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Victimized Twice: A Field Experiment on the Employability of Victims

Ali M. Ahmed ^a and Elisabeth Lång^{a,b}

^aDivision of Economics, Department of Management and Engineering, Linköping University, Linköping, Sweden; ^bSwedish Defense Research Agency (FOI), Stockholm, Sweden

ABSTRACT

Do employers discriminate against victims? We address this question and present the findings of a field experiment on hiring discrimination against victims. Matched pairs of written job applications for fictitious victims and nonvictims were sent to 1,117 employers in various labor market sectors. The probability of receiving a job interview invite or a job offer from employers was then estimated. Differences in this probability between the victim and nonvictim applicants were interpreted as discrimination. Results show that victims had a significantly lower probability of receiving a job interview invite or a job offer from employers than nonvictims. The extent of discrimination varied with applicants' sex and occupational characteristics.



KEYWORDS

Victims; labor market discrimination; field experiment

Introduction

Crime imposes significant costs upon victims and society in terms of property damage, lost workdays and productivity, medical expenses, and operating the criminal justice system (Lynch, 2010; Wickramasekera, Wright, Elsey, Murray, & Tubeuf, 2015). Crime also imposes indirect costs on victims and their families, such as physical pain and suffering, diminished quality of life, and psychological distress (Cohen, 1988; Dolan, Loomes, Peasgood, & Tsuchiya, 2005; Fuller, 2015; Hanson, Sawyer, Begle, & Hubel, 2010; Miller, Cohen, & Rossman, 1993; Miller, Cohen, & Wiersema, 1996). Research on labor market opportunities shows that people who are victims or have been victimized in the past (in most studies, victims of domestic violence) have higher rates of unemployment, lower earnings, and higher rates of absenteeism and job turnover than do nonvictims (Adams, Tolman, Bybee, Sullivan, & Kennedy, 2012; Hanson et al., 2010; Loya, 2015; Macmillan, 2000; Reeves & O'Leary-Kelly, 2007; Tolman & Wang, 2005; Yancey, Gabel-Hughes, Ezell, & Zalkind, 1994).

The disadvantages of victims in the labor market can be explained by factors on both the supply and demand side. On the supply side, being a victimized person may result in lost productivity, through physical or psychological injury (Hanson et al., 2010). The negative association between victimization and labor market opportunities is, however, of a noncausal nature if people of lower socioeconomic status are more vulnerable to crime (Meier & Miethel, 1993). On the demand side, employers, recruiters, or co-workers may discriminate against victims. Research shows that victims are often stigmatized in the workplace and that they even face the risk of losing their job (Kennedy & Prock, 2018;

CONTACT Ali M. Ahmed  ali.ahmed@liu.se  Division of Economics, Department of Management and Engineering, Linköping University, Linköping 581 83, Sweden.

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Swanberg & Logan, 2005). Such discrimination could arise if employers associate victims with lower productivity or an unstable life situation, and therefore, are less willing to hire victims. In this study, we focus on the demand-side driven negative association between criminal victimization and labor market success.

We tested the hypothesis of employer discrimination against victims by utilizing a correspondence test experiment with fictitious victim and nonvictim job seekers in the Swedish labor market. We constructed matched pairs of written applications designed for four occupational categories: female-dominated low-skilled and high-skilled jobs and male-dominated low-skilled and high-skilled jobs. The applications consisted of a short email, a cover letter, and a curriculum vitae (CV; a brief description of an applicant's education, qualifications, skills, and previous jobs). In order to convey that an applicant is a victim of crime to employers, victims' cover letter included a paragraph stating that they currently lived at a protected address and could therefore not reveal their residential address to the employer as they were under a serious threat. In the nonvictims' cover letter, a corresponding paragraph stated that the applicants did not provide a residential address as they were living with a friend during their search for a new home. Hence, the residential address of the job applicant was suppressed in all applications and the reasons for doing so were designed to implement our experimental conditions (victim vs. non-victim). The applications were sent between January and June 2016 to employers with job vacancies posted on the Swedish Public Employment Office website. Differences in the probability of receiving a job interview invite or a job offer from employers between the victim and nonvictim applicants were interpreted as discrimination.

