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ORIGINAL PAPER

Measuring the Effect of Probation and Parole Officers on Labor Market Outcomes and Recidivism

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Abstract
Objectives Use a unique dataset to pair probation and parole officers and their clients in Denmark in 2002–2009 to identify causal effects of these officers on labor market outcomes and recidivism.
Methods To identify these effects, we rely on data from all probationers and parolees in Copenhagen, where a rotational assignment process randomizes clients to officers. We apply OLS models to test whether the inclusion of probation and parole officer fixed effects improves model fit, and we show the impact of officer fixed effects by generating predicted values for one individual, varying only the officer.
Results The first stage of the analysis shows that the assignment of a probation or parole officer is indeed random in Copenhagen—at least in regards to the vast majority of background characteristics—suggesting that we are able to identify causal effects of probationer and parolee assignment on labor market outcomes and recidivism. The second stage of our analysis shows that although to a lower degree than common sense might suggest, probation and parole officers do matter for their clients' dependency on public benefit transfers (around 10 percentage points) and criminal recidivism (around 30 percentage points), whereas earnings are unaffected.
Conclusion As no study has yet to identify causal effects of probation and parole officer assignment, this study makes a novel contribution to the literature on the effects of criminal justice contact on subsequent life-course outcomes. Although generalizability to the US context is uncertain because we rely on Danish data, our findings nonetheless point in interesting directions for future research.

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Introduction

Most recent research on the effects of contact with the criminal justice system emphasizes the effects of incarceration for individuals (e.g., Lopoo and Western 2005; Massoglia 2008; Massoglia et al. 2013; Pettit 2012; Schnittker and John 2007; Turney et al. 2012; Western 2006) and those tied to them (e.g., Clear 2007; Wakefield and Wildeman 2013; Wildeman et al. 2012). On the one hand, this emphasis makes good sense, as around 1.6 million Americans were incarcerated in a state or federal prison at the end of 2011. Yet on the other hand, focusing solely on the penal system seems to miss the mark, as it neglects to consider the 4.8 million individuals who were under community supervision—on probation or parole—at the end of 2011 (Maruschak and Parks 2012; Makarios et al. 2012), and completely neglects earlier criminal justice contacts that are both common (e.g., Brame et al. 2012) and consequential for future life-course outcomes (e.g., Kirk and Sampson 2013), such as arrest.

Of these oversights, maybe the most important is the inattention to the effects of probation and parole on individuals, as these forms of community supervision have become so common in recent years.

Despite a tendency towards policies that favor noncustodial punishment over incarceration, we currently know much more about the effects of incarceration than the effects of noncustodial alternatives like community supervision (for thorough discussions of this imbalance, see Phelps 2013; Seiter and West 2003). And this is notably true regarding the effect of probation or parole officers on their clients, a significant gap in the literature since this effect is likely to be of great importance for criminal justice outcomes, as well as broader labor market outcomes. This gap in our knowledge is especially stymying as literally millions of individuals are placed on probation or parole every year in the United States, as mentioned, and as the use of community supervision more than doubled over the past 25 years.

Probation and parole officers are charged first and foremost with making sure that their clients neither commit new crimes nor violate the conditions of their probation or parole by committing technical violations. Yet beyond these criminal justice roles, parole and probation officers also serve a social welfare function, helping their clients obtain employment, find suitable housing, facilitate communication with social or unemployment offices to obtain welfare or unemployment benefits, and, on a therapeutic level, discuss the risk of criminal recidivism and strategies for the probationer or parolee to cope with this risk.

The probation or parole officer thus plays a dual role, one of social worker and criminal justice employee (West and Seiter 2004), case worker and supervisor (Seiter and West 2003), or, more generally, one of support and control (Glaser 1969; Klockars 1972; Gayman and Bradley 2013). The probation or parole officer performs various tasks to assist the probationer or parolee and also has discretionary powers regarding the probationer or parolee. Given this important dual role, it therefore seems highly likely that differences between individual probation or parole officers matter for the treatment probationers and parolees get (Andrews 1980, 2011; Andrews et al. 2011; Bonta et al. 2008; Grattet et al.

2011; Palmer 1995; Paparozzi and Gendreau 2005; Schwalbe and Maschi 2009; Seng and Lurigio 2005; Skeem et al. 2007; Taxman 2008).

Existing research, quantitative and qualitative alike, also suggests that probation and parole officers matter for their clients. Studies link probation and parole officer attitudes to a host of outcomes including pre-sentence recommendations (Katz 1982; Rosecrance 1985, 1987), job task selection (Clear and Latessa 1993), acceptance of agency directives (Lynch 1998; Makarios et al. 2012; Sigler and McGraw 1984), and supervision practices (Seng and Lurigio 2005; Steiner et al. 2011; West and Seiter 2004). In a similar vein, research also suggests that the amount of training a probation or parole officer has received is of the utmost importance for his or her clients (Bonta et al. 2008; Dowden and Andrews 2004; Lowenkamp et al. 2014; Oleson et al. 2012; Robinson et al. 2012; Trotter 1996) and that probationer or parolee outcomes like criminal recidivism are influenced by their probation and parole officers (Andrews 1980; Andrews et al. 1990; Bourgon and Gutierrez 2012; Huebner and Berg 2011; Jalbert and Rhodes 2012; MacKenzie et al. 1999; MacKenzie and De Li 2002; Paparozzi and Gendreau 2005; Steiner et al. 2012; however, see also Horney et al. 1995).

Qualitative inquiries further buttress the case for probation and parole officers affecting client outcomes by describing just how much it matters for probationers and parolees which probation or parole officer they are assigned to. Lynch (1998) shows how probation and parole officers may be viewed as policy interpreters who adjust agency directives according to what they believe is more suitable for any given client, which indicates that an officer's interpretations of the client's situation and needs matter for the treatment that the client actually receives. In a similar vein, Hanrahan (2005) finds that the attitude or style of the individual probation or parole officer matters profoundly for their clients:

> My parole officer over there, he was pretty cool, you know? He was like, "Let's get you in college, let's do things that will help you get ahead in life. Let's find you a job, let's find you this, let's do that." You know? He was there, on point. And then the parole officer in the halfway house, was, I mean, he was arrogant, he was like, "Do this, do that. Just get it done." It's not like he tried to help me get a job or gave me advice to go to college or anything. He just said, "Do this. Here's what you got to do." It was like, you know, he was a machine, and I was the, ah, you know, the receiver of that message. (Respondent from Hanrahan 2005:262)

Thus, existing quantitative and qualitative research suggests that the assignment of a particular probation or parole officer to a client is likely to shape the client's chances of achieving successful reintegration, as common sense also directs us to expect. Unfortunately, none of the existing research on how probation and parole officers affect their clients has used a research design that is capable of producing causal estimates of the effects of a specific probation or parole officer on a client's outcomes. We therefore do not know whether such effects are driven by a causal effect of that probation or parole officer or selection—possibly on the basis of the best probation and parole officers getting matched with the most promising clients, for instance. And, thus, even though both existing research and common sense suggest that probation and parole officers matter for their clients, it remains unclear whether they do in fact matter—and if they do, exactly how much.

