2024 Research Report

Determinants of Recidivism: How Criminal Justice Interactions and the Post-Release Environment Affect Repeat Offending

Randi Hjalmarsson

SNS

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SNS – the Center for Business and Policy Studies, is an independent, non-profit organization founded in 1948 that aims to be Sweden's leading platform for objective debate and knowledge-sharing among decision-makers on key societal issues. SNS brings together representatives from the business community, public sector, academia, and politics. SNS takes no positions on policy issues, which supports its bridge-building role. Members include companies, public authorities, and organizations.

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Print: Rapid-Tryck AB, Norsborg 2024
ISBN 978-91-89754-54-6

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Foreword

This report by Randi Hjalmarsson, professor of economics at the University of Gothenburg, is timely. She discusses a number of factors that may affect repeat offending by criminals, also known as recidivism, and emphasizes what research has to say about the causes of reoffending.

The focus is on the probability of getting caught, diversion from the criminal justice system and prison, time in prison and prison conditions, and the post-release environment, including the labor market.

The author reviews a large body of research, conducted in Sweden and internationally, that uses quasi-experimental research designs to disentangle correlation from causation. She then discusses the extent to which these findings are applicable to the Swedish context and offenders.

SNS hopes that this study may contribute to the current Swedish discussion on ways to reduce and control crime.

The author is solely responsible for the analysis, conclusion, and policy recommendations presented in the report. SNS as an organization does not take a position on these. The mission of SNS is to initiate and present research-based analyses of issues of importance for society.

Just like Randi Hjalmarson's earlier SNS report, *Social Policies as Crime Control*, this report is part of the SNS project *Crime and Society*, a project made possible through funding from a reference group that also follows the research project.

The reference group consists of Akavia, Avarn Security, City of Gothenburg, City of Malmö, City of Stockholm, Confederation of Swedish Enterprise, Fryshuset, Insurance Sweden, Mellby Gård, Ministry of Finance, MKB Fastighets AB, Swedish Bar Association,

Swedish Enforcement Authority, Swedish National Courts Administration, Swedish Police Authority, Swedish Police Union, Swedish Prison and Probation Service, Swedish Property Federation, Swedish Prosecution Authority, Swedish Public Employment Service, Swedish Social Insurance Agency, Swedish Supermarket Owners' Association, and the Swedish Tax Agency.

Martin Hällsten, professor of sociology at Stockholm University, is the SNS Scientific Council's representative in the reference group.

The author has received valuable input and comments on an earlier draft of the report from the members of the reference group.

At an academic seminar, Anders Nilsson, professor of criminology at Stockholm University, provided constructive comments on the report.

SNS and the author are grateful to the reference group members, Anders Nilsson, and the other participants in the academic seminar.

Stockholm in November 2024

Stefan Sandström
Research Director, SNS

Swedish Summary/ Svensk sammanfattning

Ett samhälle som strävar efter att minska brottslighet måste förhindra personer från att begå sitt första brott, men även förhindra att dömda brottslingar återfaller i brott. Den här rapporten undersöker hur fyra faktorer påverkar återfall: (i) sannolikheten att åka fast, (ii) alternativ till fängelse, (iii) fängelse och (iv) miljön efter frigivning. Rapporten framhåller forskning från i första hand nationalekonomer, som använder så kallade kvasiexperimentella metoder för att få fram orsakssamband mellan de fyra nämnda faktorerna och återfall i brott. Rapporten bidrar därmed till att nationalekonomer, som traditionellt sett inte brukar uppfattas som experter på kriminalitet, kommer till tals i den offentliga debatten. Denna diskussion tar idag stor plats i Sverige, inte minst på grund av tilltagande oro för brottslighet och en förmodad kraftig ökning av antalet intagna på fängelser. Många av de teman som diskuteras i rapporten – i synnerhet effekter av elektronisk övervakning, ungdomsfängelser, längre fängelsestraff och förhållanden på fängelser – är i högsta grad relevanta för dagens situation i Sverige.

Den nationalekonomiska modellen för att analysera brottslighet, som den formulerades av Gary Becker 1968, erbjuder ett enkelt ramverk för att förstå hur en individ formar sitt beslut att begå brott. Enligt modellen kan återfallen minskas genom faktorer som ökar de förväntade kostnaderna för att begå brott och som förbättrar möjligheterna att få en inkomst genom vanligt arbete. Modellen förutsäger tydligt att återfallen minskar om risken för att åka fast ökar (exempelvis genom att individen finns i DNA-databaser) och om möjligheterna att få arbete förbättras. Däremot är det inte lika tydligt om strängare fängelsestraff – exempelvis fler eller längre straff – minskar återfall. Å

ena sidan kan fängelsestraff minska återfallen genom sin avskräckande effekt eller genom olika behandlingsprogram som påverkar individens möjligheter efter avtjänat straff eller dennes beslutsförmågor. Å andra sidan kan återfallen öka genom stigmatisering på arbetsmarknaden, förlorat socialt kapital och förstärkt brottskapital.

Svenska politiker och myndigheter strävar efter ett evidensbaserat förhållningssätt till straffrätten. Ett exempel på denna ambition är den nyligen framtagna utredningen om åtgärder för att bekämpa återfall (SOU 2024:54). I den här rapporten argumenterar jag för att en mycket viktig del i ett sådant tillvägagångssätt är att det finns evidens för att en viss politik har den avsedda *kausala* effekten på den grupp som avses – i detta fall brottslingar. Till exempel, leder hårdare straff till färre återfall? Den frågan är svår att besvara, och för att illustrera varför kan vi titta på mönster som observeras i grunddata. Ofta finns ett positivt samband mellan hårdare straff och återfall: individer som får hårdare straff är mer benägna att återfalla i brott. Betyder det att hårdare straff ökar brottsligheten? Nej. Det återspeglar helt enkelt utmaningarna forskare står inför när det gäller att skilja på korrelation och kausalitet. Att det uppstår en positiv korrelation beror på det straffrättsliga systemets utformning. Till exempel döms de som begår brott inte slumpmässigt till fängelse, utan det är snarare allvaret i själva brottet och brottshistoriken som bestämmer straffet. Det är inte bara svårt att besvara frågan om huruvida hårdare straff ökar brottsligheten, det är även av yttersta vikt. Åtgärder som enbart bygger på korrelationer kan vara ineffektiva för att minska brottsligheten. De kan även medföra höga kostnader för samhället.

Den bästa metoden för att skilja på kausalitet och korrelation inom många områden är att använda så kallade randomiserade kontrollerade experiment. Inom det straffrättsliga området är det emellertid sällan etiskt godtagbart att genomföra sådana studier. Den mest övertygande kausala evidensen inom forskning om brottslighet finner man snarare med *kvasiexperimentella upplägg*, det vill säga att i frånvaro av sant randomiserade experiment utnyttjar forskaren »som om« det är slumpmässig variation som avgör vem som får ett visst straff jämfört med ett annat. Eftersom tillgången till detaljerade brottsregister är stor, har denna typ av kausal evidens ökat kraftigt under de senaste åren, särskilt i de nordiska länderna.

Sannolikheten att åka fast

EVIDENS

Polisinsatser (antalet poliser, deras närvaro och uppgifter) ses vanligtvis som den huvudsakliga metoden för att öka sannolikheten att åka fast. Det finns en omfattande forskning inom detta område som tillhandahåller evidens om polisens allmänt avskräckande effekt. Men polisnärvaro ökar inte nödvändigtvis sannolikheten att åka fast annorlunda för förstagångsförbrytare än för återfallsförbrytare. Det finns emellertid teknik som särskilt kan påverka sannolikheten att en redan känd gärningsperson åker fast. Två viktiga exempel är DNA-databaser, vilka innehåller DNA-profiler för vissa dömda brottslingar som kan användas för att matcha framtida DNA-bevis från brottsplatser, och (vanligtvis offentliga) sexualbrottsregister.

- Forskning från USA och Danmark har visat att registrering i DNA-databaser leder till färre återfall. Dessa resultat gäller dock bara för brottslingar som gjort sig skyldiga till tillräckligt allvarliga brott för att deras DNA ska registreras.
- Det finns ingen övertygande evidens för att förekomsten av register över sexualförbrytare minskar återfall i USA.

DEN SVENSKA KONTEXTEN

DNA-register används även i Sverige. De flesta brottslingar som döms till fängelsestraff hamnar i registret. Men såvitt jag vet har det inte genomförts några tillförlitliga utvärderingar av hur DNA-registrering påverkar återfall i brott. Även om det knappast finns anledning att anta att resultaten från USA och Danmark inte skulle gälla även för Sverige, skulle det vara bra om en svensk analys kunde genomföras och då dessutom för olika brottstyper.

Alternativ till fängelse

EVIDENS

Ett ökande antal intagna på fängelser över hela världen har lett till att vissa straffrättsliga system står inför problem med överbeläggning och kapacitetsbegränsningar samt oro över de potentiellt skadliga effekterna av att sitta i fängelse. Alternativa åtgärder för att hålla brottslingar som begått relativt små brott borta från fängelse kan vara att åklagare

avstår från att väcka åtal och att domare skjuter upp en fällande dom till framtiden, förutsatt att individen kan uppvisa ett gott beteende. En annan metod som alltmer används för att minska antalet intagna på fängelser är elektronisk övervakning. Vissa dömda brottslingar utrustas med teknik som via GPS övervakar var de befinner sig. De är därmed intagna i hemmet snarare än i ett fängelse.

› När det gäller att minska återfall hos individer som begått mindre allvarliga brott finns det övertygande evidens för att andra påföljder än fängelse fungerar i USA.
› Elektronisk övervakning har utvärderats i flera länder – Sverige, Norge, Argentina, Frankrike och Storbritannien – och i olika sammanhang. Forskningen visar enhälligt att elektronisk övervakning inte ökar återfall, utan istället ofta minskar återfall.

DEN SVENSKA KONTEXTEN

När det gäller alternativ som att avstå från att väcka åtal eller skjuta upp domar baseras evidensen enbart på situationen i USA, där brottslingar hålls utanför ett straffrättsligt system som har mycket hårdare straff än det svenska. Det är därför inte säkert att vi skulle se samma effekter i Sverige. I den mån som dessa alternativ används i Sverige krävs tillförlitliga utvärderingar för att kunna avgöra effekterna på återfall. Vad gäller elektronisk övervakning kan dessa resultat troligtvis generaliseras till den svenska kontexten, och faktum är att ett antal av de studier som diskuteras har genomförts i Sverige. Man bör ändock vara försiktig när det kommer till slutsatser. De svenska utvärderingarna har hittills inte gett någon information om exempelvis de potentiella effekterna av att Kriminalvården fått större möjlighet att använda sig av elektronisk övervakning eller att utvidga elektronisk övervakning till brottslingar med längre straff.

Effekter av fängelse

EVIDENS

Allt mer forskning ägnas åt att undersöka hur brottslingars beteende efter frigivningen påverkas av själva fängelsevistelsen och längden på tiden i fängelse. Resultaten är lite blandade.

› Evidensen är blandad om effekterna av ungdomsfängelse på återfall i brott i ett antal amerikanska delstater.

- Detsamma gäller evidensen om vuxna. Studier från USA finner i allmänhet inga signifikanta effekter (positiva eller negativa) av att bli inlåst i fängelse när det gäller återfall. Däremot finns det övertygande evidens i Norden, och i synnerhet i Norge, att inlåsning kan minska återfall, särskilt för dem som var arbetslösa innan de hamnade i fängelse.
- Längre tid i fängelse har visat sig leda till färre återfall, åtminstone för vissa grupper av kriminella, i de amerikanska delstaterna North Carolina och Georgia samt i Sverige. Även om man inte ser någon ökning i återfall för någon grupp av kriminella i Sverige, minskar återfallen bara för dem som suttit flera gånger i fängelse.

Med andra ord fungerar vissa fängelsevistelser för vissa brottslingar. Det är oklart varför det är så. En möjlig förklaring kan vara heterogenitet, eftersom olika studier uppvisar olikheter när det gäller brottslingarnas egenskaper, såsom typ av brott och kriminella erfarenheter. Samma fängelseupplevelse kan ha olika effekter på olika typer av individer. En annan potentiellt viktig förklaring är emellertid heterogenitet vad gäller själva fängelseupplevelsen. Fängelse kan sägas utgöra en svart låda som innehåller många olika faktorer som kan påverka den intagna: själva anläggningen som sådan, andra fångar, domens längd, olika program, missbruksbehandling samt hälso- och sjukvård. Det finns en ny, men fortfarande ganska begränsad, forskningslitteratur som utvärderar dessa olika upplevelser av fängelse.

- Bättre fängelseförhållanden minskar återfall i brott.
- Kognitiv beteendeterapi, särskilt för förstagångsförbrytare och i nära anslutning till frigivning, har visat sig minska återfall.
- Merparten av forskningen finner ingen effekt på återfall av att få besök i fängelse.
- Nyligen genomförd forskning i Sverige finner ingen effekt på återfall av nya ADHD-diagnoser i fängelse.
- Andra fångars egenskaper spelar roll, men denna effekt varierar mellan individer. Att exponeras för individer med en liknande kriminell historia som en själv tenderar att förstärka dessa kriminella beteenden.
- Vissa fängelseprogram minskar återfall. Positiva effekter har uppvisats för program som fokuserar på våld, utbildning och sysselsättning.

DEN SVENSKA KONTEXTEN

Sverige och Norge satsar mer pengar per fånge än något annat land, och ungefär tre gånger mer än i USA. Det svenska samhället i övrigt är också mycket mer likt Norge än USA. När forskningsresultaten generaliseras till svenska förhållanden bör därför större vikt läggas vid de norska studierna än de amerikanska. Försiktighet bör ändå iakttas och svenska utvärderingar bör genomföras utifrån potentiella skillnader mellan de svenska och norska fängelsepopulationerna. Både de svenska och norska systemen lägger stor vikt vid behandling och på att ha fängelser av hög standard. Även om dessa sannolikt kommer att ha en positiv effekt när det gäller återfall, betyder det inte nödvändigtvis att svenska fängelser fördelar sina resurser på ett optimalt sätt. Ett potentiellt exempel på detta är att betoningen på ADHD-diagnoser i svenska fängelser inte verkar minska återfallsbeteenden. Det krävs mer forskning för att tydligt identifiera »vad som fungerar« i svenska fängelser och för vem.

Miljön efter frigivning

EVIDENS

Det kan vara så att inte ens de allra bästa fängelserna lyckas minska återfall i brott om tidigare brottslingar inte kan få ett arbete eller ett arbete som betalar en tillräckligt hög lön. Detta är ett påtagligt problem eftersom forskning visar att företag ogärna anställer personer med ett brottsligt förflutet. En växande forskningslitteratur studerar relationen mellan återfall och två faktorer som potentiellt påverkar brottslingars ekonomiska situation efter frigivning: arbetsmarknadsförhållanden och offentligt stöd.

- Återfallen minskar om lönerna för lågkvalificerade jobb höjs.
- Återfallen minskar om arbetstillfällena ökar i lokal bygg- och tillverkningsindustri.
- Återfallen minskar om mediebevakning av jobbskapande åtgärder ökar, vilket tyder på att information om möjligheter till arbete kan spela en viktig roll.
- Återfallen tenderar att minska om det finns tillgång till välfärdsförmåner och in natura-stöd som matkuponger.

DEN SVENSKA KONTEXTEN

Den stora merparten av dessa resultat baseras på hur det ser ut i USA, där brottsregister till mycket stor del är offentliga, inte återkommande rensas och där bakgrundskontroller är vanliga. Huruvida dessa resultat även gäller för Sverige är oklart och något som kräver ytterligare forskning. Detta gäller i synnerhet i den nuvarande svenska miljön där en kriminell bakgrund blir alltmer synlig som ett resultat av databaser på nätet och ett växande antal bakgrundskontroller under jobbsökningsprocesser, både på grund av obligatoriska regler och arbetsgivares preferenser. När det gäller välfärdsförmåner förlorar, såvitt jag vet, inte dömda brottslingar i Sverige sin tillgång till sådana stöd, vilket är vanligt förekommande i USA. Därmed blir den relevanta (men obesvarade) frågan huruvida dessa individer tar del av allt stöd de har rätt till.

Executive Summary

Repeat offending or recidivism is prevalent around the world. Therefore, societies aiming to reduce crime must not only prevent individuals from committing their first crime but also prevent known offenders from returning to crime. This report reviews the evidence related to how four factors impact recidivism: (i) the probability of getting caught, (ii) diversion from the criminal justice system and prison, (iii) prison, and (iv) post-prison environments. Research (primarily by economists) using quasi-experimental designs to provide *causal evidence* of the determinants of recidivism is highlighted. This report thus serves to bring the voices of economists, who are not traditionally perceived as criminal justice experts, to the public debate. This debate is particularly active in Sweden today, given increasing concerns about crime and forecasted growth of Swedish prison populations. Many of the topics discussed in this report—and especially the effects of electronic monitoring, juvenile incarceration, longer sentences, and prison conditions—are highly relevant to the current Swedish context.

The economic model of crime, as formulated by Gary Becker in 1968, is a simple framework to think about an individual's decision to commit crime. Within this framework, recidivism can be decreased by both factors that increase the expected costs of crime and factors that improve legitimate income opportunities. This framework clearly predicts a reduction in recidivism due to an increase in the chance of getting caught, e.g., via the introduction of DNA databases, and an increase in labor market opportunities. But, whether harsher sanctions—e.g., more or longer prison sentences—reduce recidivism is less clear cut. On the one hand, prison can decrease recidivism via

deterrence or rehabilitation programs that impact post-release opportunities or decision-making abilities. On the other, recidivism can increase due to labor market stigmas, lost social capital and accumulated criminal capital.

Swedish policy makers and authorities aim to use an evidence-based approach to criminal justice policy. One example of this ambition is the recently completed government investigation on measures that can combat recidivism (SOU 2024:54). This report argues that a first-order ingredient to such an approach is evidence that a policy has the intended *causal* effect on the targeted population—in this case, offenders. For instance, do harsher punishments *cause* a reduction in recidivism? This is a hard question to answer. To illustrate why, let us consider the patterns one may observe in the raw data. There will very often be a positive relationship between sanction severity and recidivism: individuals who receive harsher punishments are more likely to reoffend. Does this mean that harsher punishments increase crime? No. This simply reflects the challenges faced by researchers in disentangling correlation from causation. The positive correlation often arises from the nature of the criminal justice system. In particular, we do not randomly sentence offenders to prison, but rather systematically assign sanctions based on the severity of their offense and criminal history. Answering this question is not only difficult, but also of fundamental importance. Policies based on correlational evidence only may be ineffective at reducing crime and costly to society.