This study fills an important knowledge gap in the literature on crime and labor market outcomes of victims. It highlights the role that the demand side of the labor market plays for the labor market outcomes of people who have been victims of crime. Specifically, it examines the hypothesis that victims are discriminated against in the labor market. The results of this study are important both from a theoretical perspective as well as from a practical and policy perspective.

Conceptual framework

Following Altonji and Blank (1999), we define labor market discrimination as a situation in which equally qualified and productive workers are treated unequally in hiring or pay on the basis of some characteristics, such as, race, ethnicity, or sex. In the present study, we focus unequal treatment on the basis of workers being victims or nonvictim. Provided that employers can distinguish between victims and nonvictims, else equally qualified and productive, we hypothesized that employers discriminate against workers who are or have been victims.

Using the economic notion of statistical discrimination (Arrow, 1973; Coate & Loury, 1993; Lundberg & Startz, 1983; Phelps, 1972; Rosén, 1997), we argue that victims face the risk of being treated differently compared to nonvictims in the labor market if employers ascribe undesirable group-level characteristics of victims to individual victims in the hiring process. The question is if victims as a group (on average) have some unfavorable characteristics that can influence employers when they draw inferences about an individual victim's productivity in a hiring situation. The answer is yes. Recent research shows that victims of assault face severe and long-lasting labor market consequences from victimization. Using rich administrative population data from Sweden, Ornstein (2017)

found that victims of assault experienced significant negative effects on their productivity. She showed that women, on average, lost 25% of their income, and men 14%, after being victims of assault. She demonstrated that the reduced incomes were largely a consequence of decreased probabilities to work: 11 percentage points lower probability to work for women and 7 percentage points lower probability to work for men, post-assault. Moreover, she showed that female and male victims of assault, on average, increased their sick leave with 31 and 15 days, respectively, per year. Most importantly, she showed that all of her observed effects were persistent during the time period that she studied (eight years post-assault) and that there were no tendencies that the effects she found would decline or cease over time.

Furthermore, research on intimate partner violence has shown that victims, on average, have lower self-reported productivity, are more absent from work, have higher unemployment rate, suffer from higher degree of tardiness, and supply less hours of labor to the labor market than comparable nonvictims (Reeves & O'Leary-Kelly, 2007; Staggs & Riger, 2005). Other types of victimization have also been shown to affect the victims' productivity and abilities to perform in the labor market (Kessler, 2000; Mezey, Evans, & Hobdell, 2002; Murphy et al., 1999; Resick, Calhoun, Atkeson, & Ellis, 1981; Yancey et al., 1994). Yancey et al. (1994), for example, found a positive relationship between violent trauma victimization and unemployment. All of these documented unfavorable group-level characteristics of victims constitute an information set that employers (under uncertainty about workers productivity) can utilize in order to make statistical inferences about an individual victim's productivity in a hiring situation. Statistical discrimination will then be present.

Besides testing for discrimination against victims, we also investigate how an applicant's sex and occupational characteristics affect the magnitude of discrimination. Specifically, previous field experimental research has shown that the size of labor market discrimination, based on some trait, may also depend on the applicants' sex (Bursell, 2014), whether an occupation requires high- or low-skilled workers (Ahmed & Lång, 2017; Carlsson & Rooth, 2007), and whether an occupation is dominated by men or women (Ahmed, Andersson, and Hammarstedt, 2013).

Christie (1986) proposed a set of characteristics that a victim should have in order for society to perceive the victim as "true" or "ideal" victim who deserves sympathy. Among other things, the victim should be a woman. In line with this idea, society should be less sympathetically disposed to male than female victims. Moreover, one of the most victimized social groups is people with a criminal background (Barnes & Beaver, 2012; Daday, Broidy, Crandall, & Sklar, 2005; Jennings, Piquero, & Reingle, 2012; Nilsson, 2002), and most criminal offenders are men (Olseryd, 2014). Thus, if discriminating employers believe that male victims are likely to be criminals as well, then employers may feel even lesser sympathy for male victims. We, therefore, believe that employer discrimination is more likely to occur against male than female victims, since male victims are less likely to receive sympathy and more likely to be perceived as criminals themselves than female victims.