In this article, we fill this gap in the literature by analyzing the causal effect of probation and parole officers on their clients' subsequent labor market outcomes and probability of criminal recidivism. To reach this end we, as the first article we are aware of to do so, pair individual identifiers for all probation and parole officers with their clients in Denmark in 2002–2009 to produce a unique dataset that allows us to identify causal effects of probation and parole officer assignment on various outcomes in a subsample where a rotational assignment process nearly randomizes clients to officers (and compare these effects to a different subsample that does not include a rotational assignment process).[1] Consequently, this article offers knowledge on the causal effects of probation and parole officer supervision on various outcomes—labor market outcomes and recidivism—just as it reveals the magnitude and nature of selection issues that hamper direct interpretations of the link between officers and their clients.

Data, Measures, and Analytic Strategy

The Danish Context

Although the scale and rate of change in the level of community supervision in Denmark is dwarfed by those in the United States, Denmark witnessed a dramatic increase in the use of community supervision over the past 25 years. And, indeed, the number of people under community supervision more than doubled in Denmark from 1985 to 2011, and today 17 per 10,000 of the 5.6 million Danes are under community supervision (compared to 153 people under community supervision per 10,000 of the 315 million Americans). Being a small country with a comparatively low use of community supervision, the absolute increase in the number of people on community supervision was thus small relative to the US, and in Denmark, this number went up from 4,100 in 1985 to 9,300 in 2011—a 128 % increase—compared to an increase from 2.15 million in 1985 to 4.80 million in 2011 in the US, a 124 % increase. The trends in the use of community supervision are hence comparable (in the sense that the percentage change is similar), yet while the change in the US is driven by increases in both probation and parole, the Danish increase is strictly attributable to an increase in the number of probationers.

Unlike the American system, where there is a strict distinction between jail (typically sentences of less than a year) and prison (typically sentences of more than a year) incarceration, the Danish system does not have a strict divide. Sentences in Denmark are generally shorter than in the US, furthermore. As many as 85 % of all sentences in Denmark are shorter than 1 year and <7 % are longer than 2 years (2011 levels). In contrast the mean sentence length in the US is around 4.7 years for federal prisoners (2008–2009 levels) and 2.1 years for state prisoners (Danish Prison and Probation Service 2012a; Guerino, Harrison, and Sabol 2012; Motivans 2012). Despite these differences, putting individuals on probation or parole serves much the same purpose in both countries—to divert individuals who could be sentenced to (or have already served time in) a correctional facility while still keeping them under criminal justice supervision in the hopes of minimizing crime while also minimizing costs.

Prisoners sentenced to more than 3 months of imprisonment in Denmark are expected to achieve early release on parole upon having served two-thirds of their sentence, provided that at least 30 days of the sentence remains to be served. However, to promote order

[1] In this regard, our manuscript fits nicely with other recent research using rotational assignment to isolate causal effects (for four especially exceptional examples, see Abrams and Yoon 2007; Doyle 2007, 2008; Kling 2006).

inside the prison and provide an incentive for inmates to use re-socializing initiatives (like pursuing education) while being imprisoned, it is possible to achieve early release on parole after serving half the prison sentence if an inmate shows devotion to achieving re-socialization.[2] In this sense, the time at which prisoners in Denmark become eligible for parole is not that different than in the United States. Typically, the parole entails a 2 year period during which any violation of the rules that accompany the parole will result in re-imprisonment.

Probation in Denmark, as in the United States, is an alternative to imprisonment that imposes a number of rules and regulations on the offender. For example, during probation an offender might be subject to drug and alcohol tests, restrictions on employment, and unannounced visits to control whether the requirements of the sentence are followed. In Denmark, probations typically entail a 1 or 2 year trial period. By law the probation or parole officer is required to meet with the probationer or parolee every 2 weeks during the first 2 months, and once a month thereafter. At the first meeting—which should take place within the first week following release for parolees, and within the first 2 weeks following conviction for probationers—central aims regarding reintegration should be agreed upon.

There are important differences between the amount of discretion probation and parole officers have in Denmark and in the US. In the US the conditions of probation and parole are often so many that the probation or parole officer could seemingly revoke probation or parole whenever he or she wishes to do so, as there are so many conditions to satisfy that the officer can almost always find some technical violation to report. Probation and parole officers in the US thus have a high amount of discretionary powers. But in Denmark discretion is lower and many rules regulate the supervision that probation and parole officers perform. Probation and parole officers in Denmark do have discretionary powers, for example when they are on a home visit or during meetings with probationers and parolees, but regarding the revocation of probation and parole for technical violations, such a decision has to be validated at court to shield the probationer or parolee against the untimely use of discretionary powers by the probation and parole officers. Also, the central aims of probation and parole in Denmark are, as mentioned, agreed upon during their first meeting, which in turn might lead to fewer revocations. As the amount of discretionary powers assigned to the probation and parole officers is lower in Denmark than in the US, we may assume that probationers and parolees receive more uniform supervision in Denmark, and that the effects of probation and parole officers that we measure in this research article would perhaps be even bigger in a US sample, although this is, of course, an empirical question in need of testing.

The distinction between probation and parole officers is somehow artificial in Denmark, as neither wears a uniform nor carries a gun, and both are more likely to be social workers rather than former police officers. In this sense, we might expect the parole supervision that Danish parolees experience to be similar to what Danish probationers experience. Naturally, this challenges the possibility for generalizing our results regarding Danish parole officers and parolees to American parole officers and parolees, a challenge that we discuss in our conclusion. Yet since both the correlation between probation or parole officer assignment and client outcomes and the causal effect of this assignment on client outcomes has yet to be analyzed, this article makes a significant contribution to the understanding

[2] These rules were in effect from 2004 onwards. Before 2004 inmates were expected to serve their full sentence while good behavior could earn them an early release on parole after having served two-thirds of a sentence.

and awareness of the effect of probation and parole officers on their clients, national context notwithstanding.

Most social workers in Denmark supervise both probationers and parolees, and probationers and parolees are subject to the same treatment in terms of control and support as exerted by their probation or parole officer. Therefore—and because probationers and parolees in Copenhagen are assigned to officers following the same rotational assignment process that may be exploited for causal inference, a point we return to—this article assumes the officer fixed effect to remain identical across probationers and parolees once we control for sanction type, probation or parole, and during the course of the article we will refer to probation and parole officers jointly as 'officers', and probationers and parolees jointly as 'clients'.[3]

Data

For this article, we combined data from the Danish Prison and Probation Service with registers of the Danish population, which are available from Statistics Denmark, to produce a unique dataset that allows us to analyze the magnitude of probation and parole officer fixed effects on client outcomes. From the Danish Prison and Probation Service, we obtained unique IDs on all probation and parole officers in Denmark in 2002–2009 along with personal identification numbers on their clients, which makes the clients linkable to the full population registers at Statistics Denmark, which then allowed us to add a range of information on each client both prior to (e.g., controls) and following (e.g., outcomes) supervision.

