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J.J. Prescott  
 University of Michigan Law School  
 Ann Arbor, MI 48103  
 U.S.A.  
 jprescott@umich.edu