The gold-standard used in many fields to disentangle causation from correlation is randomized control trials. Such an approach is rarely ethical in the context of criminal justice. Rather, the most convincing causal evidence in the crime research space relies on *quasi-experimental research designs*: in the absence of true randomized experiments, researchers utilize "as if" random variation in the real world that affects who gets one punishment versus another. Such causal evidence has sharply grown in recent years, especially in Scandinavia, due to access to detailed criminal justice registers.

The Probability of Getting Caught

THE EVIDENCE

Police (force size, visibility, and tasks) are typically thought of as the main channel through which one can increase the probability of getting caught. A large corresponding literature provides empirical evidence of the general deterrence effects of police. But police do not necessarily increase the probability of getting caught differentially for first time versus repeat offenders. There are, however, technologies that may specifically impact the probability that a known offender gets caught. Two prominent examples are DNA databases, which keep the DNA profiles of selected convicted offenders on record to match to future DNA crime scene evidence, and (typically public) sex offender registries.

- There is evidence from research in the US and Denmark that registration in DNA databases decreases recidivism. This evidence is restricted to offenders charged with serious enough offenses to be eligible for DNA registration.
- There is not convincing evidence that sex offender registries reduce recidivism in the US.

THE SWEDISH CONTEXT

As in the US and Denmark, DNA registries are also used in Sweden—today, most convicted offenders with a prison sentence are eligible to have their DNA entered into the registry. Yet, to the best of my knowledge, no evaluations of the causal effects of DNA registration in Sweden on recidivism have been conducted. Though there is little reason to expect the findings in the US and Danish contexts do not replicate in Sweden, an analysis in the Swedish context—especially across different offense groups—would be important to see.

Diversion from the Criminal Justice System and Prison

THE EVIDENCE

Growing prison populations worldwide have led criminal justice systems to confront the issues of prison over-crowding and capacity constraints as well as concerns about the potentially harmful effects of

prison. One tool used to combat these issues is "diversion," which is used to keep relatively minor offenders away from the legal system and prison. Prosecutors can decide not to press charges or judges can defer a conviction to the future conditional on good behavior. Another tool increasingly used to decrease prison populations is electronic monitoring (EM): selected convicted offenders are equipped with a technology that monitors their location via GPS and detained at home rather than prison.

- There is convincing evidence that diversion in the US works to decrease recidivism for minor offenders.
- Electronic monitoring has been evaluated in multiple countries—Sweden, Norway, Argentina, France, and the UK—and contexts. The research uniformly finds that electronic monitoring does not increase recidivism, and often decreases recidivism.

THE SWEDISH CONTEXT

With regards to diversion, the evidence is solely based on the US context, in which offenders are being diverted away from a criminal justice system that is much more punitive than that in Sweden. It is not clear that the same effects would be found in Sweden; to the extent that diversion is used in Sweden, evaluations of its recidivism effects are needed. With regards to electronic monitoring, these results are likely to generalize to the Swedish context, and indeed, a handful of the discussed papers are conducted in Sweden. But caution should still be exercised. The Swedish evaluations thus far do not inform us of the potential effects, for instance, of more discretion being given to Kriminalvården in its use of EM (permitting Kriminalvården to consider EM as an option for convicted offenders who do not apply themselves) or of expanding EM to offenders with longer sentences.

The Effects of Prison

THE EVIDENCE

A growing body of research evaluates the overall effect of any time in prison and more time in prison on criminal behavior post-release. The findings are somewhat mixed.

- There is mixed evidence on the impact of juvenile incarceration in different US states on recidivism.

- The evidence on adults is also mixed. US-based studies generally find no significant effects (positive or negative) of incarceration on recidivism. But, there is convincing evidence in Scandinavia, and especially Norway, that incarceration can reduce recidivism, especially for those who were unemployed prior to prison.
- More time in prison has been seen to reduce recidivism, at least for some offender groups, in the US states of North Carolina and Georgia as well as in Sweden. In Sweden, though an increase in recidivism is not seen for any offender group, the reduction is only seen for those not in prison for the first time.

Thus, some prisons work for some offenders. Why is up for debate. Heterogeneity is one potential explanation. There is heterogeneity across studies in offender characteristics, such as offense type or criminal experience. The same prison experience may have different impacts on different types of individuals. But, another potentially important channel is heterogeneity in the prison experience. Prison is a black box of many treatments: facility, peers, sentence length, programming, substance abuse treatment, and healthcare. A newer and still relatively small literature evaluates these dimensions of the prison experience.

- Better prison conditions reduce recidivism.
- Cognitive behavioral therapy, especially for first time offenders and near the time of release, has been shown to decrease recidivism.
- Most research finds no impact of visitation in prison on recidivism.
- Recent research in Sweden finds no impact of new diagnoses for ADHD in prison on recidivism.
- The characteristics of one's peers in prison matter, but the effect varies across individuals. Exposure to peers with similar criminal histories as oneself tends to reinforce those criminal behaviors.
- Some prison programming decreases recidivism. Positive effects have been seen for programs targeting violence, education, and employment.

THE SWEDISH CONTEXT

Swedish and Norwegian prison systems spend more money per inmate than anywhere else in the world, and about three times more than in the US. Swedish society outside prison is also clearly more comparable

to Norway than the US. Thus, in terms of generalizing the effects of the above research to Sweden, more emphasis should be placed on the Norwegian studies than those in the US. At the same time, caution should still be exercised and Swedish evaluations conducted given the potential differences between Swedish and Norwegian prison populations. Both the Swedish and Norwegian systems place a substantial emphasis on rehabilitation and providing high-quality prison conditions. While these are likely to be generally beneficial in terms of recidivism, this does not necessarily mean that Swedish prisons are optimally allocating their resources. One potential example of this highlighted in the report is that an emphasis on ADHD diagnoses in Swedish prisons does not appear to decrease recidivism behavior. More research is needed to explicitly identify "what works" in Swedish prisons, and for whom.

The Post-Release Environment

THE EVIDENCE

Even the very best prisons may not succeed in reducing recidivism if ex-offenders are unable to get a job or one that pays sufficiently high wages. Given evidence that firms are unwilling to hire workers with criminal records, this is a real concern. A growing body of research considers the relationship between recidivism and two factors that affect how liquidity constrained offenders may be post release: labor market conditions and public assistance.

- Recidivism decreases in response to higher wages for low-skilled jobs.
- Recidivism decreases in response to local construction and manufacturing job opportunities.
- Recidivism decreases in response to media coverage of job creation, suggesting that information about job opportunities may be important.
- Evidence suggests that access to public benefits—cash and in-kind benefits like food stamps—decreases recidivism.

THE SWEDISH CONTEXT

The vast majority of these findings are based on the US context, where criminal records are very visible and not regularly cleared and background checks are common. Whether the findings generalize to Sweden is an open question and one on which more research is needed. This is especially true in the current Swedish climate, where criminal backgrounds are becoming more visible with the advent of online databases and growing numbers of background checks requested during the job search process due to both mandatory regulations and firm preferences. With regards to public benefits, to the best of my knowledge, convicted offenders in Sweden do not lose access to their benefits as they do in the United States. Thus, the relevant (but unanswered) question is whether these individuals take up all of the benefits to which they are entitled.

1. Introduction

Many crimes worldwide are not committed by first-time offenders. In other words, worldwide recidivism or reoffending rates are high. A commonly cited statistic is that for the United States: about two-thirds of state prisoners are re-arrested within three years of release from prison (Alper, Durose, and Markman, 2018).[1]

What are the recidivism rates in Sweden? 31 percent of those who completed a prison sentence or began a probation sentence in 2019 received a new sentence supervised by Kriminalvården (The Swedish Prison and Probation Authority) by 2022. These rates are markedly lower today than they were in the 1990s (when they were around 40 percent). Recidivism statistics also vary by type of sentence: rates today are still around 40 percent for those released from prison but are less than 10 percent for those released from a probation sentence with community service. Finally, recidivism rates tend to be much higher for younger offenders and vary substantially across offense categories.[2] The Swedish National Council for Crime Prevention (Brå) reports that in Sweden, 40 percent of offenders released or treated in 2015 recidivated within three years, where recidivism is measured as a relapse into crime as opposed to returning to prison.[3]

Comparing recidivism statistics across countries, and even within

1. Similarly high rates of returning to crime for serious offenders are seen in many (though not all) other countries (Yukhnenko, Sridhar, and Fazel, 2020).
2. See Kriminalvården's website for recent recidivism statistics: https://www.kriminalvarden.se/forskning-och-statistik/statistik-och-fakta/aterfall/.
3. See https://www.bra.se/bra-in-english/home/crime-and-statistics/crime-statistics.html.

the same country over time, is thus especially hard for a number of reasons. First, as will be seen throughout this report and as indicated above, there are many ways to measure recidivism. Definitions vary both in the nature of repeat offending (e.g., new arrest, new conviction, or new prison sentence) and the time horizon (e.g., within 1, 3, 5, or 10 years). Moreover, aggregate statistics mask significant heterogeneity in recidivism rates for different sub-populations. For instance, recidivism rates are markedly higher for men than for women and offenders who have earlier convictions. Repeat offending rates also vary substantially across offense types. These patterns imply that as the characteristics of offending populations vary across countries or over time, so will recidivism rates.

Regardless of how they are measured, high recidivism rates do imply that a large share of all crimes are committed by a small group of offenders. Societies aiming to reduce crime must therefore also reduce repeat offending or recidivism.

Economic Framework and the Determinants of Recidivism

Gary Becker's (1968) economic model of crime provides a simple economic framework to view an individual's decision to commit a crime. This framework is relevant both when thinking about factors that can impact an individual's decision to commit their first crime and a decision to *return to crime* or recidivate.[4] In this model, rational individuals decide whether to commit a crime by comparing the expected costs and benefits from criminal and legal activities. Basically, individuals will commit a crime if the expected utility from doing so is greater than the expected costs. The expected costs of crime are a function of the probability of being caught as well as the severity and probability of punishment: as the probabilities of arrest and/or punishment increase and as the severity of punishment increases, the expected benefits of crime decrease. In the context of whether an individual decides to recidivate, any criminal justice policy or interaction with the justice

4. Doleac's (2023) survey on the determinants of criminal desistance includes a simple model that depicts how a wide range of factors, including those discussed in this report, may impact recidivism via the economic model of crime.

system that impacts the offender's (expected) chance of getting caught (e.g., DNA databases make it easier to catch ex-offenders) or the probability and/or severity of punishment would be forecast by Becker's model to reduce crime via deterrence. We would often call this *specific deterrence,* since it is the impact of the treatment (e.g., sanction) on the individual specifically sanctioned and not the general deterrence impact on the rest of the population. This report will discuss the recidivism effects of a wide range of justice system interactions, ranging from diversion from the justice system to electronic monitoring and prison. Significant attention will especially be given to the black box of prison, as there are many aspects of a prison sentence that make it more or less severe, including the length of the sentence and prison conditions.

Also in the context of the Becker model, an improvement in an individual's legitimate labor market opportunities (i.e., wages and employment) should decrease their propensity to commit (or re-commit) crime. Thus, labor market conditions and policies as well as welfare and public benefits can impact recidivism behavior. This channel may be particularly relevant for individuals recently released from prison; even the very best prisons may be unable to prevent offenders from returning to crime if they, for instance, cannot get a job. To the extent that time in prison leads to a deterioration of social and human capital, employment opportunities upon release may be especially weak (Western, Kling, and Weiman, 2001). Moreover, a significant stigma is associated with having a criminal record, let alone a prison record; there is corresponding empirical evidence that having a criminal record decreases one's labor market opportunities, at least in part because firms are less willing to hire workers with criminal records. This report thus also discusses the role that the post prison environment—especially labor market conditions—plays in explaining recidivism behavior.

Sanctions can also impact recidivism behavior through channels other than specific deterrence. A fundamental component of the Swedish criminal justice system today is the role of rehabilitation. Prisons include a wide range of treatment programs, such as substance abuse treatment or cognitive behavioral therapy, and healthcare more generally. Prisons can include education and training programs. Such rehabilitation programs in prison could also reduce recidivism, still in the Becker framework, in part by making the opportunity costs of returning to crime (i.e., potential legitimate opportunities) higher.

Therapy programs could also impact an offender's subjective perceptions of the cost of crime, by teaching them to be less impulsive and to put more weight on the future consequences and costs.

At the same time, there is significant societal concern about the criminalizing effects of the penal system. Sanctions may not necessarily deter or rehabilitate, but could actually serve to harden offenders. This could occur via a breakdown of social capital and stigma upon release, but also via an accumulation of criminal capital (knowledge and networks) in prison. Exposure to criminal peers is almost certainly greater in prison (where all peers are offenders) than on the streets (where some peers are offenders).

There are two important take-aways from this discussion. First, there are a wide range of factors that can impact recidivism—ranging from new technologies that affect the probability of catching repeat offenders to the ladder of criminal justice sanctions to prison conditions to the post-prison environment. Second, due to the potential off-setting channels, theory does not clearly predict the impact of many of these factors on recidivism. For instance, sanctions like prison can decrease recidivism via specific deterrence and rehabilitation programs but can increase recidivism due to post-release labor market stigmas, disrupted social capital and accumulated criminal capital. Empirical research is needed to evaluate which of the theoretical channels dominate.

An Ambition for Evidence-Based Criminal Justice Policy

Swedish authorities have a goal of using an evidence-based approach to criminal justice policy. This ambition has been most recently demonstrated by the recent government investigation into factors that determine recidivism (SOU 2024:54). But this is not just a recent ambition. For instance, Tallving (2018, p. 133) writes that the Swedish prison authorities strive to "*…establish, using scientific methods, whether an intervention, a directed effort to an individual or group, has the intended effect.*" My reading of this quote and definition of an evidence-based approach is that policy makers should ask themselves three questions:

i. Does the policy have the intended effect on the targeted population?
ii. Are there unintended effects on the targeted population?

iii. Are there unintended effects on non-targeted populations?

The first question is clearly of first-order importance, and is the main focus of policy makers and researchers alike. I also focus on the answer to this question in the current report; in this context, the "targeted population" is the offender and "intended effect" is a reduction in recidivism or repeat offending. To be even more precise, the word "effect" implies that a specific policy, intervention, or treatment *causes* a reduction in recidivism behavior. Does an increased chance of getting caught *cause* a reduction in recidivism behavior? Do harsher punishments *cause* a reduction in recidivism behavior? Do better labor market opportunities and decreased liquidity constraints *causally* decrease an offender's propensity to commit a crime again?

Disentangling causality from correlation, i.e., providing scientific evidence of a policy's effects, is a fundamental challenge in many policy arenas. But, the nature of the criminal justice system makes identifying the causal effect of a policy or treatment both especially difficult and important. Let us start with the latter: why is it so important? There are many reasons why a correlation between a treatment and crime can arise, even if there is no underlying causal relationship. In fact, there can be a positive correlation even if the causal effect is in the opposite direction. Thus, if policies or reforms are based on correlational evidence only, then they are unlikely to be effective at reducing crime, and could even increase crime. Given that most crimes are not victimless, there are large potential costs to society of such non-scientific evidence.

Why is it so difficult to disentangle the causal effects on recidivism of a criminal justice treatment or policy from correlation? The simple answer is that the nature of the criminal justice system is—rightly so—not random. Criminal justice authorities do not randomly assign sentences or treatments to offenders; rather, offenders are systematically assigned to sanctions or treatments on the basis of characteristics of the offender and/or offense. Because of this inherent "selection" into treatment, correlations can arise between receiving a treatment (e.g., prison) and the outcome of interest (e.g., recidivism) that are not causal.

Here are some examples. One decision taken by authorities in Sweden is whether an offender should serve his/her sentence under electronic monitoring rather than in prison. But, only certain offenders are eligible for electronic monitoring. As will be discussed in more detail in

Chapter 3, eligibility criteria have changed over time but are a function (at least) of sentence length and employment: individuals with shorter sentences and a connection to the labor market are more likely to be eligible—and to receive and/or take-up—electronic monitoring. But, if these individuals are exactly the same individuals who have a lower propensity to recidivate, then electronic monitoring may appear to reduce recidivism when the effect is completely driven by the "selection" of individuals with a lower recidivism risk into the treatment.

Similar issues arise when considering sanction severity more generally. Sentences are typically determined both by a combination of statutes or sentencing guidelines and judge discretion. Sentencing guidelines in many countries and jurisdictions are in fact explicitly a function of the severity of both the current offense and criminal history. These guidelines prescribe harsher punishments for more serious offenders. But, if these more serious offenders are also those most likely to recidivate, then a positive correlation will arise between punishment severity and recidivism even if harsher punishments have no effect on recidivism.

There is also often room for judges to use discretion in deciding sentences. How such discretion is applied can again yield correlations between recidivism and sanctions that are "biased" estimates of the true causal relationship. The nature of this bias can even be either positive or negative depending on how judges use this discretion. This is highlighted by Manski and Nagin (1998), who consider the challenges of identifying the causal treatment effect of sentencing juvenile offenders to residential treatment facilities. Do judges sentence juveniles to residential placement to minimize recidivism (i.e., on the basis of whom they think will be most affected by this treatment) or do they do so on the basis of who is at the highest risk for recidivism? In the former case, a program or treatment would look very effective while in the latter it could look very ineffective. But these correlations arise because of who is actually "selected" into the program and do not represent the causal effects the program or treatment would have on others. These kinds of selection issues are not just relevant for who gets electronic monitoring or prison (or for how long), but also in many other criminal justice decisions including facility placement, with whom one shares a cell, whether one receives therapy in prison, and the type of therapy or other in-prison treatments and training.

It is thus important but challenging to identify the causal effect of criminal justice policies or treatments on recidivism. The bottom line is that because of the non-random nature of justice, simple comparisons of mean recidivism rates for two groups of "treated" and "non-treated" offenders will not be informative about the causal effect of the treatment. The inherent selection into treatment implies that those who are not treated do not provide a good counterfactual for how the treated group would have behaved in the absence of treatment. We do not observe what would have happened if the treated individuals were not treated!

Researchers play a fundamental role in overcoming these challenges to provide scientific causal evidence on which to base effective and informed policy. In many fields, researchers would conduct *randomized control trials* to disentangle causal effects from correlation. If we were to randomly assign individuals to alternative sentences—say prison or no prison—then the observable (and even unobservable) characteristics of the two groups are similar, and we can use the no prison group as a counterfactual for the prison group. For ethical reasons, such randomized control trials are rarely conducted when studying crime. Researchers thus need alternative approaches. One approach is to *simply control for observable differences* between those in the treated and control groups. Doing so will generally reduce the biases inherent in the raw comparison of means but will not eliminate them. This is because there is often much that is unobserved by the researcher but observed by the criminal justice agents making treatment decisions. For instance, if whether a defendant shows remorse (i.e., something rarely recorded in the data) impacts the sentence, then a relationship between sanctions and recidivism will still occur (if remorse is also related to recidivism). Thus, controlling for observables will rarely be sufficient to reach causal conclusions. The most convincing evidence in the criminal justice space, rather, relies on *quasi-experimental research designs* based on so-called natural experiments. In other words, in the absence of true randomized experiments, researchers need to find "as if" random variation in the real world that affects who gets one punishment versus another.