The degree of discrimination may also vary with occupational characteristics. First, research has shown that criminal behavior and victimization are more common among groups of low socioeconomic status, which may, or may not, affect the size of discrimination across low- and high-skill occupations (Byrne, Resnick, Kilpatrick, Best, & Saunders, 1999; Cunradi, Caetano, & Schafer, 2002; Hjalmarsson, Holmlund, & Lindquist, 2015; Levitt, 1999; Thacher, 2004; van Wilsem, Wittebrood, & de Graaf, 2006). More importantly, there is

a large body of research that shows that a lower rate of education is associated with a higher degree of hostility and discrimination (Hagendoorn & Nekuee, 2018). The latter fact suggests that discrimination should be more widespread in low-skill than in high-skill occupations. Second, it is also possible that employers are less sympathetic to victims of the opposite sex. This would be the case if employers identify with victims of their own sex but not with those of the opposite sex. Previous studies have found that the sex of the observer as well as the sex of the victim affect the extent to which the observer assigns unfavorable characteristics to the victim (Grubb & Harrower, 2009; Savage, Scarduzio, Lockwood Harris, Carlyle, & Sheff, 2017). Hence, we expect that male victims are less accepted in female-dominated occupations than in male-dominated occupations and we expect that female victims are less accepted in male-dominated occupations than in female-dominated occupations.

Method

Measuring discrimination against victims

We sent matched pairs of fictitious applications to employers with available job vacancies for the same-sex victim and nonvictim job seekers. The pairs of job applications were carefully coordinated to be as similar as possible, but different enough to be seen as two distinct individuals. The two application templates were randomly assigned to the victim and nonvictim conditions for each job application, reducing potential bias from employers favoring one of the templates to the other. The order in which the two applications were sent was randomized across job vacancies. A positive employer response was defined as an applicant receiving a job interview invite or a job offer from an employer. We then interpreted differences in the probability of receiving a positive employer response between the victim and nonvictim applicants as discrimination.

Construction of the fictitious job applications

We constructed matched pairs of job applications for two male and two female fictitious job seekers. To avoid counterfactuals, and to signal the sex of the applicant, our constructed job seekers had similar and distinctively Swedish male and female names. We chose four of the most common Swedish female and male first names that we combined with two of the most common Swedish surnames to construct the following fictitious job applicants: Anna Eriksson, Sara Andersson, Daniel Andersson, and Johan Eriksson. All fictitious applicants were 28 years old, had a partner but no children, and were interested in cultural activities. To imply that the job seekers were healthy, the applicants also stated that they enjoyed recreation and physical activity. Depending on the required qualifications for each occupation, the applicants had between four and nine years of work experience and no long spells of unemployment. Each applicant was assigned a mobile phone number with a voicemail and an email account.

The job applications consisted of a short email, a narrative application letter, and a CV. The short email stated that the applicant was interested in the vacant position and had the narrative application letter and CV enclosed. The narrative application letter provided name, age, education, work experience, current employment, interests and leisure

activities. The CV provided contact information (email and mobile phone number), year of birth, a complete record of education, and current and previous employments. The CV also had information about the language and computer skills and indicated possession of valid driver's license.

Several measures were taken to make sure that the job applications were realistic. First, we followed the recommendations of the Swedish Public Employment Office when constructing the application letters and the CVs. Second, we studied real-life job applications and consulted experts within each occupation. Third, we conducted blind tests to ensure that the matched pair of applicants for each occupation was signaling two distinct individuals of similar abilities, work experience, and personal attributes.

The matched pairs of job applications were constructed for occupations within four occupational categories: female-dominated low-skilled jobs (cleaner, enrolled nurse, and restaurant worker), female-dominated high-skilled jobs (accounting clerk and preschool teacher), male-dominated low-skilled jobs (auto mechanic and truck driver), and male-dominated high-skilled jobs (salesperson and software developer). We classified the selected occupations into male- and female-dominated jobs based on the distribution of men and women across occupations in Sweden. The included female- and male-dominated jobs in the present study had a share of roughly 70–95% of women and men, respectively, in 2014 (Statistics Sweden, 2016a). We defined low-skilled jobs as those that do not require a university education and high-skilled jobs as those that do require a university degree.