Our final analytic sample includes 19,534 probationers and parolees along with the 371 probation and parole officers they are assigned to. All supervision cases are initiated and terminated between 2002 and 2009. The raw sample had 53,814 probationers and parolees. We excluded 18,505 drunken drivers, mentally ill offenders, and sexual offenders since such offenders receive their treatment outside the Prison and Probation Service premises and therefore remain unaffected by their probation or parole officer. We have also excluded clients who change their probation or parole officer during their case, as it remains unclear which officer they should be attributed to (which excludes 1,169 cases).[4]

[3] Several observed as well as unobserved differences are likely to exist between probationers and parolees, and our choice of analyzing them as a single category is thus debatable. We have chosen to analyze probationers and parolees simultaneously, as we find that this strategy is closer to the empirical reality in Copenhagen, where we obtain our data for causal inference from. Also, as the assignment of officers to clients in Copenhagen is random, which we show that it is close to, the consequences of observed as well as unobserved differences between probationers and parolees should be netted out due to the random assignment. We have, however, performed all analyses for the two groups separately to see whether one or the other group drives our main results. Results showed that the separate assignment of probationers and parolees to officers is as close to random as when they are treated together, and that the consequences for the main results of analyzing the two groups separately are few (all results are substantially the same, except that the effect of officers on probationers' dependency on public benefit transfers in the Copenhagen sample is statistically insignificant for probationers, whereas it is significant for parolees).

[4] Those who change officer during supervision represent a highly selected group, and we have chosen to exclude them from our analyses to focus on the overall implications of probation and parole officer assignment for more standard probationers and parolees. Comparing the covariate distribution of those who are supervised by one officer and those who change officers during supervision also point to such selection issues, as those who change officers are generally worse off. They are older and a higher share are of ethnic minority background, they have lower education and lower prior earnings, and they have more previous crimes and a higher risk of previous imprisonment in the national sample. In the Copenhagen sample, they are more likely to have children, they have lower education and lower prior earnings, and they have a higher

We have further kept only clients younger than 65 years (which excludes 76 clients) and excluded both clients and officers in cases where clients are assigned to officers with less than five total cases over the study period (which excludes 98 officers and 196 cases). Our sample then includes 371 officers who have between 5 and 225 clients, with the mean number of clients being 52.7. A total of 7,245 cases do not match a probation or parole officer and are hence excluded.[5] For clients with more than one entry during the period (with several separate probation or parole cases over our observation period), we kept only the first entry, to avoid clients receiving multiple treatments (which excludes 3,085 cases). Also, we drop 94 parolees because their initial imprisonment date is earlier than what we have registers available for (see below). The final analytic sample thus includes around 35 % of the raw case stock, yet if we disregard the exclusion of clients who are in fact not treated by probation or parole officers (drunken drivers, mentally ill offenders, and sexual offenders), we still have 55 % of the relevant client stock. Importantly, these 55 % of the relevant client stock are chosen so as to make the sample optimal for analytic purposes and to avoid serial correlation between observations of the same client across different officers.[6]

Measures

Dependent Variables

In line with most studies that investigate the effects of criminal justice supervision, we focus on the effect of officer supervision on labor market outcomes and criminal recidivism. Specifically, we analyze three outcomes, of which two are labor market outcomes and one is criminal recidivism. The labor market outcomes are earnings and the rate of dependency on public benefit transfers. Earnings are measured during the first full calendar year following supervision initiation, and the dependency rate is measured during the first full year following supervision initiation. Specifically, earnings are all wages from legitimate employment, and the rate of dependency on public benefit transfers indicates the share of all weeks during the first year following the week the supervision started that the

Footnote 4 continued
risk of previous imprisonment. When re-running our analyses while including those who change officer during supervision, the only changes to our results are that whereas officers do not matter significantly for dependence on public benefit transfers in the Copenhagen sample, they matter even more for criminal recidivism in the Copenhagen sample.

[5] We are able to identify valid covariates on 6,226 of these 7,245 excluded cases, and the covariate distribution among these 6,226 of the excluded cases is roughly identical to that of our final analytic sample. The three largest dissimilarities are regarding the share that is convicted of violence, property crimes, and drug related crimes, 0.191/0.247, 0.048/0.089, and 0.050/0.082 for dropped cases/analytic sample. And even though the number of observations is high, we only find seven differences between the excluded cases and the analytic sample that are significant at the 0.1 % level, none at the 1 % level, and three at the 5 % level. This suggests that the "error" of unmatched clients is not substantial, and our final results are hence little likely to be biased by this case exclusion.

[6] Among the 15,775 observations that are excluded from the raw sample (excluding drunken drivers, mentally ill offenders, and sexual offenders), we find information on 10,070 in the register. Testing the covariate distribution of these excluded observations against the analytic sample reveals how more are on parole, less are convicted of violence, they earn less, and have heavier criminal backgrounds, just as their criminal recidivism rate is higher than in the analytic sample (these are the substantial differences, due to a large *n* other significant differences prevail too). These differences should come as little surprise, as the excluded observations are on for example offenders who return to parole or probation after having endured the supervision in our analytic sample.

client received social welfare benefits. Both variables are continuous, yet while earnings is zero or greater for all, the dependency rate falls between zero and one: zero indicates that the client did not receive any subsidized income during the first year since supervision started; one indicates that the client depended on public transfers during all weeks of the first year; and any number in between indicates the share of the first year the client depended on public transfers. Our criminal recidivism outcome measures reconviction during the first two years from the date supervision starts. Criminal recidivism is thus a binary indicator variable taking the value one if a probationer or parole is reconvicted during the follow up period and zero otherwise, with technical violations of probation or parole also included in this measure.[7]

Explanatory Variables

Our main interest is in the individual probation or parole officer's fixed effect on these outcomes. Typically, fixed effects are used to absorb variance that researchers know is important but are not interested in, as when we, for example, use state or year fixed effects. In our study, however, we are interested in the fixed effects themselves in two ways—what they mean for model fit (e.g., if they improve our predictive power) and how they are distributed (e.g., how exactly they matter).

As mentioned earlier, our data contain unique IDs on all 371 parole and probation officers in the sample, which allows us to model the relationship between each officer and his or her clients' outcomes. Specifically, we produce a dummy variable for each officer (except one) and enter these into our models. Our reference officer is from Copenhagen and has 104 clients, 70 probationers and 34 parolees, so the reference officer is the same for our analysis of the entire country (where we have 370 dummies) and of Copenhagen (where we have 35 dummies). In the analytic strategy section, we provide a description of our modeling strategy, along with the distinction between the full sample and the Copenhagen sample.