I do believe that it is essential that policy makers recognize the potential limitations of research in this space. Criminal justice research can take many forms, including both qualitative and quantitative

analyses. And empirical research can be purely descriptive or allow for causal policy evaluations. Descriptive research is surely important—we cannot treat a problem, for instance, without understanding and measuring what the problem is. But for the purposes of making evidence-based policy decisions, not all research is necessarily "good" research. Without credible research designs, empirical evaluations may yield mis-leading conclusions about policy impacts. Even worse, not all researchers acknowledge the limitations of their research in published papers —this can lead uninformed consumers of the research, including many policy makers, to reach unjustified conclusions.

Developing such credible research designs is, however, not easy. Researchers must find "as if" random variation in the implementation of a policy, for instance, that is not meant to be random. Policy makers and governmental agencies can play (at least) three fundamental roles in our ability to conduct high quality quasi-experimental research. First, in-depth institutional knowledge is necessary to develop such research designs, but it is usually the criminal justice agencies and their employees, for instance, who have this knowledge. Regular, transparent, and active conversations between researchers and representatives of these institutions is a necessary ingredient to a good research design. Second, I would encourage policy makers to be forward-thinking when implementing reforms and communicate with researchers in advance. Such conversations could shed light on potential ways in which a reform can be implemented or rolled out that can increase the feasibility of credible causal policy evaluations down the road. Finally, researchers do not only need suitable research designs but also access to the necessary data: we rely on the cooperation of the agencies holding these data.

Focus and organization of the report

The focus of this report is therefore on papers using quasi-experimental research designs to reach credibly causal conclusions about the determinants of recidivism. This research is conducted largely (though not exclusively) by economists who have brought the "credibility revolution" of empirical labor economics to studying the determinants of crime. Thus, this report is not meant to be an exhaustive survey of the literature, but rather aims to highlight contributions by economists to identifying factors that causally impact recidivism. Economists are

not traditionally seen as experts on crime, and are not always given a prominent voice in the criminal justice policy debate. This report aims to bring the scientific evidence provided by economists to the crime control arena.

This report thus summarizes the empirical evidence on the causal impact of four broad factors on recidivism. These factors include: (i) the probability of getting caught, (ii) diversion from the criminal justice system and prison, (iii) prison, and (iv) post-prison environments.

Chapter 2 focuses on changes in the probability of getting caught, with an emphasis on the impacts of DNA databases.

Chapter 3 highlights the impacts of diverting offenders away from the criminal justice system and prison. Such diversions can occur at multiple stages, including prosecutor charge decisions and judge sentencing decisions, via deferred adjudication or electronic monitoring.

Chapter 4 reviews the research on the impact of prison on recidivism. It begins by discussing papers that evaluate the overall impact of a prison sentence. The bottom line from this literature is one of mixed conclusions: some prisons are good, for some offenders. The remainder of this chapter aims to open the black box of prison by reviewing what we know about the impacts of specific characteristics of the prison experience, including time in prison, prison conditions, peers, and treatment.

Chapter 5 highlights the fact that even the very best prisons, for instance, may not prevent recidivism if the conditions of the post-release environment are especially poor. This chapter reviews the literature focusing on the impact of labor market opportunities and public benefits that affect how liquidity constrained offenders are post-release.

The goal of each chapter is to highlight the channels through which these factors can impact recidivism, the reasons why it is hard to identify a causal effect, and the approach and findings of the quasi-experimental literature studying these questions. As will be seen, a significant number of papers are conducted in the Scandinavian context, in part because of the access to high quality register data sets that allow for research designs capable of disentangling correlation and causality.[5]

5. Additional surveys of the literature, especially that in economics, can be found in Chalfin and McCrary (2017) and Doleac (2023); the former discusses deterrents of crime more generally.

When possible, each chapter will emphasize what is known in the Swedish context and/or the relevance of a specific topic to the Swedish debate. The report will also conclude with a more in-depth discussion of what we can learn from the existing literature regarding the potential impacts on recidivism of reforms (recently passed or still on the table) in Sweden.

2. Probability of Getting Caught (Again)

The probability that an offender is caught can be impacted by many factors, including the size, quality and tasks of the police force, the use of surveillance cameras, and even the offender's criminal "skills." In fact, hiring more police is one of the main channels via which the probability of catching an offender can be increased. Though there is a large literature (see Chalfin and McCrary, 2017, for a review) evaluating the deterrence effects of police on crime, this literature tends not to study the effect of policing on recidivism. Broadly speaking, the effect of police on crime is not specific to repeat offending—the topic of this report—but rather criminal behavior overall. The same can be said for surveillance cameras for instance.

This chapter thus focuses on factors that explicitly target the probability of catching a potential recidivist. An early example of such a "technology" is the Register of Habitual Criminals, which was created by the London Metropolitan Police following the 1871 Prevention of Crimes Act. This register included details—names, aliases, physical characteristics, offense history, intended destination—about offenders convicted of more than two crimes who were released from prisons throughout England and Wales. The main purpose was to ease the surveillance and future identification of known offenders. Whether this database actually increased the chance of catching repeat offenders, and whether this deterred recidivism, has to the best of my knowledge not been studied. But, evaluations have been conducted of the impacts of two modern-day versions of this phenomenon—DNA databases and sex offender registries.

DNA Databases

DNA databases of known offenders' genetic profiles were introduced around the world in the 1990s. Depending on the context, national (or state) laws govern who is required to provide a DNA sample to the registry—felony versus misdemeanor offenders, violent versus property offenders, convicted versus suspected offenders, offenders with sufficiently long prison sentences. The basic idea is much the same as the historical habitual offenders registry. By having a DNA profile of a convicted offender in the register, genetic material retrieved at a crime scene can point to DNA matches as potential suspects. These DNA matches are likely to be much more accurate than matching the historical descriptions of an offender (e.g., he had a scar on his cheek) to the habitual offender register. DNA registries can actually decrease crime through two channels. The first is by deterring criminal behavior of those whose DNA is put in the register. The second is by increasing the likelihood of catching repeat offenders, thereby incapacitating them again in the future. In the context of this report, we are interested in the former. Though there are clearly privacy concerns associated with DNA databases, others have argued that the accuracy of such technology has the potential to avoid wrongful and costly convictions.

These registers have expanded in size over time both as new offenders have been caught and added and as jurisdictions have revised their laws to mandate DNA records of a broader set of offenders. Estimating the causal effect of introducing or expanding DNA registries on crime is complicated by multiple issues. The first is omitted variable bias: in this case, the main concern is that a jurisdiction does not just fight crime by introducing or expanding DNA registries, but rather may take other measures at the same time. It can be challenging to disentangle the effect of DNA registries from other contemporaneous (and potentially omitted from the analysis) policies. The second issue is simultaneity bias: these DNA related crime control policies were not randomly introduced, but rather introduced as a response to crime. Since the effect goes both ways—crime impacts policy and policy impacts crime—it is hard to disentangle one path from another. Similar identification problems exist even when studying the effect of registering a specific offender in the database on their recidivism behavior. Not all offenders are required to submit a DNA sample; rather, these laws tend to target

the most serious offenders with potentially the largest chance of repeat offending. In other words, this is another example of the potential "selection" bias issue described in the introduction.

Two papers written by Jennifer Doleac and co-authors identify the causal effect of DNA registries on recidivism behavior by taking advantage of the timing of expansions to the registries. Doleac's (2017) first natural experiment considers expansions (with new categories of offenders) of US state databases; in the US, whether an offender is required to submit a DNA sample to the registry is decided at the state level rather than nationally. Every state had established a DNA database by 1999. Doleac obtained longitudinal criminal history data, which allow her to observe repeat offenders, from the Department of Corrections in Florida, Georgia, Missouri, Montana, New York, North Carolina, and Pennsylvania. Detailed information on conviction and incarceration dates as well as offense types were essential to determine if the offender was required, according to the law, to submit a DNA sample to the database upon release. One caveat of this analysis is that Doleac does not actually observe if an individual is in the registry—just if s/he *should be* in the registry according to the law. But she can take advantage of sharp changes in whether an offender should be in the registry around the dates of state level expansions. Using what is called a regression discontinuity design, Doleac basically compares the recidivism rates for observably similar offenders who were released before and after the expansion date. The intuition of such a design is that being in the DNA registry is not a function of offense severity, but rather the arguably pseudo-random timing in whether one was "lucky" enough to be released before the expansion. She finds that the requirement to submit a DNA sample significantly reduces the five-year chance of a new conviction for serious violent offenders by 17 percent. Effects are smaller and marginally significant for serious property crimes. But, it is important to keep in mind that these are what we call "intent to treat" effects: we do not know if the individual was actually put in the registry, just that they should have been. Moreover, the effects could be a lower bound since these individuals should actually be more likely to be caught if they commit a crime.

Using a very similar research design, the second study is conducted by Anker, Doleac, and Landersø (2021) in Denmark. In this case, the DNA database expansion occurred via a 2005 reform that expanded

the list of crimes that qualified for DNA registration to all offenses with a maximum penalty of 18 months or more in prison. Prior to the reform, suspects of only the most serious crimes (e.g., murder, robbery, arson, major violence, incest, and rape) could be included in the DNA register. After the reform, offenses like burglary and simple violence/assault became eligible. The reform also increased the likelihood of a DNA sample by giving the police more discretion (i.e., before the reform, it was only allowed if necessary for an investigation) and making it easier and cheaper to obtain the DNA samples. In contrast to the US study, the authors can actually observe DNA registration: in the months around the reform, the likelihood of being registered increased from 4 to 40 percent. Once again, the intuition of the research design is to compare recidivism rates for offenders who were "randomized" into the registry based on the timing of their offense. The analysis sample excludes charges for certain minor offense categories, such as traffic offenses and small-scale drug possession, for which these reforms are not relevant. The analysis also focuses on the most criminally active population: relatively young males (aged 18–30). The authors find large effects on recidivism: being added to the database reduces the chance of recidivism in the first year by about 40 percent. These effects are indeed large enough to be observed in aggregate crime statistics, especially when zooming in on the offenses treated by the reform, like burglaries. The largest and most significant and persistent effects are seen for violent offenders; a similar pattern that is less precisely estimated is seen for property crimes. Significant effects are generally not seen for offenders initially charged with sex offenses, weapons offenses, or other penal offenses.

These two studies find generally consistent effects: registration in DNA databases decreases recidivism rates (for those offenses/offenders eligible to be in the database). The results do not tell us what the effect would be of expanding eligibility to more minor offenses, like drug possession. The fact that they are conducted in two very different countries, including Denmark, and criminal justice systems speaks to the potential generalizability of these effects to Sweden and other countries around the world.

DNA Databases: The Swedish Context

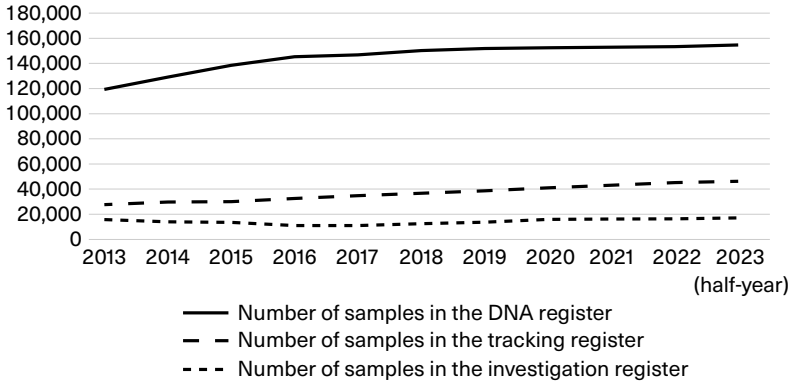
DNA registries do indeed exist in Sweden today. First used by the police in April 1999 (SFS 1998:622), the registries were significantly expanded with reforms enacted in 2006 (SFS 2005:877). These registries are in fact more comprehensive than those described above in the US and Denmark.

More specifically, there are three types of DNA databases in Sweden today. The first database is the *spårregistret* ("tracking register"), which consists of unmatched DNA samples obtained at crime scenes. A sample in this register is generally kept for 30 years and in special circumstances up to 70 years. As seen in Exhibit 2.1, there are more than 40,000 DNA samples in the tracking register today—a statistic that has increased from less than 30,000 ten years ago.

The second register (*utredningsregistret* or "investigation register") allows DNA to be collected from persons who are suspected on reasonable grounds of offenses for which a conviction would likely result in a prison sentence. These DNA are removed from the registry once a suspect is convicted (and the DNA moved to the third registry) or if the person is cleared of suspicion. This database may especially serve a role in *clearing crimes*, as criminal justice agents try to match samples from the investigation register to the tracking register. There have been less than 20,000 DNA samples in the investigation register throughout the last decade (see Exhibit 2.1).

The third database is the DNA register and is that which can play a role in *deterring recidivism*. Individuals convicted with a punishment other than a fine or probation are eligible today to have their DNA registered in the database. This basically includes all offenders with a prison sentence. This DNA will remain in the registry until the criminal record is cleared from the *belastningsregistret* or "criminal background register." As discussed later in the report, criminal records are formulaically automatically cleared after 3, 5, or 10 years, depending on the age at the time of offense (below 18 or not) and the type of sentence. Exhibit 2.1 shows that the size of this main DNA register has increased substantially over the last decade, from around 120,000 individuals in 2013 to nearly 160,000 today. Such an increase could be both due to changing crime rates but also changing practices when it comes to taking DNA samples. While I do not have official statistics on how

Exhibit 2.1 The Number of People in Swedish DNA Registries.



Note: These statistics were sourced from Figure 6 of the Nationellt forensiskt centrum – NFC (2022) annual report for 2021 on DNA registers.

many individuals are eligible but not entered into the DNA registry, anecdotal evidence suggests that the use of the registry is fairly widespread. For instance, a 2017 survey found that 80 percent of persons in charge of an investigation say they always apply for DNA sampling if legally entitled (Regeringens skrivelse 2017/18:50, p. 4).

Reforms have expanded the use of DNA registers in Sweden over time. The "investigation registry" was actually first introduced during the 2006 expansion of DNA sampling. At the same time, the laws expanded regarding to whom DNA samples from the tracking register could be compared. Before the reform, only suspects with potential sentences of at least two years could be considered, but after the reform, any suspect could be considered regardless of the length of the potential prison sentence. Likewise, before the reform, only convicts with a sentence of more than two years were eligible for the registry, but after the reform, eligibility expanded to all prison sentences.

The use of the DNA registries and other types of biometric evidence, including fingerprints, voice recordings, and facial recognition, are being actively investigated and debated in Sweden today. A recent Swedish Government Official Report (SOU 2023:32), for instance,

recommends simplifying the rules for adding individuals to the DNA registry. To the best of my knowledge, research has not been conducted to date evaluating the causal effect of the creation and expansion of DNA sampling in the Swedish criminal justice system on crime clearance and recidivism behavior. And there is no research that I know of on the effect of registries of other types of biometric evidence on recidivism. Thus far, the small body of existing research does suggest that DNA registries can have large deterrent effects on recidivism (for offenders charged with serious enough offenses to be in the databases).

Sex Offender Registration

Sex offender registries differ from the DNA databases in that they are not just meant for the police, but are also often accessible to the public on the Internet. These registries could in theory reduce recidivism by adding a further penalty when convicted for those not yet on the registry, by giving potential victims the opportunity to take more precautions, and via increased monitoring by the police. But, in contrast to the research on DNA databases, quasi-experimental research designs evaluating the recidivism effects of sex offender registries find them to be ineffective or to have mixed effects (e.g., Agan, 2011; Prescott and Rockoff, 2011; Carr, 2015). Given these findings and the narrowness of the criminal justice population to which these registries are relevant, I do not discuss these studies in detail.[6]

6. To the best of my knowledge, such sex offender registries do not exist in Sweden. There was a motion, inspired by the US context for instance, suggesting a public registry for serious sex offenders in 2012 that did not pass. See: https://www.riksdagen.se/sv/dokument-och-lagar/dokument/motion/offentligt-register-over-domda-pedofiler_h002ju371/). Last accessed November 7, 2023.

3. Alternatives to Prison: Diversion and Electronic Monitoring

Criminal justice systems around the world are contending with multiple issues related to growing prison populations, which is occurring both due to increasing crime rates but also longer prison sentences. One first-order issue is capacity constraints being reached and over-crowding. Another increasingly prevalent concern is about the potentially harmful effects of incarceration, especially for minor offenders. Such an issue may be especially salient if prison conditions deteriorate due to overcrowding.

This chapter discusses the impacts on recidivism of two approaches taken by criminal justice systems around the world to address these issues: diversion and electronic monitoring (EM).

3.1 Diversion from the Criminal Justice System

"Diversion" is a label given to a wide range of approaches used by criminal justice systems to keep individuals out of the legal system and from beginning a lifetime of prison spells. Offenders can be "diverted" at multiple points and by different actors. The police can choose to divert people suspected of minor (misdemeanors or nonviolent) crimes from arrest. Prosecutors can divert individuals before making a charge. And the courts and judges can divert individuals, often through what is called deferred adjudication, from a conviction and sentence. But, does keeping minor offenders away from the potentially harsh criminal justice system, and giving them a second chance, reduce or increase recidivism? On the one hand, if convictions and sanctions like prison lead to stigmas that decrease the chance of employment (as discussed

in Chapter 5), then diversion can reduce recidivism by avoiding such a stigma. On the other hand, if diversion decreases the perceived probability and severity of punishment, then the offender may be more likely to commit crime again.

Whether or not diversion works is thus an empirical question. But like all questions highlighted in this report, it is not an easy one to answer. Who should be diverted? Generally speaking, the police, prosecutors, and judges want to divert those offenders who are the least criminal—perhaps with no criminal record to date—and for whom a conviction or incarceration spell could be the most stigmatizing and harmful. In other words, offenders are "selected" into diversion on the basis of characteristics related to the propensity to recidivate. We would thus expect to see in the raw data that diverted offenders recidivate less than non-diverted offenders simply because of this selection. But does diversion cause a decrease in recidivism?