Labeling victims

When labeling the victims, we wanted a crime that is common in Sweden and that a potential employer would find believable. We, therefore, chose one of the most commonly reported crimes in Sweden: “serious threat” (Command, Hambrook, Hvitfeldt, & Irlander Strid, 2016). Serious threat, or “unlawful threat”, includes all threats of criminal acts that evoke serious concerns for the victims, regarding their own safety or the safety of kin, friends, and property (Swedish Code of Statues, 1962, p. 700). People under serious threat are eligible for a “protected address” – a confidential or substitute address (Skatteverket, 2016; Statistics Sweden, 2016b). The victim status was revealed in the narrative application letter by adding the following paragraph (English translation):

I currently live at a protected address as I am under serious threat. This is why I have not provided a postal address. I would therefore ask that all correspondence be limited to email or phone.

This paragraph was excluded in the application letter of the nonvictim. Instead, to minimize any counterfactual effects of not providing a postal address in the victim's job application, we added the following paragraph to the nonvictim's application letter (English translation):

I am currently staying with a friend while waiting to move in to my own apartment. I therefore ask to be contacted by phone or email.

The experimental design of the present study measures potential discrimination when victim status is salient to the employer. But how realistic is this assumption? Since serious threat is one of the most common reasons for having a protected address in Sweden, we

argue the following: Even if victims are reluctant to reveal their status under other circumstances, it is reasonable to believe that victims would reveal their status in this case since providing a residential address is customary when applying for a job. It would also be plausible that victims would reveal their status if they believe that it would simplify future work relationships and potential remedial actions related to the work environment, or if they just want to be forthcoming with a prospective employer. This study investigates the consequences for victims if their status is salient to the employers. In other words, this study measures the cost of openly revealing victimization in the labor market.

Application procedure and protocol

To find job vacancies, we used the database Platsbanken on the Swedish Public Employment Office website. Platsbanken is the largest job-search channel in Sweden with about 30–40% of all job vacancies in Sweden being posted here (Siksjö, 2015). We applied to vacant positions between January 29, 2016, and June 3, 2016. For practical reasons, we limited the job applications to vacancies that accepted applications through email or simple online forms. Hence, we did not apply for jobs that involved complicated online application systems, which corresponded to about 10% of all advertised vacancies.

For each job that we applied for, we documented the applicant's sex, experimental manipulation (victim or nonvictim), and the version of the application template used for each applicant. We then documented the type of occupation, the date of the application, the city in which the position was advertised, if it was a full- or part-time position, and if it was a temporary or permanent position. Employer responses were received via email or mobile phone and whenever an applicant was invited to an interview or received a job offer it was recorded as a positive employer response. We quickly and respectfully declined all interview invitations and job offers to minimize the inconvenience to the employers. Description of the variables recorded and analyzed in this study is presented in Table 1. The data that support the findings of this study are openly available in Zenodo at <https://doi.org/10.5281/zenodo.2285765>.

Table 1. Description of variables.

Variables	Explanation
<i>Outcome variable (y)</i>	
Positive employer response	1 if an applicant received a promising response (job interview invite or job offer) from an employer, 0 otherwise
<i>Explanatory variables (x)</i>	
Victim	1 if the fictitious applicant was a victim, 0 otherwise
Female applicant	1 if the fictitious applicant was a woman, 0 otherwise
Female job	1 if the job belonged to a female dominated occupation, 0 otherwise
High-skill job	1 if the occupation required a university degree, 0 otherwise
<i>Control variables (c)</i>	
Full-time	1 if the job opening offered a full-time position, 0 otherwise
Tenure	1 if the job opening offered a tenured position, 0 otherwise
County fixed effects	1 if the job was located in a particular county (one dummy for each county), 0 otherwise
Season fixed effects	1 if the job was applied to in a specific month (one dummy for each month), 0 otherwise
Application template	1 if the first application package out of two matched ones was used, 0 otherwise
Application order	1 if a fictitious applicant (out of two) applied first to an employer, 0 otherwise

Female-dominated occupations were accounting clerk, cleaner, enrolled nurse, preschool teacher, and restaurant worker. Male-dominated occupations were auto mechanic, truck driver, salesperson, and software developer. High-skill occupations were accounting clerk, preschool teacher, salesperson, and software developer. Low-skill occupations were cleaner, enrolled nurse, restaurant worker, auto mechanic, and truck driver. Sweden consists of 21 counties. The experiment was conducted from January to June 2016.