Control Variables

From the register available from Statistics Denmark, we add a range of control variables to our sample of probationers and parolees. First, we add basic information such as age, sex, marital status, whether or not the client has children, how many years of education the client has, and an indicator for ethnic minority background, which indicates whether the individual is an immigrant or the child of an immigrant (relative to being a native Dane). This information is measured just before the supervision is initiated. Second, we add information on the case that the client is under supervision for, namely offense type and trial period, and whether the client is a probationer or a parolee. Third, we add labor market

[7] We include technical violations in our criminal recidivism outcome variable for three reasons. First, getting probation or parole revoked for technical violations is highly consequential for the client's future, and being assigned to an officer with a higher or lower propensity for revoking probation or parole for technical violation will thus matter profoundly for the offender. Second, since getting probation or parole revoked implies being transferred back to prison, not counting technical violations as recidivism would downsize the recidivism rate of our sample due to the incapacitation of the highest risk offenders. Third, technical violations by definition means that the offender did not live up to certain rules and conditions stated by the criminal justice system, and technical violations thus implies breaking the law. We have, however, also run our analyses while excluding technical violations from the recidivism outcome variable, and this did not alter our results substantially.

outcomes prior to the case. We add prior earnings measured during the year before supervision for probationers and imprisonment for parolees, and prior dependency on public transfers measured during the year before the date of supervision for probations and imprisonment for parolees.

Notice that we measure prior earnings and prior dependency on public transfers before the parolee's imprisonment rather than before their parole since we do not expect prison inmates to have earned income nor dependency on public transfers while incarcerated. This, however, means that for some parolees we measure these variables long ago, and since the registers that contain dependency on public transfers exist only from 1985 onwards, we drop parolees imprisoned earlier (which drops only 94 observations). If we were to measure the labor market attachment and income of parolees the year before their parole supervision starts, we would thus get slightly more observations, yet many of these could not be expected to have earned income nor dependency on public transfers due to being imprisoned. Last, we add the number of previous convictions and a dummy indicator for any previous imprisonment.

Table 1 shows descriptive statistics for the full sample and the Copenhagen sample. We show these descriptive statistics on both the full sample and the Copenhagen sample because we only have random assignment of officers to clients in the Copenhagen sample, and only results from this Copenhagen sample are suitable for causal inference. We explain this in detail in the next section.

In the national sample, three out of four are on probation, and the table shows how the probationers and parolees have poor socioeconomic backgrounds. For example, the mean education length is 10.1 years, which is low considering that mandatory education in Denmark is 10 years. The probationers/parolees were on average depending on public benefits 15 % of the year preceding their supervision/imprisonment. The distribution of outcome variables tells the same story, and although the mean earned income during the first full calendar year following supervision is close to DKK 90,000 (which is higher than the mean income before supervision/imprisonment), around 43 % recidivate during the first 24 months following the day their supervision starts—which corresponds to the recidivism rate reported by the Danish Prison and Probation Service (2012b) on probationers and parolees.

In Copenhagen, more clients are on parole, and clients in Copenhagen fare worse in terms of socioeconomic background, which corresponds to anecdotes among the probation and parole officers that the Copenhagen department generally gets worse clients than the rest of the country. Disregarding anecdotes, substantial differences exist between the Copenhagen sample and the national sample.

Finally, we have calculated intra class correlation coefficients for each of the outcomes by officers. These coefficients provide a feel for the amount of variation in the outcome variables that is attributable to the probation and parole officers rather than to the individual clients themselves. These calculations reveal that around 5, 1, and 2.5 % of the variation in earnings, dependency on public benefit transfers, and criminal recidivism is attributable to differences between officers in the national sample, which indicates that officers may matter, whereas the same numbers for the Copenhagen sample are only 0.5, 0.3, and 1.4 %, which indicates that officers may not in fact greatly shape their clients' outcomes. The discrepancy between the coefficients from the national sample and the Copenhagen sample underlines that severe selection issues contaminate the causal relationship between officers and clients in the national sample, just as it shows that even though probation and parole officers might matter for client outcomes, the causal impact of

Table 1 Descriptive statistics

Sample	National		Copenhagen	
Variable	Mean	(SD)	Mean	(SD)
Supervision length	18.395	(7.650)	19.720	(8.515)
On probation	0.751	(0.432)	0.550	(0.498)
On parole	0.249	(0.432)	0.450	(0.498)
Violence	0.247	(0.431)	0.184	(0.387)
Drug related crime	0.082	(0.274)	0.088	(0.284)
Property crime	0.089	(0.285)	0.121	(0.327)
Theft	0.380	(0.485)	0.383	(0.486)
Other crime type	0.166	(0.372)	0.179	(0.384)
Age	29.601	(10.665)	30.516	(10.088)
Female	0.124	(0.329)	0.126	(0.332)
Unmarried	0.868	(0.339)	0.884	(0.321)
Has children	0.293	(0.455)	0.207	(0.405)
Ethnic minority	0.145	(0.352)	0.309	(0.462)
Years of education	10.070	(2.009)	9.878	(2.069)
Prior earnings[a]	74.776	(108.903)	37.262	(78.082)
Prior dependency on public transfers	0.148	(0.299)	0.151	(0.320)
Number of previous crimes	5.553	(7.036)	9.952	(10.680)
Previously imprisoned	0.361	(0.480)	0.368	(0.482)
Outcomes				
Earnings[a]	86.426	(120.837)	41.033	(86.084)
Dependency on public transfers	0.169	(0.318)	0.182	(0.343)
Criminal recidivism	0.425	(0.494)	0.562	(0.496)
PO N	371		36	
N	19,534		2,012	

[a] Earnings are deflated to 2005 level and reported in DKK 1,000 (DKK 1,000–USD 165 in mid-2005)

officers on clients might be less important than common sense and existing studies—which do not have random assignment of officers to clients—directs us to expect.

Analytic Strategy

When data are hierarchical in nature, as in our data where clients are nested within the officers that they are assigned to, it is standard to apply a multilevel model. The main advantage of multilevel modeling is that it allows the researcher to explicitly take into account that observations nested within the same higher-level unit cannot be said to fulfill the assumption of independence that is fundamental to statistical test theory. Since data are hierarchical, the argument goes, each additional observation does not provide a unique additional piece of information, due to the dependency among observations within higher-level units, like officers, and standard errors therefore become too small and it becomes easier to obtain statistical significance. The solution to this caveat would be to explicitly model the deviance between officers as a variance component around the officers' grand mean—and significant variability between officers would then be interpreted as the distribution of effects of individual officers on their clients.