A handful of recent papers provide convincing causal evidence that diversion in fact can work for relatively minor offenders. Mueller-Smith and Schnepel (2021) evaluate the practice of deferred adjudication in Harris County, Texas (which contains the city of Houston). In this context, individuals can avoid a felony conviction if they successfully complete a period of community supervision, which operates in a similar manner to probation. The program is most commonly used for first-time felony defendants who are charged with less serious felony offenses; the most serious violent and sex offenses are generally ineligible. About half of the offenders who receive such a sentence in Harris County successfully complete the program; for these individuals, the case is dismissed. How do the authors deal with the above-described selection bias problem? They take advantage of two sentencing reforms that lead to sudden shifts in the likelihood of diversion. One reform in 1994 sharply reduced diversion rates in the days after the reform, while the reverse happened in 2007. Using a regression discontinuity design, the authors essentially compare the outcomes for offenders whose cases were disposed in the days immediately before and after the reforms. The underlying intuition is that the dates that the defendants appear in court around the reform implementation is random, as if in a randomized control trial. Mueller-Smith and Schnepel (2021) find that recidivism rates are cut approximately in half and employment outcomes improve. The authors argue that this effect is driven by

avoiding the stigma of a conviction, since similar effects are not seen for those who already had a felony conviction on their record.

Agan, Doleac, and Harvey (2023) reach similar conclusions in a very different context—prosecutor decisions not to prosecute non-violent misdemeanor offenses in Suffolk County, Massachusetts (where Boston is located). These authors also take a different approach to deal with the selection bias issue. They capitalize on a feature that is inherent in many justice systems: offenders are randomly assigned to prosecutors who have differing propensities to take such misdemeanor cases forward. In other words, some offenders are "lucky" and assigned a lenient prosecutor while others are unlucky and receive one who is harsh. (We will see this research design reappear again and again in this report, as such random assignment is central to many aspects of the judicial process.) The authors find that defendants who were lucky are more than 50 percent less likely to be seen in court again over the next two years: in other words, recidivism decreases. Similar to the previous paper as well, the effects are largest for first-time defendants.

A final example is the evaluation by Shem-Tov, Raphael, and Skog (2024) of a restorative justice conferencing program for juveniles in California. The program is called Make-it-Right. It is meant to divert teenagers who would have been charged with medium severity felonies (e.g., burglary, theft, and assault) from the path of regular felony prosecution. They evaluate the program through a (rare) randomized control trial. The sample is small, but the effects are large enough to conclude that the Make-it-Right program reduced recidivism: youths assigned to this program are 44 percent less likely to be re-arrested within six months of randomization compared to the control group. Why do these effects occur? The program has two basic components. Like the other diversion programs evaluated above, youths in the program are diverted from felony prosecution. But they are also treated with restorative justice conferencing, which brings together the victim, accused and supporters of both parties. The result of this "conference" is an agreement in which the accused makes amends for the crime through a mutually agreed-upon set of actions. Central to the process is the individual charged with the offense taking responsibility for and acknowledging the impact of their actions on the victim(s), family, and community. The authors find evidence suggestive of the results being driven by the restorative justice treatment. Specifically, similar findings

are seen even when excluding all individuals in both the treatment and control groups who are convicted.[7]

DIVERSION IN THE SWEDISH CONTEXT?

The above-described literature highlights three papers that find convincing causal evidence that diversion from the criminal justice system can decrease recidivism, at least for low-level minor (often first-time) offenders. But, it should be noted that all of these studies are in the context of the United States, where the consequences of a conviction can (potentially) be much more severe than in Sweden. For instance, criminal records are more public, and are not automatically cleared after a certain number of years. Sanctions are harsher: prison is more common, sentences are longer, and conditions are worse. Diversion from these consequences may thus have very different effects than diversion from a conviction and/or prison in the Swedish context, especially given the emphasis placed on rehabilitation in Swedish prisons. Caution must thus be used in generalizing the findings of these studies to Sweden today.

3.2 Electronic Monitoring

Electronic monitoring (EM), which many argue is a more humane alternative to prison, is being increasingly used as an alternative to incarceration for individuals with relatively minor offenses. There are two types of electronic monitoring—front-door and back-door. Front-door EM is when individuals are "held" under electronic monitoring as a substitute for either pre-trial detention or a prison sentence. Back-door EM occurs when defendants are released early from a prison spell to serve the remainder of their sentence on EM. In either type of EM, individuals are equipped with a technology, often an ankle-bracelet, that monitors their location via GPS. There are typically additional rules (e.g., curfews, alcohol testing, and employment) that must be followed.

7. It should be highlighted that the "standard" control prosecution path does not necessarily include conviction.

ELECTRONIC MONITORING (FOTBOJA) IN SWEDEN

Front-door electronic monitoring, in which EM (at home) is substituted for prison time, is the main type of EM usage in Sweden. It was introduced as a small pilot in 1994 in selected districts for sentences of two months or less. After an initial evaluation, it was expanded in 1997 to all districts and longer sentences (sentences less than four months). See Brå (1999) for details. Another expansion occurred in 2005 to sentences less than seven months in length. But, how much is electronic monitoring used in the Swedish criminal justice system? Using an extract of data from Kriminalvården, I can observe that though offenders with sentences of less than seven months are in theory eligible (after 2005), the proportion taking up EM as a substitute for prison is generally higher for those with shorter sentences than for those with longer sentences. Moreover, the annual share of prison spells served in EM tends to lie between about 20 and 55 percent, depending on the sentence length.

Understanding the eligibility criteria and how electronic monitoring is assigned to offenders is essential to both (i) recognizing the challenges faced by researchers in disentangling the causal effect of EM from correlation and (ii) developing solutions to overcome these challenges. The basic eligibility requirements in Sweden, besides the sentence length, are that the offender has acceptable accommodation, regular employment, drug and alcohol testing, household consent, and agrees to home visits by probation officers. Breaches of program conditions can result in serving the remainder of the sentence in prison.[8] In contrast to some other countries, judges in Sweden do not decide electronic monitoring. Rather, the prison authorities have the ability to shift some offenders, according to the eligibility criteria, into EM. But, until a recent reform, inmates themselves had to make the request to serve their sentence in EM rather than prison. This final requirement was relaxed via the government's proposition (Prop. 2021/22: 196), which was enacted in October 2022 (SFS 2022: 1097). With the ambition of increasing the number of offenders serving prison sentences via EM (in part to alleviate prison overcrowding), this legislation enables Kriminalvården itself (as opposed to only the offender) to initiate an

8. See Kriminalvården's website for more details on electronic monitoring in Sweden: https://www.kriminalvarden.se/fangelse-frivard-och-hakte/fangelse/fotboja/.

application for electronic monitoring. This reform is, however, too recent to assess how much Kriminalvården exercises this discretion.

Thus, electronic monitoring is not randomly assigned but rather systematically assigned on the basis of observable characteristics that are likely to be directly related to recidivism—e.g., offense severity (sentence length) and employment. In general, one expects that those least likely to recidivate are assigned to EM as opposed to prison. In other words, this selection on observables and unobservables will likely suggest in naïve statistics that electronic monitoring reduces recidivism, even if there is no underlying causal effect.

THE EFFECTS OF ELECTRONIC MONITORING ON RECIDIVISM

Of all the literatures and "interventions" surveyed in this report, the findings regarding the effects of electronic monitoring on recidivism are perhaps the most uniform. Researchers have studied EM in multiple countries: Sweden, Norway, Argentina, France, the United Kingdom, Australia, and the United States. These studies include evaluations of EM both as a front-door alternative for pre-trial detention and/or prison and as a back-door option for early prison release. Multiple research designs have been used to identify the causal effect of EM, including the quasi-random assignment of judges with differing EM propensities, the roll-out of electronic monitoring over time and regions, and variation in sentences to EM generated by eligibility rules.[9] Moreover, different definitions of recidivism (re-arrest, re-conviction, or re-incarceration) measured over both short and long-term horizons have been used. The bottom line is that across countries, contexts and research designs, there is almost no evidence that electronic monitoring increases recidivism and fairly consistent evidence that, if anything, it reduces recidivism. Effect sizes do vary, with the estimates from the Swedish context amongst the smallest; potential reasons for this are discussed below. Another finding that is common across many studies, however, is that the estimated effects of EM on recidivism are smaller when quasi-experimental research designs are used: simple compari-

9. Others have studied the impact of EM on non-crime outcomes. For instance, Larsen (2017) finds a large increase in the chance of completing high school as a result of introducing EM in 2006 to offenders under age 25 with sentences of 3 months or less in Denmark.

sons of recidivism rates for those selected to EM versus those who are not are certainly biased.

Exhibit 3.1 lists each study, and highlights the country and context, research design, and main findings. Rather than going through the details and findings of each study, the remainder of this section provides the intuition of the research designs used to identify the causal effects and the details of the most recent Swedish study. I conclude with a brief discussion of what we do and do not yet know, especially in the context of Sweden.

An increasingly common approach to identify the effect of sanctions on recidivism is to use what we call a judge instrument design. Such a design takes advantage of the fact that offenders are randomly assigned to judges with differing propensities to impose certain (harsher) punishments. If an offender is "lucky" enough to be assigned to a lenient judge, then this offender will be more likely to have the lenient punishment (in this case EM versus prison) compared to an offender with identical characteristics (charges, criminal history, demographics, etc.) who walked into a different courtroom. In this way, there is exogenous variation in who gets assigned a sentence of electronic monitoring. Such a design cannot be used, however, to evaluate Swedish EM, since EM is not a judge decision. This design has been used in the Argentina, Australia, and US studies.

A second approach that has been used in the Norwegian, French, and Swedish contexts is a differences-in-differences design that takes advantage of the way in which electronic monitoring is introduced into a criminal justice system, i.e., the fact that it is not rolled out to a whole country at once but to different regions and/or sentence ranges at different points in time. The intuition underlying such a design is that it compares how recidivism rates change (before and after the introduction of electronic monitoring) for a "treated group" that is eligible for electronic monitoring to how recidivism rates change for a "control group" that is ineligible. This allows us to control for other factors that may be changing over time and also impact recidivism rates.

A final design is a regression discontinuity design, which takes advantage of discontinuities in the propensity to receive EM as a function of thresholds in the eligibility rules. This design has been used to study back-door electronic monitoring in England and Wales, where there are sharp jumps in the chances of EM for sentences over 90 days long

Exhibit 3.1 Quasi-Experimental Evaluations of Electronic Monitoring on Recidivism.

Paper	Country/Context	Research Design	Main Findings
Di Tella and Schargrodsky (2013)	Buenos Aires, Argentina. *Front-door* EM used for pre-trial detention from 1998 to 2007. Comparison: Effect of pre-trial EM versus pre-trial incarceration (in overcrowded prisons with poor conditions)	Quasi-random assignment of judges	Significant and large reduction (48 percent) in recidivism if assigned EM. Recidivism Definition: Return to prison by end of sample period (up to 3 years depending on release date).
Williams and Weatherburn (2020)	*Front-door* EM as alternative to prison sentence for non-violent offenders in New South Wales, Australia. Comparison: EM versus prison sentence.	Quasi-random assignment of judges	Studies long-term effects of EM on recidivism. Recidivism decreases by 22 (11) percentage points at 5 (10) year horizons. Recidivism Definition: Before the court on new charges within t months of sentencing. Number of offenses reduced by 45 percent from EM.
Rivera (2023)	*Front-door* EM as alternative to (i) pre-trial release and (ii) pretrial detention in jail in Cook County, Illinois (United States)	Quasi-random assignment of bond court judges	Effects of EM are heterogeneous. EM appears to reduce recidivism relative to detention, but beneficial results are not as clear when compared to those likely to be released. Recidivism Definition: Varies with type of analysis. Looks both at misconduct pretrial (e.g., failure to appear, new cases, and types of new charges) and post trial outcomes (e.g., number cases/felony cases within 4 years)

Paper	Country/Context	Research Design	Main Findings
Henneguelle, Monnery, and Kensey (2016)	Pilot of *front-door* EM as alternative to prison sentence in France. Comparison: EM versus prison sentence.	Quasi-random variation in use of EM across courts driven by timing of EM roll-out.	EM reduces recidivism (after 5 years) by about 10 percent. It applies to a wide range of offenders. Recidivism Definition: Re-conviction in 5 years Effects with quasi-experimental design are much smaller than those with naïve comparisons or controls for observables.
Andersen and Telle (2016)	*Front-door* EM as substitute for prison in Norway in 2002–2011.	Differences-in-differences design that takes advantage of geographic roll-out of EM across some Norwegian counties starting in 2008 and number of available bracelets (supply constraints).	Significant reduction in recidivating at all within two years due to EM, but no effect on number or severity of offenses. Strongest effects for those without previous incarceration and recent unemployment. (But, note concerns on assumptions needed for causal interpretation.) Recidivism Definition: Suspected of new criminal offenses within 1, 2, or 3 years.
Grenet, Grönqvist, and Niknami (2024)	*Front-door* EM as substitute for prison in Sweden.	Differences-in-differences design that takes advantage of 1997 rollout of EM in Sweden to just offenders with sentences up to 3 months. Compares differences in recidivism before/after reform for eligible offenders to differences for ineligible offenders.	Introduction of EM (for sentences less than 3 months) reduces recidivism: about 2 percent for re-conviction and 4 percent for re-arrest. Recidivism Definition: Re-conviction or re-arrest (in suspects register) within 3 years of trial. Labor market outcomes improved: Chance of being employed after three years increased by about 13 percent and average earnings by 23 percent.

Paper	Country/Context	Research Design	Main Findings
Al Weswasi and Bäckman (2024)	*Front-door* EM as substitute for prison Sweden.	Differences-in-differences design that takes advantage of the 2005 expansion of EM in Sweden to sentences of 4–6 months. Compares the change in outcomes for all 4–6- months sentences before and after the reform with the change in outcomes for shorter sentences (which were already eligible for EM).	Expansion of EM to 4–6 months sentences reduces recidivism at short and long run (10 year) horizons. Recidivism Definition: Re-conviction and re-incarceration. Effects are primarily driven by those with more stable labor market attachment prior to the prison sentence.
Marie (2022)	*Back-door* (early release) EM (called Home Detention Curfew) in England and Wales. Assignment to EM in part a function of risk assessment.	Regression discontinuity design that takes advantage of rules that provide exogenous variation in who receives EM. Rules increase chance of EM if sentences greater than 90 days and/or age over 18.	Both research designs find a reduction in recidivism associated with EM. Estimates range from a reduction of 18–37 percent during 24-month post-release period. Recidivism Definition: Released prisoner being arrested within 24 months.
Marklund and Holmberg (2009)	Evaluates the effect of *back-door* EM or early release (for the first 260 individuals participating) in Sweden. Treated: ankle bracelet, mandatory daily occupation, regular sobriety checks.	Compares recidivism outcomes between treated group and a matched control group.	Early release EM did not increase recidivism, and some evidence that it decreased crime, especially for those with moderate offending histories. Recidivism Definition: New convictions and new prison sentences within 3 years of release.

and offenders 18 and over. In such a design, we can compare recidivism rates for individuals in very small windows on either side of these cut-offs: the intuition is that whether one is, for example, 7 days before or after their 18th birthday at the time of decisions is as good as random.

One issue to keep in mind with all of these studies is that each one identifies what we call a "local" effect. In the judge design, we identify the effect of EM for those individuals for whom the judge's decision is not obvious, i.e., marginal cases for which judges use discretion. In the regression discontinuity case, we identify the effect for individuals close to age 18 or with sentences of 90 days. The local nature of these estimates raises the question of external validity: do the findings generalize to other contexts? The fact that the same findings are seen across multiple studies suggests that, at least to some extent, they do.

THE EFFECTS OF ELECTRONIC MONITORING ON RECIDIVISM IN SWEDEN

There are two published studies (Marklund and Holmberg, 2009; Grenet, Grönkvist, and Niknami, 2024) and one working paper that is soon to be published (Al Weswasi and Bäckman, 2024) of the effects of EM on recidivism in Sweden. The Marklund and Holmberg study is one of the first evaluations of EM on recidivism. This paper evaluates the effect of back-door EM in Sweden for the first 260 individuals to participate in the Swedish early release program. To try to deal with the positive selection into EM, the authors compare recidivism rates between the EM treated group and a matched control group; matching is another strategy which essentially allows researchers to control for observable differences between the treated and control groups. Marklund and Holmberg (2009) find some evidence that EM decreased recidivism, especially for those with moderate offending histories.

The second Swedish study evaluates the introduction of front-door EM in Sweden for sentences of up to three months in 1997. Grenet, Grönkvist, and Niknami (2024) use a differences-in-differences design where they define the treatment group as offenders with sentences of three months or less and the control group as offenders with sentences of four to twelve months. The latter group was not eligible for EM. The former group was eligible if they met the other eligibility criteria (e.g., acceptable accommodation, regular employment, and agreement to

alcohol and drug testing and home visits by the Swedish Prison and Probation Service). In this Swedish context, the authors note that most individuals who take up EM were convicted of drunk driving or violent crimes. To estimate the causal effect of the EM reform on crime, the authors compare the change in recidivism rates (before and after the reform) for individuals in the control group to the change for those in the treatment group. The authors find that EM lowers the chance of reoffending within three years by about 2 percent when using re-conviction and 4 percent when using re-arrest. One caveat to keep in mind is that these estimates are the overall effect of the reform: not everyone in the treated group actually serves their sentence via EM. This could in part explain why the effect sizes are smaller here than for other contexts. Though these recidivism effects are the focus of this report, the most novel findings of the work by Grenet, Grönkvist, and Niknami (2024) are those related to why EM has positive effects and whether there are spill-over effects onto family members. The authors in fact find very large effects of EM on employment outcomes: being employed within three years increases by about 13 percent while average earnings increase by about 23 percent. There are also positive spill-over effects: children whose parents serve their prison spell in EM are more likely to complete compulsory school and have higher adult disposable income. In other words, there may be additional benefits to EM—besides reduced recidivism—that justify its use.

In a recent working paper, Al Weswasi and Bäckman (2024) use a similar design to evaluate the impact of the 2005 extension of the electronic monitoring to sentences of four to six months. These authors study how recidivism outcomes changed for offenders with a sentence of four to six months before the reform compared to similar offenders after the reform. Because they would like to control for other determinants of recidivism that could be changing over time, they compare the change in recidivism for those with four-to-six-month sentences to that for a control group of offenders with less than four-month sentences. These individuals were already eligible for electronic monitoring prior to the reform. As in the Grenet, Grönkvist, and Niknami analysis, Al Weswasi and Bäckman also find that expanding electronic monitoring does not increase recidivism and, in the majority of their empirical specifications, significantly decreases it. They also find that these effects are primarily driven by individuals whose pre-prison attachment to the labor market is relatively high.