Results

Descriptive statistics and main results

Table 2 presents the number of jobs applied to in each occupational category and the fraction of positive employer responses (job interview invite or job offer) for male and female applicants. We applied to 536 and 581 jobs for male and female applicants, respectively, corresponding to a total of 1,072 male applications and 1,162 female applications. The positive employer response rate for all applicants was 24 and 35% in low- and high-skilled occupations, respectively. Male applicants had a higher positive employer response rate in male-dominated occupations and female applicants had a higher positive employer response rate in female-dominated occupations.

Nonvictim applicants received 15% more positive employer responses than victim applicants. The difference in positive employer responses between the nonvictim and the victim varied by applicants' sex and occupational category. Male nonvictims had up to 28% more positive employer responses than male victims in female-dominated occupations, whereas there was no notable difference in response rate in male-dominated occupations. Female nonvictims had on average 16% more positive employer responses across all occupations than did female victims, and up to 27% more positive responses in female-dominated high-skilled occupations.

The ratios of relative positive employer responses between nonvictims and victims tabulated in Table 2 give indications of disparate treatment of victims and nonvictims in the Swedish labor market. However, to test if the differences in the positive employer responses are statistically significantly different, we rearranged the data to show the distribution of employer responses.

Table 3 tabulates the total number of employers in the sample ("Number of Employers") and the number of cases when neither the nonvictim nor the victim got a positive employer response ("Neither"), both the nonvictim and the victim got a positive

Table 2. Fractions of job applications that led to a positive employer response.

Occupation category	No. of jobs	All	Nonvictim	Victim	Ratio
Male Applicants					
Low-skill	350	0.21	0.22	0.19	1.16
High-skill	186	0.33	0.35	0.31	1.12
Female-dominated	369	0.21	0.24	0.19	1.26
Male-dominated	167	0.34	0.34	0.34	1.00
Female-dominated, low-skill	259	0.17	0.19	0.15	1.25
Female-dominated, high-skill	110	0.30	0.34	0.26	1.28
Male-dominated, low-skill	91	0.30	0.31	0.30	1.04
Male-dominated, high-skill	76	0.38	0.37	0.38	0.97
All occupations	536	0.25	0.27	0.23	1.14
Female Applicants					
Low-skill	376	0.27	0.28	0.26	1.08
High-skill	205	0.38	0.42	0.33	1.26
Female-dominated	379	0.32	0.34	0.30	1.13
Male-dominated	202	0.28	0.31	0.26	1.21
Female-dominated, low-skill	263	0.28	0.29	0.27	1.06
Female-dominated, high-skill	116	0.40	0.45	0.35	1.27
Male-dominated, low-skill	113	0.24	0.26	0.22	1.16
Male-dominated, high-skill	89	0.34	0.38	0.30	1.26
All occupations	581	0.31	0.33	0.28	1.16

Ratio is calculated by dividing the fraction of applications that led to a positive employer response for the nonvictim by the fraction of applications that led to a positive employer response for the victim.

Table 3. Distribution of employers' responses to fictitious job applicants.

Occupation category	No. of jobs	Neither	Both	Nonvictim	Victim	Net difference
Male Applicants						
Low-skill	350	258	53	25	14	-11
High-skill	186	111	48	17	10	-7
Female-dominated	369	269	56	31	13	-18***
Male-dominated	167	100	45	11	11	-0
Female-dominated, low-skill	259	201	32	18	8	-10*
Female-dominated, high-skill	110	68	24	13	5	-8*
Male-dominated, low-skill	91	57	21	7	6	-1
Male-dominated, high-skill	76	43	24	4	5	-1
All occupations	536	369	101	42	24	-18**
Female Applicants						
Low-skill	376	248	74	31	23	-8
High-skill	205	116	65	21	3	-18***
Female-dominated	379	231	93	35	20	-15*
Male-dominated	202	133	46	17	6	-11**
Female-dominated, low-skill	263	168	53	23	19	-4
Female-dominated, high-skill	116	63	40	12	1	-11***
Male-dominated, low-skill	113	80	21	8	4	-4
Male-dominated, high-skill	89	53	25	9	2	-7*
All occupations	581	364	139	52	26	-26***

"Neither" and "Both" are the number of cases in which both applicants failed and succeeded, respectively, and "Victim" and "Nonvictim" are the number of cases in which only the victim and the nonvictim, respectively, succeeded to receive a positive employer response. "Net Discrimination" is the difference between cases given in columns "Victim" and "Nonvictim". The symmetry between the number of employers who favored the nonvictim and the number of employers who favored the victim was evaluated using a two-sided binomial exact test. A corresponding McNemar test generates similar results.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

employer response ("Both"), and when only the nonvictim ("Nonvictim") or only the victim ("Victim") got a positive employer response. The last column ("Net discrimination") in Table 3 tabulates the difference between the number of exclusive positive employer responses received by the victim and nonvictim.