But even though the multilevel model might be more parsimonious than the fixed effects approach—which is the approach we apply—because fewer parameters are estimated in multilevel models, this comes at the price of precision in estimates. Having less than 50 higher-level observations leads to biased estimates of the higher-level standard errors in both linear (Maas and Hox 2005) and nonlinear multilevel models (Paccagnella 2011), just as higher-level slopes may be unreliable due to influential observations, especially when sample size is low (Van der Meer et al. 2010). As we have only 36 officers in the Copenhagen sample, which we use for causal inference as officers are (almost) randomly assigned to clients in this subsample, we consider the risk of imprecision to be of vital importance.

Our approach is to analyze the effect of probation and parole officers on labor market outcomes and recidivism in five steps. Central to these steps stand our outcome model that regresses the dependent variables on the control variables along with year fixed effects and our explanatory variables of interest, the officer fixed effects. For model diagnostics and interpretations of the individual residual, we estimate the model using OLS with standard errors clustered at the officer level, since this estimator estimates the residual variance from data rather than assuming an error term distribution. However, to avoid predictions outside the [0:1] interval, we use the logit estimator to predict binary outcomes when we apply and interpret model predictions (with standard errors again clustered at the officer level).

First Step

The first step is to show that by adding officer fixed effects to the model we improve model fit, which implies that there are systematic differences between client outcomes based on which officer the clients are assigned to. Empirically, we apply two tests of the importance of officer fixed effects. First, we re-estimate the model without the officer fixed effects and use the F-test statistic to test the joint significance of the excluded fixed effects. Second, because the causal relationship between officers and their clients appears to be weak—as was indicated by the low intra class correlation coefficients that we already discussed—we supplement the F-tests with significance tests of the officer fixed effects Partial R-squared, as developed by Shea (1997) and implemented by Davis and Kim (2002).[8]

However, for significant tests to imply the existence of a causal relationship between officer assignment and client outcomes the assignment of officers to clients should be random. In a small country with comparatively few cases of probation and parole, such as Denmark, each officer typically gets all cases within one or more municipalities. Some municipalities are of course larger or have the larger cities in them, and therefore also have more clients and more officers, but generally one or a few officers suffice to perform the needed supervision within municipalities. Because parole and probation officers cannot be randomly assigned in small municipalities, as one and the same officer is assigned to all cases, we cannot differentiate the causal effect of this officer assignment from features of those municipalities. Thus, although analyses of the entire country provide an important first step in considering the effects of probation and parole officers on clients, they cannot provide an uncontaminated estimate of these effects.

[8] Also see Davis (2008) for the correct distribution of the Partial R-squared test statistic.

Second Step

In order to deal with this obstacle to causal inference, we show that a rotational officer assignment process in Copenhagen leads to (mostly) random assignment of officers to clients, meaning that estimates from Copenhagen will not be contaminated by endogeneity bias. Here the case and officer stock is greater and whenever a new probation or parole case finds its way to the Copenhagen department, an administrative assistant registers basic information on the case and assigns it to the next officer in line who then receives notification. The administrative assistant does not distinguish between probation and parole cases in her rotational assignment process and she thus assigns both case types to all officers, which is why we do not distinguish between probation officers and parole officers in our analyses.

To show that the Copenhagen rotational assignment indeed randomizes officers to clients, we regress each of the client characteristics on the officer fixed effects, along with year fixed effects. If we do have random assignment of the characteristic in question across officers, an F-test of the joint significance of the officer fixed effects should be statistically insignificant. We also show how the assignment of officers to clients in the national sample (excluding Copenhagen) is far from random. We show this with significant F-tests of the joint significance of officer fixed effects on all client characteristics in a similar model that instead of year fixed effects also includes department fixed effects. While our analysis of Copenhagen allows us to generate an estimate of the effect of probation and parole officers on clients, the national analysis shows results that are hampered by non-random assignment of officers to clients.

Third Step

The third step of our analysis is to show that in the Copenhagen subsample, where assignment is random, the inclusion of officer fixed effects improves the model fit of our outcome model. We apply the same tests as in the first analytical step.

Fourth Step

The fourth step of our analysis moves beyond statistical significance and describes the substantive importance of the officer fixed effects. Here, we consider how the officer fixed effects are distributed by outcome variable, by depicting the prediction of each outcome as it varies by officer. For these predictions, we use a fictional observation that is a probationer with 0 on all binary controls and the Copenhagen subsample mean on all continuous control variables, and we artificially assign this fictional observation to all officers in the sample. We show these results for both the officers in the national sample and the officers in the Copenhagen sample. The graphs from the Copenhagen subsample shows the uncontaminated effects of officers on clients, while the national sample shows estimates hampered by non-random assignment.

Fifth Step

The fifth and last step of our analysis investigates whether some officers should be viewed as consistently "good" or "bad" at their job. Put directly, we wish to investigate whether one can extrapolate from the distribution of officer fixed effects on one outcome variable to know whether an officer is generally "good" or "bad" on other outcomes. To investigate this, we calculate the correlation between the different outcome predictions of the fictional observation by which officer he is assigned to. If some officers are consistently "good" or "bad" at their job, this correlation should be sizeable.

Results

National Sample Results

Table 2 shows how our outcome model is improved by including probation and parole officers in the model, which implies there are systematic differences between client outcomes based on which officer they are assigned to. More precisely, the table shows model fit diagnostics, F-tests, and the Partial R-squared for the full sample with each outcome variable. The inclusion of officer fixed effects significantly improves the model fit (*p* values for all F-tests of excluded officer fixed effects are highly significant, and the Partial R-squared exceeds the critical value [0.020] for earnings [0.042] and criminal recidivism [0.024]—and is close to doing the same for dependency on social welfare benefits [0.020]). The model R squared increases by between 0.01 and 0.02 for all outcomes, and even though this increase is not an overwhelmingly large one, it still corresponds to a model fit improvement in the area of 2–8 %. Essentially, then, the inclusion of officer fixed effects in our model enhances our understanding of the life chances of probationers and parolees, even though the majority of variation in the outcomes is still attributable to the probationers and parolees themselves.

Officer Assignment

The inclusion of officer fixed effects significantly improves the model fit in the full sample, yet since the officer fixed effects are conflated with municipality of residence, it remains unclear whether this result shows the true and unbiased effect of probation and parole officers on their clients. However, the rotational officer assignment process in Copenhagen provides such a possibility, and Table 3 therefore reports the F-tests for the joint significance of the officer fixed effects when regressed on each of the client covariates in both the Copenhagen sample and in the national sample excluding Copenhagen. The table tells three key stories. First, in the national sample, the assignment of officers to clients is far from random, and in fact the only covariate that is not correlated with officer assignment is the client's sex. The results on the importance of probation and parole officers reported in Table 2 are hence subject to severe selection mechanisms that pair certain types of officers with certain types of clients, which corresponds to the knowledge that in most parts of the country the assignment of officers to clients is based on municipality of residence—which is correlated with important client (and officer) characteristics.