SUMMARY AND REMAINING QUESTIONS

Overall, there is a growing body of research that demonstrates that the use of EM as an alternative to prison reduces recidivism rates (via both front-door and back-door EM). This literature includes three studies of EM in Sweden, and though the size of the effects on recidivism are not always large, there are findings of other potential benefits for the offender and their family members. One limitation of both the Al Weswasi and Bäckman (2024) and Grenet, Grönkvist, and Niknami (2024) studies, however, is that they both primarily only estimate the intent to treat—in other words, the effect of being eligible for electronic monitoring rather than the effect of experiencing electronic monitoring oneself. Since the take-up of electronic monitoring is largely determined by an offender's motivation and self-initiated application during this period, which could also be related to recidivism, it is hard to confidently understand what drives these effects. As a result, we do not know the effect of (i) the recent reforms giving Kriminalvården the discretion to nudge offenders into EM who would not have applied themselves and (ii) further relaxing the eligibility criteria beyond six-month sentences—something that could feasibly be considered in trying to address prison over-crowding issues today. Caution should be exercised, and further evaluations conducted.

4. Prison and Recidivism

A growing literature evaluates the effect of prison on post-release crime outcomes as well as other measures of societal reintegration, including labor market outcomes, health, and family dissolution. This report focuses on the first-order outcome of crime desistance: Does prison decrease recidivism? As will be seen below, the findings are mixed. Some prisons appear to reduce crime for some individuals. Making policy recommendations on the basis of such heterogeneous findings is difficult and necessitates an understanding of why the findings diverge.

One potential explanation is that prison is not a simple well-defined treatment, but rather a black box of many treatments (Western, 2021). These include differing facilities, sentence lengths, conditions (e.g., over-crowding, violence, visiting rights, etc.), peers in prison, health-care and substance abuse treatment, and education and training. Until recently, however, the vast majority of research evaluated the overall effect of the prison black box rather than the various dimensions of the box. This chapter reviews both the literature on the overall effect and that at the frontier of opening the box.

Thus, some prisons may be more effective at reducing crime due to differing components of the black box. A priori, it is not clear whether it is in fact prisons with harsher conditions (that lead to specific deterrence) or prisons with a rehabilitation emphasis that reduce crime. It could even be the case that both do—at least for different subsets of the population.

In a similar vein, another explanation for divergent findings is that prison does not have the same effect on all individuals. This could yield heterogeneous findings if samples differ across studies or, more subtly,

there are differences in the so-called marginal offender off of which a quasi-experimental design identifies a causal effect.

4.1 The Effect of Prison

Empirical researchers studying the effect of serving any time (or more time) in prison on recidivism must overcome the problem of "selection." The more serious the offense or the more serious the criminal history, the more likely it is that an offender receives a (longer) prison sentence. It is thus not surprising that, in the raw data, recidivism rates are higher for offenders sent to prison than those who are not. Nor is it surprising that recidivism rates are higher for offenders with longer than shorter sentences. These relationships, however, may simply be correlational: does prison or more time in prison actually cause higher recidivism rates? A first step towards determining causality would be to adjust the raw relationships for those variables that impact whether one is sentenced to prison, i.e., to control for the severity of the offense and offender. Studies that do this generally find that the positive relationship is attenuated towards zero, but rarely find that the "effect" actually turns negative: does this mean that prison does not reduce recidivism? The fundamental problem in analyses that only control for observable characteristics of the offender and offense is that we, as researchers, rarely observe all relevant factors or (as highlighted in the introduction) the decision rules used by judges. Many factors for instance may be difficult to quantify or measure, such as a defendant's remorse. Quasi-experimental research designs are needed to identify causal effects in the presence of such a non-randomly assigned treatment.

Similar to the above discussion of electronic monitoring and alternatives to prison, there are two core types of analyses—judge instruments and sentencing grid-based regression discontinuity designs—used by researchers to estimate the treatment effect of prison on recidivism. Loeffler and Nagin (2022) provide a thorough review of this quasi-experimental literature.

The intuition underlying the *judge instrumental variable design* is that even though offenders are not randomly assigned to prison, there is some randomness in the sentence driven by their luck of the courtroom draw. The random assignment of offenders to judges, with

differing propensities to use prison as a sanction, generates some exogenous variation in who goes to prison. Thus, judge instrumental variable designs do not identify the causal effect of prison by comparing all offenders in prison to all who are not. Rather, causality is obtained by comparing those offenders who went to prison due to drawing a harsh judge versus those who did not because they drew a more lenient judge. Of course, there are many criminal cases in which all judges—regardless of how lenient or harsh they are—would sentence the offender to prison. And there are cases where all judges would abstain from prison. The causal effects in a judge design are identified from those cases in which there is some discretion—i.e., cases for what we call the marginal offender. And this may vary across contexts: the marginal offender sentenced to prison may be very different in Sweden than the US for instance.

The second design takes advantages of discontinuities in sentencing outcomes that are driven by sentencing guidelines. Many criminal justice systems use a sentencing grid to determine the recommended sentence. These grids are typically a function of two scores: a criminal history score and a current offense class or score. Within these grids, there are typically cutoffs beyond which a harsher punishment is prescribed. Thus, rather than comparing all individuals who go to prison to all individuals who do not, this regression discontinuity design essentially compares those offenders who were "unlucky" enough to fall just above a cutoff to those who were lucky and fell in the grid just below. Offenders just above and below such cutoffs are very similar in terms of their scores but different in terms of their judicial treatments. In this way, researchers can control for even unobservable differences between treated and untreated offenders because they utilize that pseudo-random variation in the prison treatment driven by "luck."

In Hjalmarsson (2009), I used such a design to study the impact of juvenile incarceration in Washington State on recidivism. I use this example here to illustrate the intuition of such a design. Exhibit 4.1 displays the 1998 sentencing grid. On the y-axis is the current offense class: this ranges from a class E misdemeanor to a class A+ felony. On the x-axis is the criminal history (or adjudication score). Offenders receive 0.25 points for each past misdemeanor conviction and 1.0 points for each past felony. The total score is rounded down to determine in which column the offender falls. Individuals whose history and current

Exhibit 4.1 Washington State Juvenile Sentencing Grid.

Current offense class	0	1	2	3	4 or more
A+	180 weeks to age 21 for all category A+ offenses				
A	103–129 weeks for all category A offenses				
A-	15–36 Except 30–40 weeks for 15 to 17 year olds.	52–65	80–100	103–129	103–129
B+	15–36	15–36	52–65	80–100	103–129
B	LS	LS	15–36	15–36	52–65
C+	LS	LS	LS	15–36	15–36
C	LS	LS	LS	LS	15–36
D+	LS	LS	LS	LS	LS
D	LS	LS	LS	LS	LS
E	LS	LS	LS	LS	LS

Rounded adjudication score

Note: This grid is sourced from Hjalmarsson (2009) and was effective for any offense committed on or after July 1, 1998. LS = Local Sanction.

offense place them in a shaded cell receive a local sanction, such as a fine or community service. All others receive a sentence to incarceration in a state detention facility for the time period (in weeks) indicated by the grid. Take an offender who has committed a class B felony as an example. All such offenders with a criminal history score of 1.75 or less will receive a local sanction while those with a 2.0 or more will be incarcerated. Comparing everyone to the right of the cutoff to everyone to the left implies a comparison of those with almost no criminal history to those with very serious histories. But, if one focuses on only those near the cutoff, then we can compare individuals with the same current offense and *almost* the same history. This is a simplified version of the design, but provides the general intuition.

The remainder of this section presents the results from a selection of empirical studies, listed in Exhibit 4.2, using these designs to estimate the effect on recidivism of any time in prison as well as the effect of more time in prison. I label these the extensive margin and intensive margin effects of prison, respectively. In other words, the literature considers not only the possibility that simply being exposed at all to prison (the extensive margin) can matter for post-release crime but that the length of the exposure (the intensive margin) also can matter.

Exhibit 4.2 Quasi-Experimental Valuations of Effects of Prison on Recidivism.

Paper	Country/Context	Research Design	Main Findings
Panel A. Extensive Margin Effect of Any Prison: Juveniles			
Hjalmarsson (2009)	Juvenile detention in Washington State	Sentencing grid-based regression discontinuity	(-) Incarcerated juveniles are significantly less likely to be re-arrested.
Aizer and Doyle (2015)	Juvenile detention in Cook County, Illinois	Judge instrumental variable	(+) Incarcerated juveniles are more likely to be incarcerated as an adult.
Eren and Mocan (2021)	Juvenile detention in Louisiana	Judge instrumental variable	(+) Incarcerated juveniles more likely to be convicted of drug offenses as an adult. (-) Incarcerated juveniles less likely to be convicted of property offenses.
Panel B. Extensive Margin Effect of Any Prison: Adults			
Bhuller et al. (2020)	Incarceration in Norway	Judge instrumental variable	(-) Incarceration significantly reduced recidivism. Results are driven by individuals previously unemployed prior to prison, for whom incarceration increased employment program participation and future employment and earnings.

Paper	Country/Context	Research Design	Main Findings
Green and Winik (2010)	Incarceration in Washington DC	Judge instrumental variable	(/) No observable effect after 4 years.
Loeffler (2013)	Incarceration for felony defendants in Cook County Illinois	Judge instrumental variable	(/) No significant effects on recidivism (or employment).
Mueller-Smith (2015)	Incarceration in Harris County, Texas	Judge instrumental variable	(+) Incarceration increases the chance of re-arrest.
Nagin and Snodgrass (2013)	Incarceration for felony defendants in Pennsylvania.	Judge instrumental variable	(/) No effect on re-arrest in the short or long term (1–10 years).
Huttunen et al. (2023)	Incarceration for defendants charged with financial crimes in Finland.	Judge instrumental variable	(-) Incarceration reduces recidivism in three years following crime and that colleagues are charged with crime.
Panel C. Intensive Margin Effect of Any Prison: Adults			
Rose and Shem-Tov (2021)	Time served in North Carolina prisons	Sentencing-grid based regression discontinuity	(-) More time in prison reduces recidivism. Large and significant effects seen in both medium term (3 years) and long term (8 years).
Kuziemko (2013)	More time in Georgia (US) prisons	Sentencing reform that eliminated parole for certain offenders	(-) More time in prison decreases recidivism risk.
Hjalmarsson and Lindquist (2022)	More time in Swedish prisons	Sentencing reform: two-thirds reform in 1993 and 1999	(-, /) On average, more time in prison decreases recidivism (re-conviction) in the short run. Heterogeneity analyses (shown in Online Appendix) suggest effects are driven by certain subgroups, including individuals who were previously incarcerated and property offenders. No effects are seen for those with no previous incarceration. Mortality (the focus of the paper) also decreases.

Paper	Country/Context	Research Design	Main Findings
Al Weswasi et al. (2023)	More time in Swedish prisons for first-time incarcerated adult offenders	Sentencing reform: 1983, 1993, 1999 reforms	(/) No effect on recidivism.
		Panel D. Spill-Over Effects of Prison	
Bhuller et al. (2018a)	Spill-over effects of Norwegian prison sentences onto children	Judge instrumental variable	(/) No effect on child criminal behavior of paternal incarceration.
Dobbie et al. (2019)	Spill-over effects of parental incarceration in Sweden	Judge instrumental variable	(+) Parental incarceration significantly increases teenage crime and worsens education and employment outcomes. Effects are driven by disadvantaged families.
Norris, Pecenco, and Weaver (2021)	Spill-over effects of parental and sibling incarceration in Ohio	Judge instrumental variable	(-) Incarceration significantly reduces chance of child and sibling incarceration.
Bhuller et al. (2018b)	Spill-over effects of Norwegian prison sentences onto crime of brothers and criminal network members	Judge instrumental variable	(-) Incarceration significantly reduces chance that younger brothers and criminal network members are charged with a crime over the next four years.

Note: This table highlights the main quasi-experimental papers studying the effects of incarceration at the extensive and intensive margin on the recidivism behavior of the offender. Spill-over effects are also highlighted. To summarize the results, the following symbols are used. (-) indicates that incarceration *reduces* recidivism, (+) indicates an *increase* in recidivism and (/) indicates a *null* finding. This is by no means an exhaustive list of papers.

THE EFFECT OF ANY TIME IN PRISON: EXTENSIVE MARGIN

Juveniles

Panel A of Exhibit 4.2 presents the extensive margin effects of prison (i.e., the effects of *any* time in prison) for juveniles. Three studies test whether incarceration in a juvenile facility impacts recidivism. The first is the paper described above in Washington State (Hjalmarsson, 2009), which finds evidence that incarcerated juveniles (compared to those who receive a local sanction, like community service) are less likely to be re-arrested. But, these results contrast with those from a judge based design in Illinois. Aizer and Doyle (2015) find that juvenile incarceration (in Chicago) increases adult incarceration. Aizer and Doyle also put forward evidence of a potential mechanism: juvenile incarceration in Chicago increases the chance of dropping out of school. As highlighted by Becker's economic model of crime and described in my previous SNS report (Hjalmarsson, 2022), educational attainment is an important pathway through which criminal behavior can be impacted. In another judge study of juvenile incarceration in Louisiana, Eren and Mocan (2021) find mixed results: adult drug offenses increase while property offenses decrease.

In summary, the results of three causal studies of the effects of juvenile incarceration are mixed. Though each study is conducted in the US context, they evaluate juvenile facilities in three different states, which may have very different incarceration conditions. For instance, the amount spent per adult prisoner varies significantly across states: the Vera Institute estimates a cost in 2015 of $33,507 in Illinois, $37,841 in Washington, and $16,251 in Louisiana.[10] In addition, though education is mandated for incarcerated youths (up to the state-specific mandatory schooling age, which today is 17 in Illinois and 18 in Washington and Louisiana), the quality of schooling within these facilities may vary.

Adults

Panel B of Exhibit 4.2 presents six papers evaluating the impact of adult incarceration on recidivism using the judge instrumental variable design. Four are conducted in the US (Washington, DC, Illinois, Texas,

10. See https://www.vera.org/publications/price-of-prisons-2015-state-spending-trends/price-of-prisons-2015-state-spending-trends/price-of-prisons-2015-state-spending-trends-prison-spending. I do not have similar statistics for juvenile facilities.

and Pennsylvania) while two are set in a Scandinavian context (Norway and Finland). Once again, the findings are heterogeneous across studies. The US based studies either find no significant effects on recidivism (Green and Winik, 2010; Loeffler, 2013; Nagin and Snodgrass, 2013) or a criminogenic effect, in which prison increases post-release crime (Mueller-Smith, 2015).

In contrast, the Norwegian study by Bhuller et al. (2020) finds that incarceration significantly reduced recidivism. Given the similarities between the Norwegian and Swedish prison systems, I discuss the findings for Norway in a bit more detail. The main analysis in this paper is based on criminal cases decided between 2005 and 2009, so that each offender can be followed for at least five years. Some restrictions need to be made to implement a judge design. For instance, courthouses must have more than one judge—otherwise, there would not be random assignment to judges. And judges must see a sufficient number of cases to measure how stringent they are in sentencing decisions. The final sample includes more than 33,000 cases and 500 judges. The authors highlight three main findings. First, incarceration reduces recidivism: the chance of reoffending decreases by 27 percentage points in the five years after conviction. These results are not just driven by incapacitation while in prison, as they are also seen after release. Second, not using the judge instrument design would yield a positive correlation: prison would appear to be criminogenic in naïve analyses of the data. Third, the reduction in recidivism is driven by individuals who were unemployed prior to the prison spell. For these individuals, prison leads to a significant increase in job training programs and employment. In contrast, for those employed prior to prison, incarceration decreases employment, but does not impact recidivism.

What can explain the different results in the Norwegian and US contexts? One likely explanation, also highlighted by the authors, is the rehabilitative nature of Norwegian prisons. Prison conditions are generally of high quality, with high spending per prisoner, high staff to inmate ratios, open conditions, little overcrowding, single cells, and an emphasis on healthcare, education, and training. While the paper cannot explicitly open the black box of the Norwegian prison experience, it does suggest that at least for some individuals, this rehabilitative emphasis can make a difference.

Are these results specific to Norway? Huttunen et al. (2023) study

the impact of incarceration for defendants charged with financial crimes in Finland. Crimes studied primarily include fraud, business offenses, forgery, and money laundering. Fraud is by far the largest category, encompassing 60 percent of cases. These authors again find a reduction in recidivism behavior, and in fact also find that this effect spills over onto the colleagues of the incarcerated worker, suggesting deterrence may play a role.

In general, though not the first-order question of this report, the extent to which there are spill-over effects of incarceration is important to the big picture. In fact, judge designs are also regularly used to study the spill-over effects of prison onto family members. Some of these studies are listed in Panel D of Exhibit 4.2. In the Norwegian context, no spill-over effects are found of paternal incarceration onto the criminal behavior of the children (Bhuller et al., 2018a) but a reduction in the criminal behavior of younger brothers and criminal network members is seen (Bhuller et al., 2018b). Parental incarceration has even been seen to improve the crime outcomes of children and siblings in Ohio (Norris, Pecenco, and Weaver, 2021). But the one paper (Dobbie et al., 2019) that uses a judge design to study the spill-over effects of parental incarceration in Sweden finds detrimental effects on the children, including higher conviction rates, lower education outcomes, and worse adult employment. The results are driven by children from the most disadvantaged families. This paper also includes a brief analysis of incarceration on the impact of the behavior of the parent themselves and finds little impact of incarceration on future convictions.

THE EFFECT OF MORE TIME IN PRISON: INTENSIVE MARGIN

Finally, a handful of papers have considered the intensive margin question of whether more time in prison causally reduces recidivism. Though sentence length can be thought of potentially as one dimension of the black box, these studies still bundle all aspects of what happens in prison together. The strategies in these papers use either sentencing grid discontinuity designs, in which offenders in nearby cells of a grid (similar to that in Exhibit 4.1) both go to prison but with different prescribed lengths or sentencing reforms that exogenously impact the amount of time an offender spends in prison. Despite the different contexts—time served in two US states (North Carolina and Georgia) and in Sweden—the findings are similar. All three papers find

at least some evidence that more time in prison decreases recidivism.