If employers did not differentiate victims from nonvictims, the number of employers who contacted only the nonvictim and the number of employers who contacted only the victim should be statistically equal. We, therefore, tested for the asymmetry between the number of employers who exclusively contacted the nonvictim and the number of employers who exclusively contacted the victim by applying a binomial exact test (the McNemar exact test yields similar results). Table 3 shows that we overall find evidence for discrimination against victims ("All occupations"), both among male and female job applicants. However, the degree of discrimination varied with the sex of the applicant and the occupational category. For male applicants, we find discrimination against victims in female-dominated occupations (both low- and high-skilled). For female applicants, we find discrimination against victims in all occupational categories, except in low-skilled occupations (both male- and female-dominated).

We can draw four conclusions from these results. First, there is discrimination in the Swedish labor market against victims. Second, employers discriminate against both male and female victims. Third, discrimination against male victims is concentrated in female-dominated occupations, but does not vary by occupational skill level. Fourth, and in contrast to the male victims, discrimination against female victims varies by occupational skill level, with discrimination being concentrated in high-skilled occupations.

Regression analysis

Table 4 presents the marginal effect of being a victim on the probability of receiving a positive employer response (job interview invite or job offer), estimated using probit regression models. All regression models include a dummy variable for the respective occupational categories, a dummy for full-time position, a dummy for tenure, county dummies, season dummies, an application template dummy, and an order of application dummy. Excluding these dummy variables or applying a linear or fixed effects regression model yields similar results to those presented here.

According to the result of the baseline specification (Model *i*), male victims had a 3 percentage points lower probability of receiving a positive employer response than male nonvictims. The corresponding estimate for female victims was a 5 percentage points

Table 4. Positive employer response probabilities estimated by probit regression.

<i>Variable</i>	<i>Model i</i>	<i>Model ii</i>	<i>Model iii</i>	<i>Model iv</i>	<i>Model v</i>
Male applicants					
Victim	-0.034** (0.016)		-0.035* (0.021)	0.003 (0.025)	-0.019 (0.039)
<i>Interaction terms</i>					
High-skill job			0.003 (0.033)		-0.033 (0.056)
Female job				-0.057* (0.031)	-0.078* (0.046)
High-skill female job					0.051 (0.089)
Observations	1,050		1,050	1,050	1,050
Pseudo R^2	0.089		0.089	0.090	0.090
Female applicants					
Victim	-0.048*** (0.016)		-0.025 (0.022)	-0.058** (0.026)	-0.017 (0.040)
<i>Interaction terms</i>					
High-skill job			-0.057* (0.029)		-0.080 (0.056)
Female job				0.015 (0.034)	-0.012 (0.051)
High-skill female job					0.042 (0.089)
Observations	1,146		1,146	1,146	1,146
Pseudo R^2	0.073		0.074	0.073	0.074
All applicants					
Victim		-0.036** (0.017)			
Female applicant		0.058** (0.027)			
<i>Interaction term</i>					
Female applicant		-0.011 (0.022)			
Observations		2,202			
Pseudo R^2		0.056			

This table reports the marginal effects between a victim and a nonvictim in the probability of receiving a positive employer response from an employer estimated using a probit regression model. A corresponding linear probability model generates similar results. The dependent variable is a positive employer response dummy. The interaction terms are interacted with the victim dummy. All models include dummies for occupation characteristics (female job and high-skill job), a dummy for full-time positions, a dummy for tenure, county dummies, season dummies, an application template dummy, and a dummy for the order of application. Descriptions of all variables are provided in Table 1. Twenty-two observations were dropped from the regressions involving male applicants, 16 observations were dropped from the regressions involving female applicants, and 32 observations were dropped from the regression involving both male and female applicants because of insufficient within-city and month variation in the victim variable. Reported standard errors (in parentheses) are corrected for clustering of the observations at the employer level.