Second, in the Copenhagen sample, the assignment of officers to clients is far closer to random, as the distribution of almost all client covariates is not informed by the inclusion of officer fixed effects.[9] As the F-test statistic takes into account the sample size in the calculation of the test *p* value, the different sample sizes between the national sample and

[9] The stars in Table 3 indicate statistical significance at the 5 % level and below. But if several F-test statistics of random assignment in the Copenhagen sample are only marginally insignificant, this could point to nonrandom assignment, given the relatively low number of observations in this sample. But as only 6 of the 18 variables in Table 3 have *p* values between 0.05 and 0.15, we may safely conclude that the assignment of officers to clients is indeed (close to) random.

Table 2 Model fit statistics

Outcome	F-test of officer effects					Partial R-squared		
	Officers excluded		Officers included		F-test of officers	Partial R-sq.	Critical value	Reject null
	R sq.	F	R sq.	F				
Earnings	0.441	319.139***	0.456	27.184***	1.806***	0.042	0.020	Yes
Dependency on public transfers	0.493	628.643***	0.503	42.615***	1.309***	0.020	0.020	No
Criminal recidivism	0.208	307.629***	0.225	21.388***	2.186***	0.024	0.020	Yes
DF (k − 1; n –k − 1)	(23; 19,510)	(392; 19,141)	(369; 19,141)					

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Table 3 Investigation of officer assignment rule

Variable	Only Copenhagen	No Copenhagen
Supervision length	1.418	2.098***
On probation	1.279	3.558***
On parole	1.279	3.558***
Violence	1.855**	1.647***
Drug related crime	1.031	1.479***
Property crime	1.186	1.302***
Theft	1.135	1.446***
Other crime type	1.333	2.715***
Age	0.851	1.929***
Female	0.856	1.124
Unmarried	1.159	1.165*
Has children	1.162	1.390***
Ethnic minority	1.694**	2.664***
Years of education	0.968	1.727***
Prior earnings	1.266	2.085***
Prior public dependency	1.824**	1.528***
Number of previous crimes	1.256	2.824***
Previously imprisoned	1.030	1.371***
DF	35	326
N	2,012	19,534

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

the Copenhagen sample cannot explain the statistical insignificance in the model on the Copenhagen sample, and we conclude that the assignment of officers to clients is close to random in Copenhagen. This means that we may use the Copenhagen sample to obtain uncontaminated estimates of the causal effect of probation and parole officers on client outcomes.

Third, even though Copenhagen officials have repeatedly assured us that the assignment of officers to clients follows the rotational assignment process, Table 3 does reveal a few statistically significant F-values in the Copenhagen sample, which might challenge the random assignment assumption. Statistically significant at the 0.01 level, there is a correlation such that clients who are convicted of violence or who are of ethnic minority background are assigned to certain officers in Copenhagen, just as the same is true regarding the client's prior dependency on public benefit transfers.

Despite these minor signs that the assignment of officers to clients in Copenhagen might not be truly exogenous to all client characteristics, we still contribute to our understanding of the effects of probation and parole officers on their clients because no article has yet addressed the distribution of probation and parole officer fixed effects on their clients as we do—not even descriptively as we do in our national sample—and on top of this we have very close to random assignment of probation and parole officers to clients, which is an unprecedented contribution to the literature.

Copenhagen Sample Results

Table 4 examines the consequences for model fit diagnostics when we add officer fixed effects to our outcome model in the Copenhagen sample. The table reveals that in this sample, the inclusion of officer fixed effects still improves the model fit for two of three outcomes, which points to probation and parole officers being an important factor when considering client outcomes. And even though the sizes of the F-test statistics are not impressive, which suggests that officers matter to a lower degree than we would expect, the effect of probation and parole officers on their clients' dependency on public benefit transfers is significant at the 1 % level, and the effect on criminal recidivism is highly significant. The inclusion of officer fixed effects improves the model R squared by around 0.01 for both dependency on public transfers and recidivism—corresponding to an increase of 1.4–8 % in the model R squared—similar to the improvement of the full sample model. And likewise, the Partial R-squared exceeds the critical value (0.011) for all outcomes (0.013 for earnings; 0.014 for dependency on public transfers; and 0.023 for recidivism), which suggests that officer assignment could be used as a relevant instrument for these outcomes, which again supports the claim that officers matter (although, as discussed, not as much as one might expect). Only in terms of earnings does the inclusion of officer fixed effects not improve the model fit (according to the F-test).

Officer Fixed Effect Distribution

Even in the Copenhagen subsample, where officers are assigned to clients (close to) randomly, it matters for clients which probation or parole officer the client is assigned to, at least in terms of improving model fit. But statistically significant effects do not necessarily imply the effects are substantively important, especially considering the low F-test statistics and the weak intra class correlations. In this section, we therefore focus on the *magnitude* of the effect of officer assignment on clients rather than simply concluding that the officers matter, as explained in the fourth step of our analytic strategy. Specifically, we show the distribution of the officer fixed effects across outcomes for both the national sample and the Copenhagen sample (that has random assignment).

Figure 1 shows the distribution of the probation and parole officer fixed effects across outcomes, while comparing the national results to the Copenhagen results. As mentioned, the officer fixed effects in the sub-graphs of the figure are predicted on a fictional probationer who has 0 on all binary control variables and the mean of the Copenhagen subsample on continuous control variables. In all sub-graphs, the thicker and dashed horizontal line shows the predicted outcome for the fictional observation if we do not include the officer fixed effects (the thinner and dashed lines show the 95 % confidence interval of the prediction). The predictions on each of the outcomes when we do not include officer fixed effects are as follows: Earnings, DKK 45,480 (~USD 27,500); dependency rate, 0.192; and recidivism, 0.357.

The thicker and solid line in the graphs shows the predicted outcome for the fictional observation in the model that includes officer fixed effects (again, the thinner lines indicate the 95 % confidence interval). From these lines, it is evident that it matters for a probationer or parolee which officer he or she is assigned to since the outcome predictions across officers do not coincide with the horizontal line that does not include officers. For example, the same client's criminal recidivism rate lies between 0.2 and 0.5, caused only by which officer the client is assigned to in the Copenhagen subsample. Regarding dependency on public benefit transfers, the predictions range from 0.15 to 0.25, which is again an

Table 4 Model fit statistics for Copenhagen sample

Outcome	F-test of officer effects					Partial R-squared		
	Officers excluded		Officers included		F-test of officers	Partial R-sq.	Critical value	Reject null
	R sq.	F	R sq.	F				
Earnings	0.343	18.916***	0.351	8.778***	1.197	0.013	0.011	Yes
Dependency on public transfers	0.517	97.058***	0.524	43.260***	1.786**	0.014	0.011	Yes
Criminal recidivism	0.197	30.958***	0.213	15.473***	2.326***	0.023	0.011	Yes
DF (k − 1; n – k − 1)	(22; 1,989)	(57; 1,954)	(35; 1,954)					

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

important effect for the clients. In this light, the effect of probation and parole officers on their clients is substantial, and if policymakers expect a uniform effect of probation or parole on clients across officers, they are unlikely to see such an effect.