Given its relevance, I provide some more details about the Swedish study that I conducted in collaboration with Matthew Lindquist. Specifically, in Hjalmarsson and Lindquist (2022), we take advantage of the two-thirds sentencing reforms that occurred in 1993 and 1999. These reforms did not change sentence lengths, but did change the amount of time an offender would serve in prison for a given sentence length: Specifically, the share of time served increased from one-half to two-thirds. Shorter sentences (less than 12 months) were fully treated by the first reform while longer sentences (more than 24 months) were treated by the latter; other sentences were partially treated by both reforms. The intuition of our research design is to compare the outcomes for individuals with the same sentence (and offense characteristics) but who serve different amounts of time due to the timing of their conviction (i.e., either before or after the reform). The main purpose of this paper was in fact not to study recidivism but rather whether more time in Swedish prisons impact health, and in particular, mortality. Perhaps surprisingly, and contrary to correlational studies, we find that more time in prison decreases mortality. These results were largely driven by reductions in the chance of suicide, especially for those with mental health problems, and in circulatory related deaths, especially for the somewhat older population. One take-away from this study is that it speaks to the high quality of Swedish prison conditions in the 1990s.

But what about recidivism? How did more time in Swedish prisons impact post-release criminal behavior? Though not the focus of this paper, we did consider the question. We find that exposure to the reform decreased the chance of returning to prison within two years by about 4 percent. Significant or marginally significant effects are also seen when looking at other measures of recidivism, like having at least one conviction or more than one conviction in the first one to three years post-release. These effects are large enough to be observed on average in the whole sample, but heterogeneity analyses reveal that they are in fact driven by selected subsamples. In particular, the effects on recidivism are driven by those more serious offenders, i.e., with past prison experience, older offenders, and/or no employment in the past. The latter finding is in fact consistent with the extensive margin Norwegian study, whose results are driven by those unemployed from prison. We find no significant effect (positive or negative) on recidivism

of more time in prison for those in prison for the first time.

This final finding—that more time in prison does not affect recidivism of first-time prison inmates—is also found in an article by Al Weswasi et al. (2023) that evaluates the 1993 and 1999 sentencing reforms, as well as a reform in 1983. Though sample sizes and empirical modelling choices are very different between the two studies, the spirit of the research design of Al Weswasi et al. (2023) is similar to that of Hjalmarsson and Lindquist (2022). Both studies compare recidivism outcomes for offenders exposed to different sentencing regimes given the timing of their conviction dates.[11]

SWEDISH PRISONS IN AN INTERNATIONAL PERSPECTIVE

The above studies find heterogeneous effects, which can be driven by differences in the prison conditions (i.e., the contents of the black box vary across context) and/or samples studied. The same treatment can have different effects on different types of offenders. In general, the Scandinavian studies find evidence that something works. This was true at the extensive margin of prison in Norway and, for financial offenses, in Finland. This was also true at the intensive margin in Sweden, at least in the short run for individuals with past incarceration histories. Detrimental spill-over effects of parental incarceration in Sweden are also seen, however, with no effect on the crime of the parent themselves. But, none of these papers really open the black box of prison. Why do some prison sentences reduce recidivism? It is not just that individuals are spending more time in prison, but we also must acknowledge that they are spending more time exposed to a specific prison experience.

Before turning to the literature that does attempt to open this black box, I conclude with some big-picture evidence that the black box of Scandinavian prisons differs from that in other countries. And that

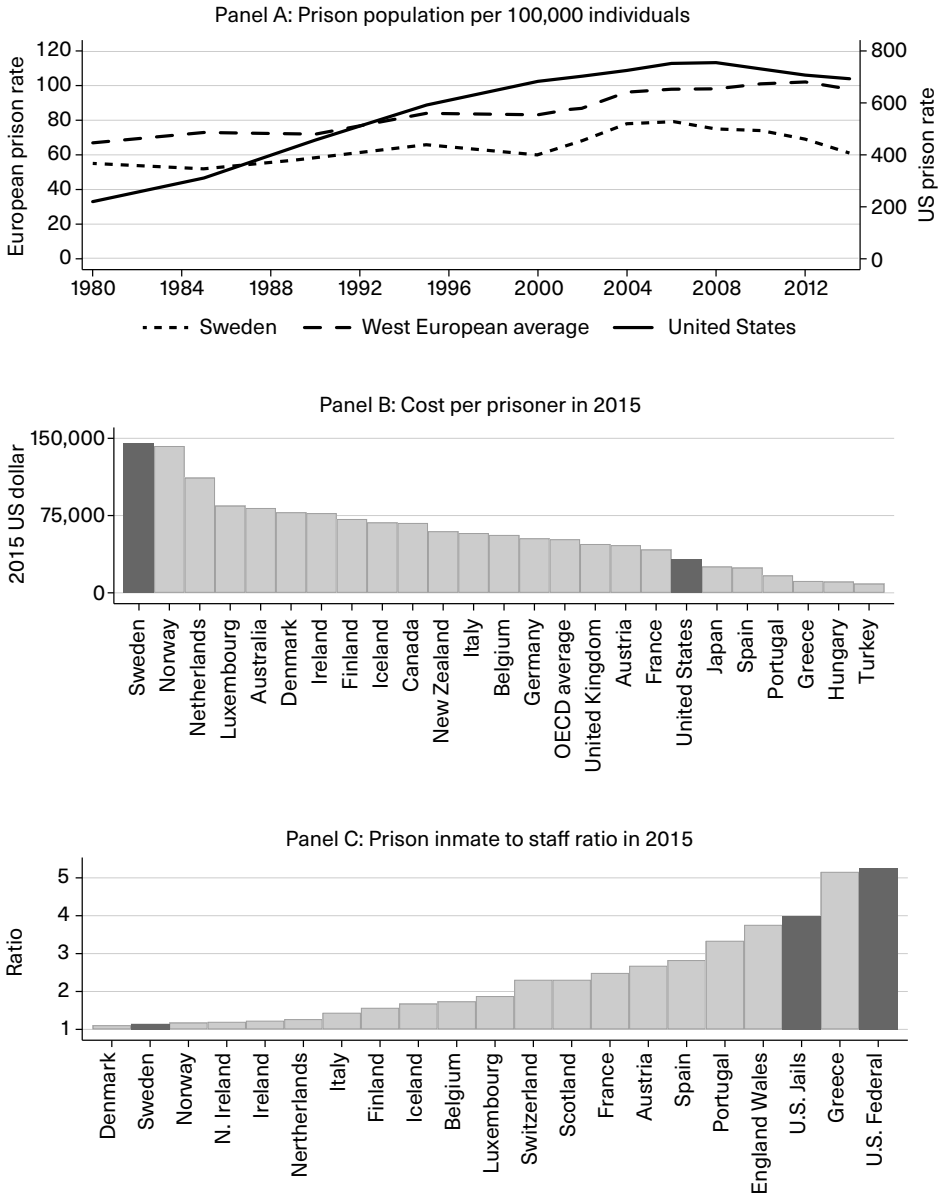
11. Another recent paper that studies the effect of more time in prison on reoffending after release is Stam et al. (2023). This paper uses a judge instrument design in the Netherlands for those who are sentenced to prison for sentences of less than a year. The results are consistent with the rest of the literature: more time in prison does not significantly impact the likelihood of recidivating but does decrease the number of post-release offenses. While this non-economics paper does adopt a research design regularly used by economists, some of the traditional tests of the validity of this design are not included in the paper, making it hard to evaluate.

Sweden is much more comparable to Norway than other countries.

Panel A of Exhibit 4.3 demonstrates that Sweden's incarceration rate has fluctuated between a high of 79 (in 2006) and a low of 53 (in 1985) inmates per 100,000 persons. This is about 25 percent lower than the average in Western Europe and 10 times lower than in the US. This alone suggests that the findings of US judge instrument designs may not generalize to Sweden. But Panels B and C make this case even stronger: Swedish and Norwegian criminal justice systems spend more per prisoner than anywhere else in the world, at least in 2015, and more than three times that in the United States. Consistent with this, the ratio of inmates to staff is the lowest in the world.

At the same time, it is important to acknowledge that cross-country comparisons of prison conditions are difficult to make. Prison spending estimates can include different line items in different countries. Cross-country prison spending can also differ because the prison populations differ, leading to inherently different budget needs.

Exhibit 4.3 Swedish Prisons in International Perspective.



Notes: This figure is sourced from Appendix Figure 1 of Hjalmarsson and Lindquist (2022). Panel A: The West European average is a country average and not a population weighted average. Source: Institute for Criminal Policy Research, UK Panel B: Institute for Public Affairs, Australia. Panel C: Source: SPACE-I, Council of Europe, Annual Penal Statistics (2018). Information on US Jails is from the Bureau of Justice Statistics (2018) and refers to the year 2016. Information on US Federal prisons is from the Bureau of Prisons (2012) and refers to the year 2011.

4.2 Peers in Prison

Social interactions play a prominent role in understanding criminal behavior. Individuals with a criminal record are more likely to have criminal peers. In the real world outside of prison, individuals generally "select" themselves into such peer groups—and sometimes formal gangs. In prison, however, all of one's peers are criminal, though they may vary in the nature and severity of their criminal characteristics (e.g., types of criminal offenses and networks). The characteristics of one's fellow inmates is one dimension of the black box of prison.

Understanding the extent to which one's criminal behavior is impacted by the criminal behavior of their peers—so-called endogenous peer effects in the economics literature—has important policy implications.[12] On the one hand, endogenous peer effects imply the presence of a social multiplier via which policies can impact a broader population than just those directly treated by the policy. With regards to criminal behavior, for instance, prison policies that reduce the recidivism of those in prison can actually have a further crime reducing effect by spilling over to other members of the criminal networks (not in prison). Drago and Galbiati (2012) find evidence of such endogenous peer effects in the Italian legal system.

But another policy implication is related to how one should assign inmates to prisons: are there optimal or non-optimal assignment policies? Do the peers to which one is exposed in prison impact post-release recidivism behavior? From the perspective of the empirical researcher, there are many challenges to disentangling correlated criminal behavior amongst peers from endogenous peer effects. For instance, individuals belonging to the same group—whether it is a street gang or a prison—can have the same criminal behavior because they were selected into the group (either by themselves in the case of a gang or the prison authorities in the case of prison). Alternatively, members of the same group can experience the same external shocks. Both of these scenarios can yield correlated criminal behavior even in the absence of any social interactions or peer effects. Besides these correlated unobservables, empirical researchers studying endogenous peer effects also

12. See Gavrilova and Puca (2021) for a survey of the peer effects in crime research. This includes papers studying the impacts of non-prison peers on crime.

face another problem—simultaneity. Social interactions are two-way streets—offender *i* can impact the behavior of offender *j* while offender *j* can impact the behavior of offender *i*. As Manski (1993) puts it, we need to disentangle the nearly simultaneous actions of individuals from the reflection of those actions seen in their peers.

I studied this question (years ago) in the first chapter of my PhD dissertation. Inspiration for this paper came from the movie *Blow*, which is about the introduction of crack cocaine to the United States. The following line is said during the movie: "Danbury wasn't a prison. It was a crime school. I went into prison with a bachelor in marijuana and came out with a doctorate of cocaine." This led me to ask whether there are peer effects and criminal learning in prison. How does criminal capital—and subsequent recidivism behavior—change with exposure to criminal peers in prison? Does it depend on the type of criminal peer? In Bayer, Hjalmarsson, and Pozen (2009), we solve the sorting and reflection problems described above to identify whether there are endogeneous peer effects with respect to crime. We study this in the context of juvenile correctional facilities in Florida, and take advantage of randomness in exactly who else is in prison during one's sentence. In other words, even though individuals are not randomly assigned to facilities (e.g., facilities are for instance in part determined by security level), the characteristics of who is there when you arrive, and how long you overlap with each inmate, is as good as random. We find evidence of what we call reinforcing peer effects. Individuals are more likely to recidivate with a specific type of crime, like drug offenses, if they are exposed to peers with drug offenses—but only if they have a criminal history of the offense themselves. Exposure to peers like themselves reinforces their pre-existing criminal behaviors. These findings are not limited to drug offenses, but also seen for burglary, petty larceny, aggravated assault, and sex offenses.

Nor are these results specific to juvenile facilities in Florida. In fact, the analysis was replicated in Damm and Gorinas (2020), who examined a population of 18–21-year-olds in Denmark. Damm and Gorinas find evidence of reinforcing peer effects for drug crimes, theft, burglary, and fencing but deterrence effects of being exposed to similar peers for violent offenses (including sex offenders). Using a similar research design, Stevenson (2017) finds evidence of a social contagion mechanism, in which the most influential peers come from unstable

homes and exhibit emotional and behavioral problems.

What are the policy implications? At face value, the results would suggest that prison authorities should not group like offenders together in the same facility. This is contradictory to what is often done, however, to facilitate the provision of treatment, such as for drug or sex offenses. If such sorting across facilities is necessary (e.g., to have sufficient treatment class sizes), then the next best option may be to increase supervision (to prevent the transfer of criminal capital) during incarceration.

4.3 Prison Conditions

The fact that peer characteristics impact recidivism begs the question of whether other dimensions of prison conditions matter. A recent but still small literature has begun to focus not on the recidivism effects of any prison sentence or more time in prison, but rather the nature of the prison experience.

Chen and Shapiro (2007) conducted one of the earliest studies of the impact of prison conditions on recidivism. Their research design essentially allows them to compare US federal inmates with similar characteristics but who are sent to different security level prisons because the score measuring their need for supervision falls just above or below a pre-determined threshold. Higher security prisons have worse prison conditions in many dimensions, including peers, freedom of movement, interaction with the community, rates of injury, and exposure to violence. Though the results are somewhat imprecise, the authors find that, if anything, harsher prison conditions lead to higher recidivism rates. Similar findings are found in another early study (Drago, Galbiati, and Vertova, 2011) of the effects of Italian prison conditions.

More recently, Tobón (2022) has studied the question in Colombia by taking advantage of a large prison construction program. Due to an overcrowding crisis, prison capacity was expanded by about 33 percent with the opening of ten new prisons around the country between 2010 and 2013. The new prisons were substantially less crowded, with more opportunities to enroll in rehabilitation programs and higher guard to inmate ratios. To identify the causal effect of prison conditions on recidivism, Tobón takes advantage of the fact that placement in the

newer prisons is in part determined by arbitrariness in the assignment process: is an inmate lucky enough to be at the point in the queue that gets assigned to the new prisons? The results point strongly towards placement in the new prisons decreasing recidivism.

But, prison conditions in each of these studies are still largely a black box. The next set of studies begins to open this box to look at the effects of more specific factors, including visitation, open versus closed prisons, and treatment and training programs.

PRISON VISITATION

There is substantial variation across prison systems, and even across different prisons in the same system, in the extent to which inmates are permitted to have visits. The theoretical effect of visitation rights on recidivism is ambiguous. On the one hand, visitation allows for the maintenance of social ties that can be important to successfully reintegrate into society. On the other hand, if these social ties are negative influences, then visitation can reinforce criminal relationships or increase access to drugs and contraband in prison. Lee (2019) studies the effect of prison visits on recidivism for inmates in the US state of Iowa. The author takes advantage of the fact that inmates placed in facilities closer to home will have more visitors. Thus, rather than simply comparing recidivism outcomes for inmates with many versus few visitors, the differences of which can be driven by the strength of their social networks to start with, Lee focuses on that variation in visitation driven by the location of the facility. The author does not find any evidence that visitation impacts recidivism one way or the other.

A similar question, with a similar approach, is studied in Denmark. Andersen, Fitzpatrick, and Wildeman (2024) use variation in the number of visitors driven by how far from home Danish prison inmates are placed. Though the rules say that inmates should be placed in the prison closest to home that is suitable, overcrowding and variation in occupancy rates imply that this prison is not always available. Though the authors do indeed find that distance from home impacts visitation, there is little evidence that visitation impacts a wide range of inmate outcomes, including recidivism.

Most recently, Otsu (2023) studies the effect of visitation on short-run recidivism in Missouri using a similar research design. In contrast to the rest of the literature, this paper finds that each additional visit in

prison significantly lowers the chance of recidivism. Why do results differ across studies? Otsu (2023) posits that this is driven by differences in sample (the Missouri sample is only men) and differences in the period during which recidivism is measured (the effect arises in the short-run for Otsu but Lee (2019) for instance looks at a 3-year measure).

OPEN PRISONS

Mastrobuoni and Terlizzese (2022) study the effects on recidivism of serving one's time in open versus closed prisons in Italy. Open prisons have better conditions in multiple dimensions—less supervision, more freedom to work, study and maintain personal relationships, and more emphasis on self-responsibility. Open prisons are based on the idea that rehabilitation will curb recidivism while closed prisons are based on the idea that harsher conditions will deter future crime. In prison systems with both open and closed prisons, assignment to open prisons is generally not random but selected on the basis of which inmates are best suited (i.e., the better behaved and/or less serious offenders). The authors use a natural experiment in the assignment of inmates to the Bollate open prison in Italy, which was built in 2000. Though some of the inmates in Bollate are selected, others are there due to displacement from nearby overcrowded prisons. This was especially the case when the prison first opened since it had a lot of space to receive these overflow inmates: neither Bollate nor the sending prison could choose which inmates to send to Bollate nor when they were sent. The authors find that recidivism rates decrease as the share of time an inmate spends in the open prison increases. Better prison conditions once again appear to decrease recidivism. The authors also find suggestive evidence that effects are largest for those who are least educated and that opportunities to work outside of prison while incarcerated play a role but are not the only relevant factor.

PRISON REHABILITATION PROGRAMS

There is a vast array of prison programming, including education, job skill training, psychological support, and addiction and substance abuse treatment. Assignment to programs is generally based on need and/or voluntary participation: from the perspective of identifying the causal effect of a program, selection is once again a problem. Another challenge in such evaluations is that within prisons, the control

groups are also often treated: those not participating in one program may participate in another. The quasi-experimental literature on the impacts of such prison programming is indeed quite small. I highlight a few very recent papers on this topic, which have begun to recognize the knowledge gap and challenges in studying these questions.

Arbour, Lacroix, and Marchand (2024) study a number of programs provided in Quebec (Canada) prisons for offenders with sentences of two years or less. To deal with selection, the authors take advantage of the fact that many programs are administered by local school boards and not usually available during summer or December holidays. The availability of other programs depends on variations in prison personnel and financial resources. In other words, the authors do not just study the relationship between program participation and recidivism, but rather focus on that variation in program participation that is (exogenously) driven by variation in what programs are available when the offender is incarcerated. The authors find that a higher availability of programs is associated with a lower propensity to be re-incarcerated but that there is actually an increase in reoffending with more minor crimes. In terms of which programs matter, the authors point towards the effectiveness of those that focus on self-development, violence, education, and employment, but find null effects associated with addiction programs and those related to arts, spirituality, and sports.

A new working paper in the US by Alsan et al. (2024) also finds evidence that educational programs (in US jails) can decrease recidivism. The authors study the program IGNITE (Inmate Growth Naturally and Intentionally Through Education) in the county jail of Flint, Michigan. Though the amount of time an inmate is in jail is not random, there is some underlying random variation driven by quasi-random court delays. The authors essentially compare outcomes for inmates who spend the same amount of time in jail before and after the IGNITE program was introduced. They find one additional month of IGNITE exposure reduces misconduct in jail by almost 50 percent and recidivism rates by 18 percent in the first three months post release; the recidivism effects get larger over time.