*** $p < .01$, ** $p < .05$, * $p < .10$.

lower probability of a positive employer response. Although the degree of discrimination seemed to have been larger for female victims than for male victims, Model *ii*, which estimates the interaction effect between being a victim and a female applicant using the full sample, shows that this difference is not statistically significant. Hence, we cannot conclude that the degree of discrimination varied with the sex of the victim. The purpose of this study was not to investigate overall sex differences. Yet, Model *ii*, still reveals the interesting finding that female applicants had almost 6 percentage points higher probability of receiving a positive employer response than male applicants, a statistically significant result. Hence, female applicants were as a group favored by employers in our experiment.

Interacting the victim variable with a dummy for a high-skill job (Model *iii*) shows that the discrimination against male victims did not vary by occupational skill level. Conversely, discrimination against female victims was concentrated in high-skilled occupations, with close to a 6 percentage points lower probability of receiving a positive employer response. Interacting the victim variable with a dummy for a female job (Model *iv*) shows that male victims had a 6 percentage points lower probability of receiving a positive employer response in female-dominated occupations. Male victims were not discriminated against in male-dominated occupations. The interaction term between the victim variable and the female-job dummy for the female applicants is statistically insignificant. Hence, the discrimination against female victims did not vary across male- and female-dominated occupations.

In Model *v*, we include three-way interactions among the dummies for victim, female job, and high-skill job. The standard errors inflate so that most of the coefficients are no longer statistically significant. The interaction term high-skill female job is small with a large standard error for both the male and female samples, suggesting that discrimination against victims did not vary with the interaction of the occupational categories female job and high-skilled job. Taken together, male victims were less likely to receive a positive employer response in female-dominated occupations, regardless of their occupational skill level. Female victims were discriminated against in high-skilled occupations, and this effect did not vary significantly between male- and female-dominated occupations.

Discussion

This paper documents the results of a field experiment on labor market discrimination against victims in Sweden. We found support for the main research hypothesis of this study: employers discriminate against victims, if information about applicants being victims and nonvictims is made salient in the application letters. The difference in the magnitude of overall discrimination against victims between female and male applicants was not statistically significant. We did, however, find that hiring discrimination was affected by the sex of the applicant and occupational characteristics. We found that discrimination against female victims was concentrated in high-skilled occupations. Moreover, discrimination was lowest in male-dominated low-skilled occupations, with no discrimination against victims of either sex, and highest in female-dominated high-skilled occupations, with discrimination against victims of both sexes. Yet, discrimination against male victims generally did not vary by occupational skill level and discrimination against female victims did not vary between female- and male-dominated occupations.

The results presented in this study show that victims are victimized a second time when seeking employment. This raises the question: Why do employers treat victims differently from nonvictims? Based on the documented victim-offender overlap (Barnes & Beaver, 2012; Berg & Loeber, 2012; Daday et al., 2005; Jennings et al., 2012), one explanation could be that employers may believe that victims are involved in criminal behavior. Another explanation is that employers attribute undesirable average characteristics of victims as a group to individual victims, such as low productivity or high absenteeism. Employers may also be reluctant to hire victims, in particular individuals under serious threat, because of workplace safety concerns. For example, if employers believe that there is a risk that a perpetrator may seek up the victim at the workplace, they might be reluctant to hire the victim.

Regardless of where discrimination against victims stems from, the labor market disadvantage of victims can have significant negative consequences for the victimized person, as well as for society. For example, job security and employment are positively associated with psychological wellbeing (De Witte, 1999; McKee-Ryan, Song, Wanberg, & Kinicki, 2005). Employment status is also a predictor of revictimization, as in leaving abusive partners (Anderson & Saunders, 2003). Hence, employer discrimination against victims is likely to result in additional psychological distress for the victimized person and place him or her at increased likelihood of revictimization. Employer discrimination against victims is also likely to contribute to a decrease in societal welfare via lost human capital, higher unemployment, and an increased number of welfare recipients.