But is should also be noted that for all outcomes, a large proportion of the outcome predictions have 95 % confidence intervals that include the predictions not taking officer fixed effects into account, and the significant effect of officers on their clients might thus be driven mainly by officers at each end of the fixed effects distribution. This shows that it matters substantially for probationers and parolees whether they are assigned to some officers (those at both ends of the distribution) and not others (those at the middle), which might also help us understand why the causal effect of probation and parole officers on their clients is generally smaller than common sense and previous studies directs us to expect.[10]

It is worth remembering, however, that the inclusion of officer fixed effects in the Copenhagen sample outcome model for earnings was statistically insignificant, and that in principle the officer effect distribution lines in these sub-graphs could thus coincide with the horizontal line that represents predictions without the officer fixed effects. The curved line in the sub-graph for earnings in the Copenhagen sample also hints at this since it is close to horizontal for almost all the officers.

In the national sample, we see larger consequences of which officer a client is assigned to, yet here assignment is not random, so it is less clear whether these effects are driven by officer fixed effects or municipality. Regarding recidivism, results from the national sample are strikingly similar to those from Copenhagen in regards both to the curvature and to the 95 % confidence interval. Regarding dependency on public benefit transfers, the fictional probationer has a predicted dependency between 0.15 and 0.3, which is a wider range than in the Copenhagen sample, at least in the upper end of the range.

The overall story in Fig. 1 is thus simple: It makes a difference for a probationer or parolee which probation or parole officer he or she is assigned to, both in the Copenhagen sample that has (close to) random assignment and in the national sample that does not. And even though the variation in outcome predictions across officers is greater in the national sample than it is in the Copenhagen subsample, the probation and parole officer fixed effects are generally similar. But according to our statistical tests of the relevance of these officer fixed effects, the effects of probation and parole officers on their clients is less important than we might previously have thought, and Fig. 1 points towards only some officers (those at the ends of the fixed effects distributions) are driving the effects of officers on their clients.

Fixed Effect Consistency Across Officers

We have shown how much parole and probation officers matter for their clients' outcomes. But we are also interested in finding out whether these differences between officers are

[10] Naturally, the predictions might include officers that could be considered outliers. To investigate this for each of the outcomes we consider, we have re-run the analyses dropping the extreme probation and parole officers, defined as the top and bottom 5 % of officers (38 officers in the national sample and 4 officers in the Copenhagen sample). We found generally statistically insignificant F-tests of the effects in the Copenhagen sample when we drop these officers, but still the Partial R-squared for criminal recidivism was significant. We have also re-run the analyses dropping only the top and bottom officer, by outcomes. We found insignificant F-tests in the Copenhagen sample, but the Pseudo R-squared for all outcomes were statistically significant. Our main results are thus driven by officers who are either very good or very bad at securing the outcomes, a point we return to in the concluding section.

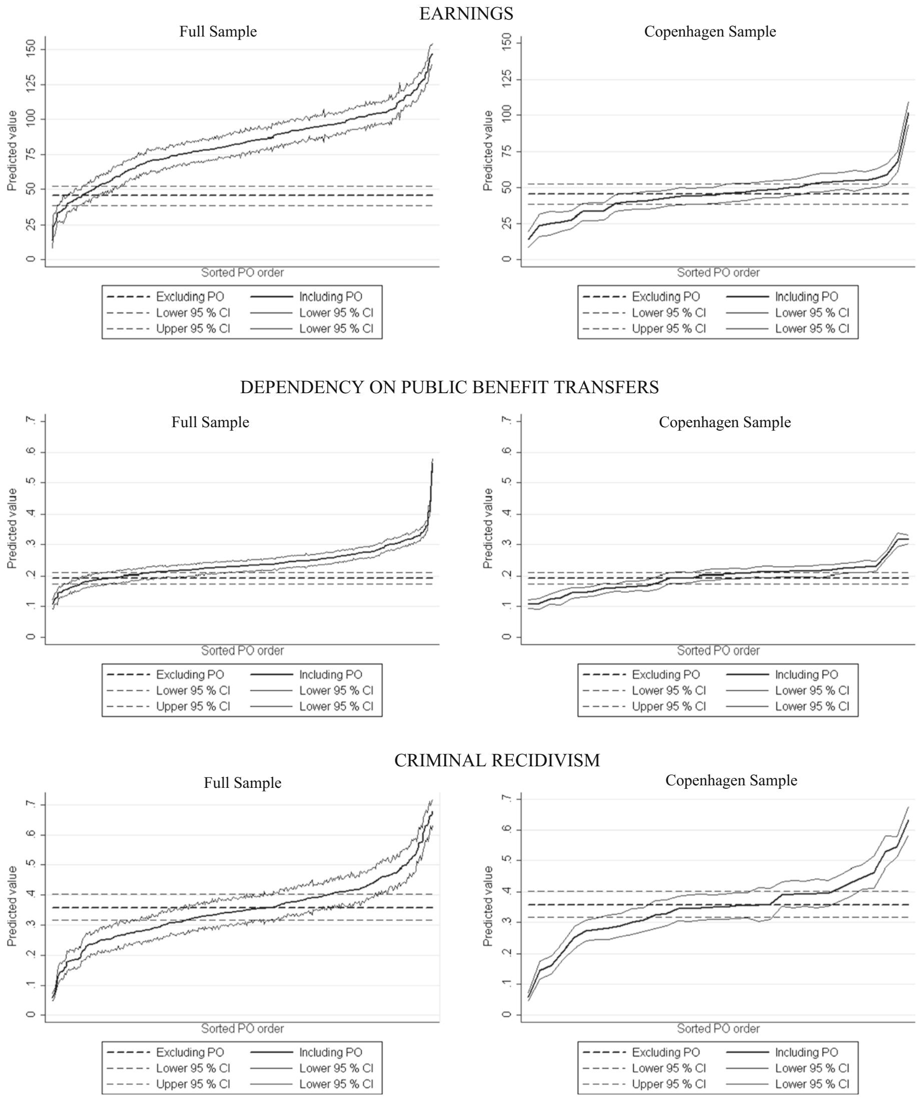

Fig. 1 Comparisons of predicted outcomes in full sample and Copenhagen subsample

consistent across outcomes, or whether each officer has his or her strengths and weaknesses. Put directly, are some officers simply “bad” at their job while others are “good”, or are some officers “very good” at some parts of their job—such as keeping their clients from recidivating—and “bad” at others—such as finding them a job?