In the same Canadian context as above, Arbour (2022) studies the effect of cognitive behavioral therapy (CBT) participation for incarcerated adults in Quebec prisons. Participation in such programs is in part determined by a recommendation of prison evaluators, who have

different propensities to make such recommendations (in much the same way as judges have different propensities to recommend prison sentences). Arbour finds that participation in the program reduces recidivism rates by 50 percent in the first year, and that these effects persist into the future, especially for first-time offenders. The author also finds that CBT is more effective if provided close to the time of release, rather than at the beginning of a relatively long sentence. This finding—on the effectiveness of CBT in adult prisons—adds to a growing literature finding causal evidence that CBT reduces crime and violence for other crime-prone (but not incarcerated) populations. One prominent paper is that by Heller et al. (2017) studying the provision of CBT, and other behavioral interventions, to at-risk youths in Chicago. Blattman, Jamison, and Sheridan (2017) have also found that CBT, in combination with a cash transfer, is effective in reducing antisocial behavior of adult men in Liberia.

Finally, Cook et al. (2015) use a randomized control trial (with 236 participants) to evaluate a re-entry program for violent offenders in Wisconsin correctional facilities. This program "reached in" to provide intensive programming (aimed at staying off drugs, away from gangs, and preparation for legitimate jobs) in the six months before release. But these were not the only treatments. Rather, the reach-in programming was in conjunction with eligibility for six months post release for subsidized employment and assistance in transitioning to unsubsidized employment. Those in the treatment group did earn more than the control group, but the evidence on recidivism is mixed: the chances of re-arrest are lower but effects on re-incarceration are not significantly different than zero.

PRISON HEALTHCARE

Another dimension of the black box of prison conditions is prison healthcare. Though there is research demonstrating that access to healthcare outside of prison decreases crime, there is little knowledge on the causal impacts of healthcare in prison. Given how negatively selected the criminal justice population is with respect to their health and how disconnected many of these individuals are from the public healthcare system, this knowledge gap is surprising.

As a first step to study the question of whether healthcare in prison reduces recidivism, Matthew Lindquist and I have begun to study the

impact of diagnosing ADHD (Attention Deficit and Hyperactivity Disorder) in Swedish prisons, with an emphasis on new diagnoses. This is recent and still unpublished work, and the beginning of a broad agenda on the public health and safety impacts of prison healthcare. We focus first on ADHD-related care in prison for a few reasons. One is that ADHD is especially prevalent amongst criminal justice populations worldwide: Young et al. (2015) estimate that 25 percent of adult prisoners have ADHD. ADHD is similarly over-represented amongst Swedish offenders: for instance, amongst the cohort born in 1995, we see that less than 6 percent of males have been medicated (by 2016) for ADHD. But, in this cohort, this statistic is 14 percent for those with a conviction but no prison sentence and 23 percent for those with a prison record (Hjalmarsson and Lindquist, 2023). In our sample of Swedish prison spells of 36 months or less from 2009 to 2013, an ADHD diagnosis was in fact the second most common diagnosis in prison. The second reason to focus on ADHD is that it originates in childhood—as such, it should not be caused by the prison experience itself making it easier to infer causality from our analysis.

Using an event study design that traces out both the take-up of ADHD medication and diagnoses from the public healthcare system in the months leading up to and following a prison spell, we find that there are sharp increases in the take-up of ADHD medication. At the same time, we observe a reduction in post-release conviction rates. Does the treatment of ADHD in prison cause both the increase in ADHD related care and reduction in crime? When comparing how these changes in behavior differ across groups that are treated with new diagnoses of ADHD in prison and not, we reach the following conclusions:

- New ADHD diagnoses in Swedish prisons increase the take-up of ADHD related care post-release. This is in fact not only true for the inmates, but also for their family members (children and siblings). Thus, this suggests that healthcare in prison may be a vehicle to bring this vulnerable population into contact with the public healthcare system.
- But, we do *not* find strong evidence that these new diagnoses of ADHD in prison translate into reduced recidivism behavior. There is some reduction, especially in the short term, of alcohol and drug offenses, but there is no overall reduction in re-

conviction rates. Thus, ADHD-related care may not be the crime prevention tool that many policy makers hope.

SUMMARY: PRISON CONDITIONS

The above studies generally find that harsher prison conditions do not deter future crime, and if anything, may actually lead to more post-release recidivism. Prison conditions do matter. But, prison conditions encompass many dimensions. This is where the frontier of the quasi-experimental research is right now: which prison conditions matter, and which do not? The above-cited papers provide some evidence of the following conclusions:

- Section 4.2 highlighted that *peer characteristics* do matter.
- *Open prisons* may decrease recidivism, and employment while incarcerated may be important.
- Some *prison programming* matters, especially programs targeting violence, education, and employment.
- *Cognitive behavioral therapy*, especially for first time offenders and when provided near the time of prison release, has been shown to decrease recidivism.
- Contrary to expectations, most research does not find that *visitation in prison* impacts recidivism.
- Some *prison programming* has not been seen to reduce recidivism, including addiction, arts, spirituality, and sports programs.
- Though ADHD may be over-represented in the criminal justice population and under-treated, there is little evidence that the provision of new *diagnoses of ADHD in prison* decreases post-release recidivism.

A number of caveats must be made regarding these conclusions, however. First, I do not claim that this is an exhaustive list of studies, and there may indeed be other evaluations in other contexts or for different samples that do not find the same effects. For instance, effects of prison programs—even the same prison programs—can differ depending on the participating population, the counterfactual (i.e., who is in the control group), and even who is providing the program.

Second, and with regards to the generalizability to Sweden, many of these studies are conducted in criminal justice systems—like the US, Italy, and Colombia—where prison conditions on average and spend-

ing per prisoner are much lower than that in Sweden. It is not necessarily the case that harsher prison conditions in Sweden would have the same effect. That said, some of the above studies are conducted in a Scandinavian or Swedish context—namely that looking at the effect of visitation (Denmark) and in prison ADHD diagnoses (Sweden).

THE SWEDISH PRISON EXPERIENCE

I have pointed out many times that prison conditions in Sweden have historically been quite high. What does this mean? I conclude this section with some descriptive evidence on the nature of the Swedish prison experience. These statistics are based on both annual reports provided by Kriminalvården and my own analyses of Swedish prison registers for my work on prison healthcare with Matthew Lindquist. (See Hjalmarsson and Lindquist, 2022 and Hjalmarsson and Lindquist, 2023.)

One aspect of the prison experience is how prisoners spend their time. According to a recent Kriminalvården report, 35 percent of one's time is engaged in paid work (*arbetsdrift*), which may include activities like working in the wood or metal shop or a laundry room. 27 percent of time is spent in other structured activities, like skills training, parenting courses, study circles, and healthcare activities. 23 percent of time is in what the prison labels service activities, which is paid work in service functions within the prison. 11 percent is in adult education. Though treatment rates are high in Swedish prisons, just 2 percent of time is engaged in treatment programs, such as substance abuse or behavioral therapy programs. We know little to date about how these various activities and the intensity in which inmates engage in them impact recidivism. However, evaluations of these activities are definitely of interest and even underway (especially of employment) by the Swedish prison authorities.[13]

Our study of ADHD care in Swedish prisons described above (Hjalmarsson and Lindquist, 2023) focused on sentences of less than 36 months from 2009 to 2013. In this sample, we saw that 41 percent of inmates began at least one treatment program, and 13 percent began a

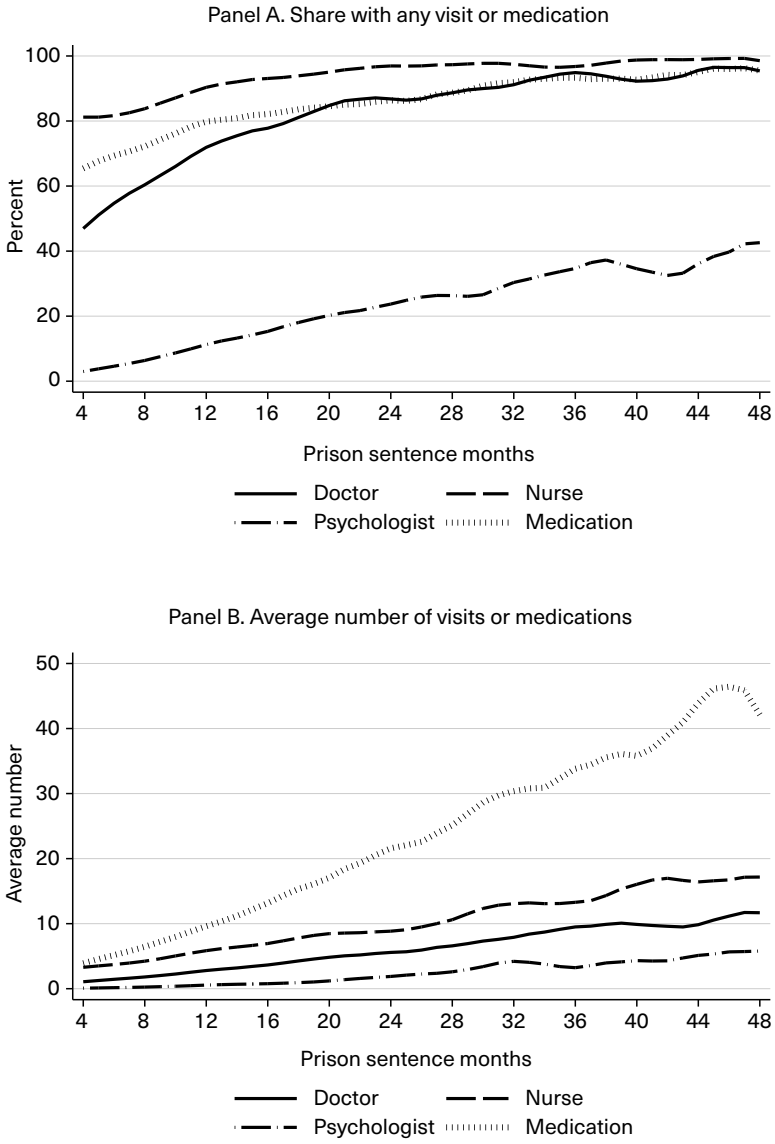
13. These statistics are sourced from Figure 2.3 of the annual Kriminalvård och statistik report. See https://www.kriminalvarden.se/globalassets/forskning_statistik/kos-2022-kriminalvard-och-statistik.pdf.

program in which the Kriminalvården description included cognitive behavioral therapy. The largest programs, however, are not CBT. 16 percent of inmates participated in Motivational Interview, 12 percent participated in Prime for Life (which was discontinued in 2014), and 10 percent and 5 percent participated in the 12-Step A and B programs, respectively. All of these programs are related to alcohol and substance abuse.

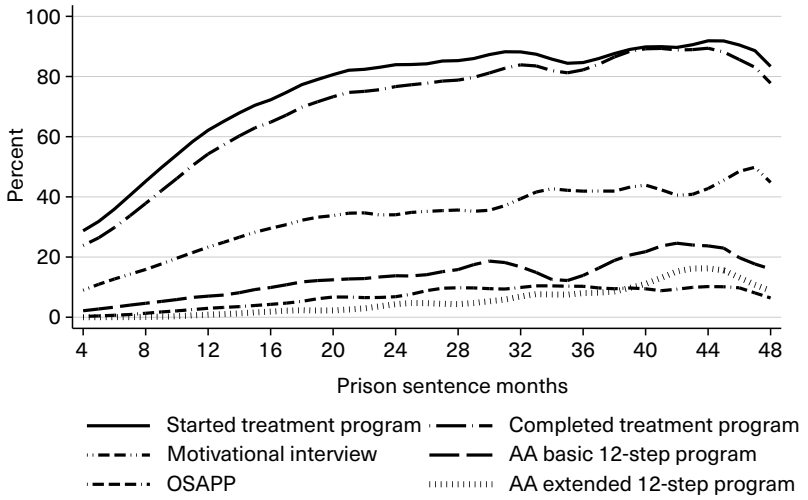
This sample can also be used to see the high intensity of healthcare provided in Swedish prisons. 32 percent of prison spells include at least one diagnosis and 62 percent include at least one prescription. 51 percent of inmates have at least one visit with a doctor, 83 percent with a nurse, and 6 percent with a psychologist. 7 percent receive a hepatitis B vaccine. Of course, the prison population (and elsewhere) is very negatively selected with respect to their health, so the provision of prison healthcare is in part a function of the needs of the population. While the provision of healthcare should not necessarily be motivated by its impact on recidivism, understanding whether there are such benefits is an important research question, and one to which we aim to contribute future knowledge.

Finally, I highlight that each of the sections above on prison conditions are not unrelated to each other. More time in prison implies more time in prison with certain conditions, and potentially more opportunities to receive care and treatment. Of course, there is still an issue of selection, since inmates with longer sentences are more negatively selected in terms of their needs and potential for recidivism. The latter is particularly relevant in the Swedish context, as Kriminalvården prioritizes treatment for those assessed to have relatively higher recidivism risks. In Hjalmarsson and Lindquist (2022), for sentences of 4 to 48 months from 2009 to 2013, we show that more time in prison is associated with more program participation and more medical visits. These statistics are displayed in Exhibit 4.4.

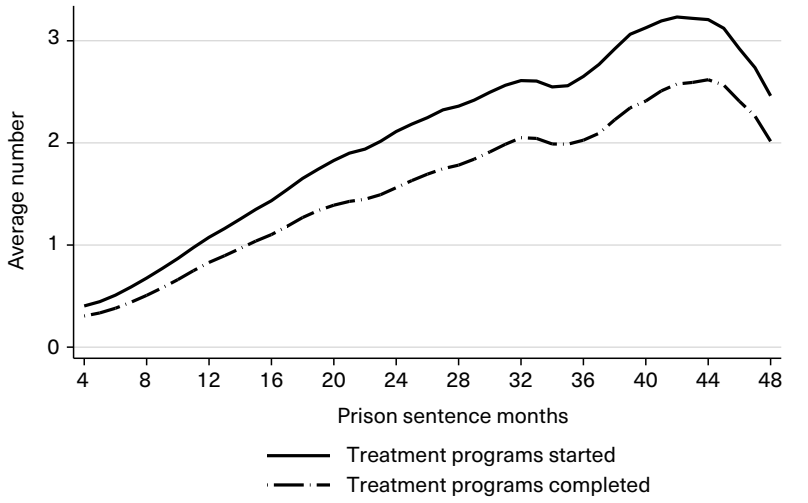
Exhibit 4.4 Increasing Treatment with More Time in Prison.



Panel C. Share who started/completed at least one treatment program



Panel D. Average number of treatment programs started/completed



Note: These figures are directly excerpted from Hjalmarsson and Lindquist (2022). The healthcare panels (Panels A and B) correspond to Figure 8 while program participation panels (Panels C and D) correspond to Figure 9 from Hjalmarsson and Lindquist (2022). AA stands for Addicts Anonymous and OSAPP for the Offender Substance Abuse Pre-Release Program.

5. The Impact of the Post-Release Environment on Recidivism

Becker's economic model of crime (1968) theorizes that criminal behavior is not just impacted by the probability and severity of punishment but also by legitimate sources of income. This chapter reviews the relatively small literature that studies the effects of two such income sources—labor markets and public assistance—on recidivism.[14]

5.1 The Effects of Labor Markets on Recidivism

EXISTING RESEARCH

While Sweden and other Scandinavian countries provide relatively high-quality prison conditions and have ambitious rehabilitation aims, ex-offenders (with or without prison records) may not be able to successfully desist from crime if they are unable to get a job or one that pays sufficiently high wages. Given a growing body of research documenting that workers with criminal records face significant labor market barriers, including the unwillingness of firms to hire them, this is of real concern. Raphael (2014) highlights the many reasons why firms may prefer workers without records. At one extreme are federal/state regulations that ban such individuals from certain jobs—especially those that entail working with vulnerable populations (like children) or sensitive information. But, even without explicit regulations, risk

14. There are, of course, other dimensions of the post-release environment that can affect recidivism. These would include, for instance, access to housing and healthcare as well as the individuals with whom one interacts (i.e., peer effects). The scope of this report does not allow for a review of all such post-release environmental factors.

averse firms may prefer not to hire workers with criminal records given the associated risks of loss due to employee theft or dishonesty or liability risks due to a harmed customer or employee. Alternatively, firms may simply be morally averse to hiring workers with criminal records. In contrast to these examples of taste-based discrimination, firms may also be statistically discriminating and using a criminal record as a signal of low productivity.

There is indeed significant empirical evidence—albeit primarily in the United States—that firms prefer not to hire workers with criminal records. A 2003 survey of California employers found that just 2 percent would definitely hire a worker with a criminal record while 37 percent said they would definitely not (Raphael, 2014). A more recent survey by Cullen, Dobbie, and Hoffman (2022) found that just 39 percent of US firms hiring temporary workers were willing to hire those with a criminal record. Similar preferences are seen in studies using experimental variation. For instance, Pager (2003) conducts an audit study in Milwaukee, Wisconsin, in which there are two pairs of auditors (one Black pair and one white) that apply in person to jobs advertised by 350 employers. Both members of the pair apply to the same employer, but one member is assigned to have a criminal record. The results are striking: For white and Black individuals, the chance of a callback for an interview decreased by 50 percent and 60 percent, respectively, when the auditor had a criminal record. In a correspondence study design (which allows for thousands of job applications and holds constant all else but the criminal record), Agan and Starr (2017) show that employers in New York and New Jersey, whose job ads included a box asking about criminal history, were more than 60 percent more likely to interview applicants without a felony conviction. The Cullen, Dobbie, and Hoffman (2022) study also solicits a firm's willingness to hire workers with criminal records under different scenarios that shed light on the source of a firm's preferences. A firm's willingness to hire workers with criminal records varies with the nature of the job—it is higher if there is no customer interaction, high-value inventory, or if the job is hard to fill. Moreover, firms are significantly more likely to be willing to hire such workers when provided (potentially prohibitively) large wage subsidies, relatively small amounts (up to $5,000) of insurance coverage, information on satisfactory previous job experience, and a clean record for at least one year.

These findings and barriers to employment are important since a small but convincing body of research demonstrates that labor market conditions and opportunities impact an ex-offender's propensity to recidivate. Much of this research highlights the conditions that exist upon one's release from prison. Before reviewing this literature, I emphasize that this finding is not obvious a priori. Offenders are typically negatively selected in many dimensions—human capital, ability, health, and mental health—that can also lead to low employment outcomes, regardless of the current labor market conditions or having a criminal record. Employment rates are in general low for this population—even before they have a criminal record.