The observed hiring discrimination against victims in this study is important from a theoretical perspective, since neglecting lost labor market opportunities underestimates the true pecuniary and nonpecuniary costs of victimization. These results call for a deeper theoretical understanding of the linkages between the stigma of being a victimized person and employers' preferences and perceptions in hiring decisions. The results are also important from a practical and policy perspective, such as policy decisions regarding which groups should receive labor market interventions to improve job security and reduce unemployment. Furthermore, if people who have already been victimized fear being discriminated against in the labor market, it is possible that employer discrimination will make them less inclined to report a crime. This would not only be problematic from the perspective of the victim, but also from a crime preventing policy perspective.

The present study is one of the first attempts to directly test whether victims are discriminated against in the labor market. Yet, the experimental design of the present study has limitations that need to be addressed in future research. First, the correspondence test measures discrimination in the initial stage of the hiring process. The question is, however, how often information about being a victim is shared by victimized individuals. How realistic is our experiment? Would it not be more realistic if victims with protected address provide employers with some other explanation for not giving their address than openly declaring that they are victims? All these concerns are valid. We, however, argue that the experiment is realistic at least to some extent. It is reasonable to believe that victims would reveal their status of being victims since providing a residential address is customary when applying for a job, at least in the Swedish context. Having a private post box, for example, is not common at all in Sweden. It is also possible that victims reveal their status of being victims if they believe that it would simplify future work relationships or if they want to avoid stigma and discrimination in later stages of the hiring process or at the workplace. Furthermore, it is important to

remember that our purpose was to examine whether discrimination is present when employers know about an individual's victim status. Now, realistic or not, the results of this study definitely underline the importance of further investigating the difficulties that victims face in the labor market and specifically in the hiring process. If not in the initial stage of the hiring process, as in the present experiment, it is reasonable to believe that employers sooner or later in the hiring process or after recruitment would find out about workers' victim status. Hence, ideally, future research should investigate how victims are treated in later stages of the hiring process and in promotions.

Second, our study is limited to the Swedish or to some extent the North European context. It is unknown if, and how, our results would generalize to other job markets. For example, we have already mentioned that proving a resident address in a job application is customary in Sweden. Not doing so would be regarded as odd. Similarly, there is nothing strange about sharing information about your age, family, and interest in a cover letter in Sweden. This would not be the case, for example, in the U.S., as pointed out by one of the journal's reviewers. In the U.S., many job seekers would not include their age and they would be unlikely to share information about if they have a partner or children. Yet, in other countries than the U.S., it is common to include even more information in a job application than in Sweden. In Germany, for instance, it is normal to attach a picture of yourself to your résumé. Hence, only future research can tell if, and how, our findings extend to other markets.

Third, there is an underlying assumption in the current research design that stating that an applicant's address has to be protected due to a serious threat means a person is a crime victim. There is a possibility that an employer may have thought of other situations. Future studies could instead implement a research design where a stronger declaration of being a victim is used, for example, by stating "due to a recent victimization, my address needs to be protected at the current time." For the nonvictim applicants, it is possible that an employer might have questioned why a person would not allow a friend, who is residing at the home during a move, to provide their address. It is possible that an employer might have also made negative assumptions about the situation. Taken together, we may have underestimated the magnitude of discrimination against victims in the current study.

Fourth, even if there might be reasons for disclosing victim status in the application letter, an alternative explanation for observed differential treatment in our experiment could be that the disclosure of victim status itself puts employers off. Employers might be uncomfortable with that type of personal information in a job application. Employers may feel that the job applicant is oversharing and does not understand the difference between professional and personal matters. However, one could also argue the other way around. That by sharing personal information, such as victim status, the job applicant is being open, honest, and upfront, which may be appreciated by some employers. Also, if employers are put off by such personal information, why would such information be more or less personal in high- relative to low-skill occupations and in female- relative to male-dominated occupations. Only future studies can provide clarity to these questions. As suggested by one of the journal reviewers of this article, a direction for future research could be a more complicated experimental design where a job applicant discloses other personal information, such as that they have recently finished a contentious divorce, so are staying with friends while they wait to move into their new apartment.

To sum up. We have shown that when victim status is made salient in job applications, employers treat victims and nonvictims unequally in the first stage of the hiring process.

The unequal treatment varies with applicants' sex and occupational characteristics. Our findings underline the importance of studying the barriers victims face in the labor market. Yet, further research is needed to validate and generalize the results documented in this article, and to fully map and understand the labor market consequences of crime and criminal victimization.

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ORCID

Ali M. Ahmed  <http://orcid.org/0000-0002-1798-8284>

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