The correlation between the predicted recidivism rate and the predicted dependency on public benefit transfers of the fictional observation by probation and parole officers is −0.03 in the national sample and −0.24 in the Copenhagen sample. None of these numbers are statistically significant, yet even if they had been, none of the correlations are strong by conventional standards. We do not correlate these predictions with the predictions regarding earnings, as the effect of officer assignment on earnings was insignificant.

Investigating the correlation between the outcome predictions by officer shows that one cannot analyze the effect of probation and parole officers on their clients on one outcome and then extrapolate to other outcomes. Rather, some officers excel at securing some outcomes, while other officers excel at securing other outcomes—and an officer's performance on one outcome thus has little to no bearing on his or her performance on other outcomes. We discuss the implications of this finding in the concluding section, but in order to fully understand why this is so, future research should dig deeper into the mechanisms of probation and parole officers' effects on their clients, which might be aided by more qualitative inquiries.

Conclusion

When imprisoned individuals are released on parole or offenders are sentenced to probation, it matters for their future which parole or probation officer they are assigned to. It matters less than what common sense and previous studies might lead us to expect, but the inclusion of officer fixed effects still improves our model for probationer and parolee labor market outcomes and recidivism. This is the main conclusion we draw from our analysis of the effect of probation and parole officers on their clients' outcomes.

While previous research has pointed out that probation and parole officers differ in the tasks they undertake and how they interpret their dual role of social worker and criminal justice employee—and has shown that it matters for probationers and parolees how their probation or parole officer weighs the control and support dimensions of this dual role—no previous study has identified the effects of probation and parole officers on their clients in a sample that has random assignment of officers to clients. The widespread presumption that probation and parole officers affect their clients has rather remained an untested assumption. But using a unique dataset that couples probation and parole officers with their clients in Denmark from 2002 to 2009, we have analyzed the effect of officer assignment on three outcomes in order to shed light on different aspects of probationers' and parolees' life circumstances as a consequence of the treatment they receive from their probation or parole officer. The three outcomes are earnings, dependency on public benefit transfers, and criminal recidivism—variables the literature identifies as central to the reintegration of ex-offenders.

Results show that while earnings remain unaffected by which officer a client is assigned to, the officer matters for both other outcomes. Depending on which officer the same client is assigned to, the client will have a rate of dependency on public benefit transfers somewhere between 0.15 and 0.25, and a criminal recidivism rate between 0.2 and 0.5. Both of these results are striking, as the probation or parole officer that the client is assigned to thus seems to have a substantial impact on the client's future. But the generally low proportion of the variance in these outcomes that is attributable to probation and parole officers rather than to their clients—and the generally weak test statistics of the importance of officers in our outcome models—suggests that we should interpret these numbers with some caution (and, perhaps unsurprisingly, that much of the variance in the outcomes of probationers and parolees is driven not by their probation or parole officers but by their own characteristics, many of which are unobserved). In fact, as we have shown, it appears as if the effects of officers on their clients might be driven mainly by those officers who are either very good or very bad at securing these outcomes, while for a large share of the officers, the assignment does not matter all that much for the probationer's or parolee's outcomes.

For this impact of officers on their clients to be causal, the assignment of officers to clients must be random. In Denmark as a whole, this is not the case, as officers are assigned to clients based on municipality of residence, and a host of characteristics are correlated with municipality. But the effects we have mentioned come from a sample that only contains cases from Copenhagen. Here, as we have shown, a rotational officer assignment process very close to randomizes clients to officers, and results are hence (very close to) causal in nature.

We have also analyzed the effect of officers on their clients in the national sample that does not have random assignment of officers to clients. Comparing these results to those from Copenhagen reveals the magnitude of selection mechanisms that rule out causal inference in the national sample. In general, the distribution of officer fixed effects in the national sample has heavier tails—implying that selection mechanisms pair officers and clients to make more "extreme" cases, which is also reflected in the larger intra class correlation between officers in the national sample compared to the Copenhagen sample—yet apart from this, results are surprisingly similar to those in Copenhagen. In fact, regarding criminal recidivism, the officer fixed effects in the national sample and the Copenhagen sample are very similar, although not identical.

Three caveats merit discussion. First, our data come from a small Scandinavian country with only some 5.6 million inhabitants, a universal welfare state, a sentencing system that differs from that in the US somewhat dramatically, and a different conception of parole and probation officers than in the US. This obviously makes it difficult to know how well our results generalize to the US context. However, since this article is the first to even show the effect of probation and parole officers on their clients in a sample with nonrandom assignment of officers to clients, and moves beyond this to present results from a subsample that has very close to random assignment of officers to clients, this lack of generalizability is perhaps not the most essential caveat. Second, the rotational officer assignment process in Copenhagen does not effectively randomize officers on all client characteristics. Three out of 17 client characteristics suggest nonrandom assignment, and even though none of these are highly significant, this problematizes our claim of causal inference. Third, to avoid serial correlation between our observations, we analyze only a client's first supervision term within the data period, just as we exclude clients who change from one probation or parole officer to another during their probation or parole, as it is unclear which probation or parole officer these clients should be attributed to. This obviously limits our conclusions accordingly. Yet even if our conclusions are only valid for a client's first probation or parole in 2002–2009—and only for clients who do not change probation or parole officer during supervision—these conclusions still offer important knowledge into the effects of probation and parole officers on these clients.

As a last step in our analysis, we have shown that probation and parole officers are not consistently good or bad across outcomes. We showed this by calculating the correlation between the predicted recidivism rate and the predicted dependency on public benefit transfers across officers of the same fictional observation. We found this correlation to be weak and statistically insignificant, which means that an officer's effect on his or her client's dependency on public benefit transfers was not informative on the same officer's effect on the same client's recidivism rate. Our results and robustness checks further indicate that the effects of probation and parole officers on their clients might be driven mainly by officers who are extremely good (or bad) in one domain. As mentioned earlier, the intra class correlation coefficients showed that the amount of variation in each of the outcomes that is attributable to the officers rather than to the individual clients themselves is negligible, around 1 % in the Copenhagen sample, and this finding therefore comes as little surprise. But it does, however, suggest that in order to understand the effect of probation and parole officers on their clients, we need to dig deeper into the mechanisms that generate the effect when officers and clients meet—especially when extremely good (or bad) officers meet with their clients. Previous research attributes officer attitudes, workloads, and training to the effect of probation and parole officers on their clients. Our findings fall well in line with these previous explanations, as it is easy to image that the insignificant correlation in officer effects across outcomes in our sample could be caused by some officers emphasizing their role as criminal justice employee whereas other officers emphasize their role of social worker, and that they therefore secure different outcomes for their clients. Unfortunately, our data does not allow us to analyze these mechanisms, but we nonetheless close by suggesting that now that we know that parole and probation officers do matter—although less than we might previously have thought—we must start to understand how they matter for their clients.

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