Yang (2017a) demonstrates that recidivism decreases in response to higher wages for low-skilled jobs at the time and location of an offender's release from prison. Recidivism is defined as returning to prison within a range of time intervals. Using data from the US National Corrections Reporting Program, Yang studies the effect of average wages at the time of release for about four million offenders released between 2000 and 2013. The impact of the Great Recession on employment and wages, which occurred during this period, varied significantly across industries and/or localities. As such, there is substantial variation in the labor market conditions over time and across the more than 2,000 counties to which these ex-offenders are released. Yang's estimates suggest that the typical wage growth during a business cycle (i.e., about 5 percent) decreases the risk of recidivism by 2–4 percent. Moreover, she finds similar effects across crime types (violent, property, or drugs). Yang uses average local wages as a proxy for local labor market conditions in general—employment and wages. Two other studies look more directly at the effect of employment.

Galbiati, Ouss, and Philippe (2021) is one of the few labor market and recidivism papers conducted outside of the United States. Using individual level data on former inmates in France, Galbiati, Ouss, and Philippe study the effect of daily variations in labor market conditions at the time of release on recidivism (in this case—re-incarceration within six months of release). The authors measure labor market conditions upon release using the number of job vacancies published within 30 days of release and in the former inmate's county of residence on the French governmental agency for employment website. The authors do not find an effect on recidivism on the overall quantity of job ads

but do find an effect of having more manufacturing opportunities—an industry in which workers with criminal records tend to be more regularly represented. Moreover, the authors consider the possibility that it is not just the existence of jobs that matters, but also the flow of information about jobs. They measure information flow using media coverage of job creation: holding the number of jobs constant, former inmates are less likely to recidivate when there is more media coverage of available jobs.

Schnepel (2018) studies the recidivism behavior of 1.7 million offenders released from California prisons between 1993 and 2008. The number of jobs in construction and manufacturing at the time and location of release is again associated with reductions in recidivism. In this case, jobs are not measured with job advertisements but actually with administrative data on the number of quarterly hires (overall, by skill, and by industry). Schnepel's work also suggests that it is not just any low-skilled job opportunity that matters, but potentially job opportunities that are "good enough" to incentivize one to stay away from crime. Schnepel does not find an impact of jobs in food service and retail, which had an expected salary that was about half that of jobs in the construction sector.

The final paper in this section, Agan and Makowsky (2023), studies the effect of labor market wage policies—the minimum wage—on recidivism. Changes in the minimum wage can affect the recidivism of newly released prisoners by impacting expected earnings but also by changing the supply of jobs and chance of employment. Agan and Makowsky use the same data source as Yang (2017a)—the US National Corrections Reporting Program—for the period 2000 to 2014. This includes about four million offenders released from prisons in 43 US states. Recidivism is defined as re-incarceration within one or three years. Over this fourteen-year period, there is variation across states (and within states over time) in the minimum wage laws. Utilizing this variation (and controlling for other state specific differences and trends), the authors find that an increase of the minimum wage by 50 cents reduces the 3-year recidivism rate by about 2 percent, with the results primarily driven by property and drug offenses.

In summary, the existing literature finds evidence that recidivism responds to higher wages, manufacturing and construction job opportunities, and information about job opportunities. Most of this work

is to date conducted in the US context, including our knowledge on the extent to which workers with criminal records are "barred" from the labor market.

THE SWEDISH CONTEXT: WORKERS WITH CRIMINAL RECORDS

Do employment opportunities impact recidivism in Sweden? Are there barriers to employment for workers with criminal records in Sweden? It is not obvious that the answers to these questions are the same—or of the same order of magnitude—in Sweden as in the US. More generally, does the disparate treatment of workers with criminal records seen in the US depend on the norms and institutional constructs specific to the US?

There are many fundamental differences between Sweden and the US that are relevant to the labor market outcomes of workers with criminal records. First, records in Sweden are automatically "cleared"—typically within 3, 5, or 10 years—depending on one's sentence and age at the time of offense. This is rarely the case in the United States, where clean slate policies for selected offenses are up for debate in many states. If criminal records are not as visible, can they still affect whether firms are willing to hire workers with a record? There is to date not much research on this question.[15]

Another difference between the Swedish and US context is the extent to which criminal records are publicly available and the prevalence of background checks. In the US, criminal records are easily searched (online or via requests to police departments) and background checks are the norm. Criminal background checks in Sweden are not as common, but they have become more prominent over time. For instance, the police conducted about 200,000 background checks in 2007 and nearly one million in 2022.[16] This rise was in part driven by the introduction and expansion of mandatory background checks in certain industries, providing shocks to regulations and firm information.

15. One recent paper (Agan et al., 2023) studies the effect of reducing felonies to misdemeanors in a California county—i.e., not record removal but rather record reduction. Employment benefits do not increase for individuals whose record was reduced (without them requesting this action). The authors also find evidence that this disappointing finding is not due to the individuals being unaware of the record reduction.
16. These statistics are based on data provided to the author by Polismyndigheten/Rättsavdelningen on June 7, 2023.

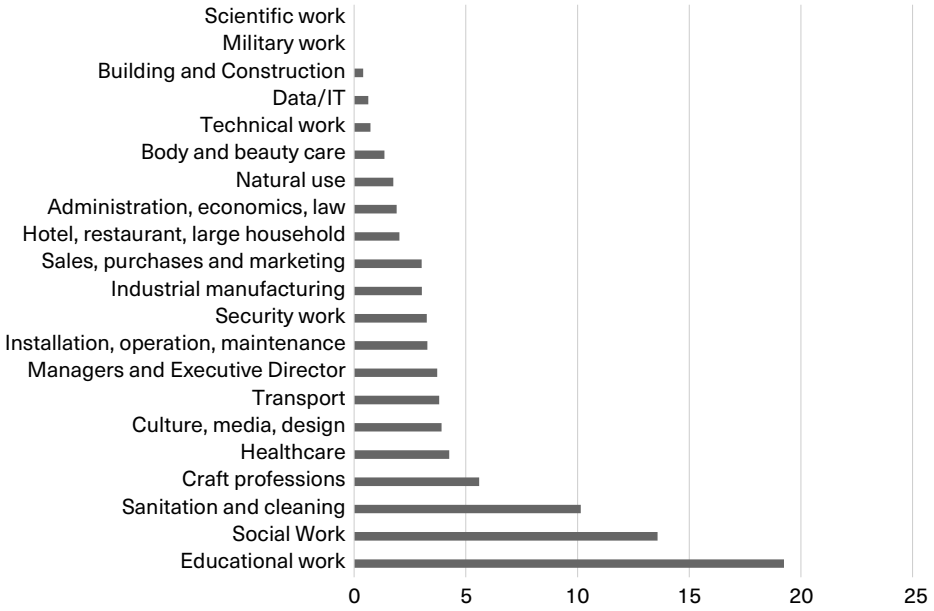
Even when mandatory, however, firms in Sweden are not supposed to ask for the background check until the end of the recruiting process (this allows workers with criminal records the opportunity to get in the door); moreover, the background check consists of an extract obtained from the police (*belastningsregistret*) by the job applicant themselves—not the employer. The ability of firms to conduct their own background checks has begun to change in recent years, with the emergence of online companies, like Lexbase (a private background check company in Sweden that started in 2014).

One way to get a sense of the prevalence of background checks in Swedish recruiting, and how this varies across industries, is to look at job ads. Exhibit 5.1 displays some crude statistics based on all job ads posted in the 30 days before May 14, 2023 on Platsbanken (the Swedish Public Employment Service Website). Specifically, within each industry, it shows the share of ads that include the term *belastningsregistret*, which generally indicates that a background check will be conducted during the recruiting process. The largest share indicating such checks is in education work (one of the fields where checks is mandatory). There is a lot of variation also within industry category. For instance, less than 5 percent of all sales, purchases, and marketing jobs indicate a background check will be conducted, while almost 30 percent of ads for a cashier, 15 percent for tour guides, and 10 percent for bank officials, customer service staff, and gas station employees. In other words, as in the US surveys, Swedish firms appear to be signaling they are less willing to hire workers with criminal records for jobs with high customer interaction and a risk of theft.

Given the evidence on the effects of employment opportunities on recidivism, I believe that more research is needed to understand the extent to which workers with criminal records in Sweden face barriers to labor market entry and the source of those barriers. Are firms unwilling to hire them? Why? Or are there other barriers—like transportation constraints that decrease the number of job options or weak informal networks that make it harder to find jobs via referrals? Understanding the source of such barriers is the first step to eliminating them. Though incomplete at the time of this report, this is a research agenda in which I am currently actively engaged.[17]

17. Understanding the firm's demand for workers with criminal records is part of the

Exhibit 5.1. Share of Platsbanken Ads with Keyword "belastningsregistret."



Note: Figure created by the author based on statistics collected from the Platsbanken website on May 14, 2023.

5.2 The Effects of Public Assistance on Recidivism

EXISTING RESEARCH

Given the challenges workers with criminal records face in obtaining jobs and the generally low employment rates for these populations, legitimate sources of income from non-employment sources, such as public assistance, may play a role in one's ability to desist from crime. This would especially be the case if the reason employment impacts recidivism is via an income channel (rather than an incapacitation chan-

goal of my European Research Council 2022 Advanced Grant.

nel in which working individuals are too busy to commit crime). Public assistance can be in the form of cash or in-kind transfers (the most common of which is food stamps). A small body of work—again mostly in the United States—uses quasi-experimental research designs to study the effects of a wide range of public assistance programs on recidivism.

One of the earliest studies was Berk and Rauma's (1983) evaluation of unemployment insurance in the US state of California. In 1978, unemployment insurance was extended to cover ex-offenders who had been incarcerated in state prisons: in other words, the work done while in prison was declared eligible (as long as it reached the threshold of 652 hours at minimum wage over a 12-month period) for unemployment insurance upon release. To study the effect of unemployment insurance on recidivism (within one year of release) the authors employ a regression discontinuity design, in which they compare ex-offenders who applied and qualified for insurance to those who applied but did not qualify because they had not worked sufficient hours while in prison. By controlling for the underlying relationship between the amount of work in prison and recidivism behavior, the authors can essentially compare those just lucky enough to be above the threshold to those just below. They do find suggestive evidence that access to unemployment insurance reduces recidivism. The robustness of these findings and their relevance to today is unclear given mixed findings in a number of other studies cited by Doleac (2023).

The next two studies consider the effects of welfare benefits in the US on recidivism. Yang (2017b) studies the effects of banning offenders from access to welfare benefits. Such an action was indeed taken in the US federal welfare reform act of 1996, in which offenders convicted of felony drug offenses were banned for life from welfare and food stamps. States, however, were given the option of opting out of this ban—and 30 states eventually did so. Yang (2017b) takes advantage of these state opt outs to compare how recidivism behavior (returning to prison within one year) differs for drug offenders released at a time when bans were in place to those released in non-ban periods. Since other factors (that impact crime) could also differ across periods, Yang (2017b) uses non-drug offenders as a control group. She finds that welfare and food stamp eligibility upon release reduces the risk of returning to prison by up to 10 percent.

Tuttle (2019) takes a different approach to study the 1996 welfare re-

form. Florida modified the ban to only include drug traffickers (i.e., not all felony drug offenders) who committed their offense after August 22, 1996. In other words, individuals who committed crimes before the ban would not lose their lifetime food stamp benefits, while the new law would be immediately applied to post-ban offenders. Though the ban also includes welfare benefits (as in Yang, 2017b), more than 85 percent of drug traffickers are male versus just 10 percent of the welfare users: the food stamp benefit is the more binding ban for this sample of offenders. By essentially comparing offenders who just miss the cutoff to those who just make it, Tuttle (2019) finds that banning food stamps increases recidivism amongst drug traffickers.

Finally, Munyo and Rossi (2015) study the effect of cash transfers provided to prisoners upon their release from prisons in Montevideo, Uruguay. The motivation for this study is the spike in recidivism on the first day of release—what the authors call "first day recidivism." The authors study the effect of an increase in the size of first day cash transfer: it increased from 30 to 100 Uruguayan dollars in September 2010. Does first-day recidivism change in the days around the implementation of the reform? The authors find that the answer is yes: increasing the gratuity provided to newly released prisoners all but eliminated the first day recidivism effect (i.e., recidivism rates on the first day post release were now no different than those on other days).

The bottom line is that the evidence to date does suggest that access to public benefits—both cash and in kind—reduces recidivism rates.

THE SWEDISH CONTEXT: PUBLIC ASSISTANCE FOR EX-OFFENDERS

To the best of my knowledge and in contrast to the US context, however, whether one is a convicted offender does not impact eligibility for Swedish public benefits and welfare. Eligibility for benefits, however, is not equivalent to access and take-up of benefits. Does everyone who needs post-release support receive it? I do not have statistics that speak to that. But there is at least an infrastructure in place (and system of post-release assistance and contact persons) that should help recently released inmates reach the relevant government agencies and help with financial planning. Once again, unfortunately, I do not know the extent to which these services are used and whether there is room for improvement. Inmates in Sweden may also have small amounts of

funds available to them upon release that they earned while working in prison; employment in "work" in prison is paid an hourly salary of 13 kronor (or a somewhat higher rate if you work more than eight hours in a day).

6. Conclusions

Given that repeat offenders commit a disproportionate share of crimes, policy makers wishing to reduce crime rates must understand the determinants of recidivism and factors that can deter or exacerbate it. The conclusion to this report proceeds in four parts. First, I summarize those factors identified in the report that do and do not causally impact recidivism. Second, I acknowledge that this report is likely far from comprehensive. There are many other potential factors that impact recidivism, which are either beyond the scope of the report or for which the scientific evidence base is minimal. Third, I highlight that though the impact of a factor on recidivism is of first-order importance for evidence-based policy, there are other dimensions that policy makers must fundamentally consider. These include both the costs of policies and other channels through which these factors can impact aggregate crime rates, such as general deterrence, incapacitation, and spill-over effects onto untreated individuals. Finally, I conclude with a discussion of the potential implications of the report's findings to some of the recently implemented or currently on the table Swedish criminal justice reforms.

Summary: What Have We Learned about the Determinants of Recidivism?

This report has presented convincing evidence that the following factors deter (and at least do not increase) recidivism:

- DNA databases that increase the probability of getting caught upon committing future crimes (in both the US and Denmark)

- Diversion for minor offenders (in the US)
- Electronic monitoring (in many countries, including Sweden)
- More time in prison (in the US and Sweden; no effect on or decreases recidivism)
- Better prison conditions
- Some prison programming—especially programs for violence, education, and employment
- Cognitive behavioral therapy, especially for first time offenders and near the time of release
- Higher wages for low-skilled jobs
- Local construction and manufacturing job opportunities
- Media coverage of job opportunities
- Access to public benefits.

For some factors, the evidence is mixed. Recidivism decreases in some analyses while it increases in others. These analyses may vary in the context or country studied, the research design used, or the subsample of offenders studied.

- Juvenile incarceration (mostly US based work)
- Adult incarceration (+ in the US, - in Norway)
- Prison peer characteristics.

Other factors have been discussed for which no effect on recidivism is seen:

- Visitation in prison (mostly no effect, one study with negative effect)
- ADHD diagnoses in prison.

What Else May Impact Recidivism?

There are of course many other factors that may causally impact recidivism, which have not been discussed in this report. For instance, one research area that is beginning to grow is concerned with whether there are deterrent effects of sanctions other than prison, including fines, community service, and probation. This is a young literature, but there is a recent work in the Swedish context that is worth discussing. Ahrsjö (2024) studies the effect on recidivism of a 2007 reform in Sweden that increased the use of community service for youths. Her findings

demonstrate that the effect of the reform depends on what a particular youth's alternative sentence would have been. Recidivism decreases for youths who get community service instead of fines but increases for youths who get community service instead of rehabilitation in a residential care facility.[18]

When it comes to detention, this SNS report discussed the deterrent effects of two types: incarceration and electronic monitoring. But, another type of detention that is increasingly getting attention is pre-trial detention. Does being held in a jail (in the US) or *häkte* (in Sweden) while awaiting trial increase recidivism? This could in theory have an effect for both those who are ultimately convicted, since jail conditions are often worse than prison conditions, but also those who are not convicted, who have been "punished" despite being found not guilty. Generally speaking, much of the research that exists finds that pre-trial detention increases post-release reoffending (Loeffler and Nagin, 2022).

Finally, this report highlighted just two features of the post-prison environment: employment/earnings opportunities and public benefits. But other aspects of the environment may also be relevant to recently released inmates, including, for instance, access to healthcare and housing. See, for instance, my earlier SNS report (Hjalmarsson, 2022) on the role of social policy as a crime control channel for a survey of the literature demonstrating the crime reducing effects of healthcare.

Beyond the First-Order Impacts on Recidivism

This report reviews recent scientific evidence of the impact of various treatments and policies on the recidivism behavior of the targeted population—the offender. The existence of such causal effects is clearly a first-order condition for successful evidence-based crime control policy. But, it is just part of the story.

Researchers and policy makers should also be asking whether there are unintended effects on the targeted and/or non-targeted populations. In other words, evaluating the impact of a particular sanction (or

18. A Danish study considers the impact of introducing community service as an alternative to prison and finds mixed effects depending on the offense type (Andersen, 2015).

public benefit) on recidivism does not tell us the full costs or benefits of the policy. Let us take the example of incarceration. Incarceration does not just impact the criminal behavior of the offender, but could also impact their physical and mental health and labor market outcomes. Moreover, incarceration does not just impact the incarcerated individual—there can also be spill-over effects onto the behavior, including crime, and outcomes of the offender's family members (e.g., children, siblings, spouses, and parents) and their criminal networks. To fully understand the costs of a treatment, we need to measure both the direct effects on the treated individual and also the indirect effects on those not directly treated. These perhaps second-order questions are increasingly being studied. Though a few pieces of research are mentioned in this report, a thorough review of the unintended effects or spill-over effects of sanctions is beyond its scope.

This report highlights how the probability and severity of punishment can impact recidivism behavior. But, it is important to acknowledge (again) that these factors can also impact crime rates by deterring the general population from committing crime—i.e., not just through recidivism. And to the extent that punishment leads to incarceration, there are some crimes that will be prevented while offenders are explicitly incapacitated or isolated from society. This report has abstracted away from such incapacitation effects, and focuses on the impact of incarceration on post-release behavior.

Another part of the story is how much a treatment costs. From a policy perspective, we clearly care not just about how much a given policy reduces crime, but how much it reduces crime per dollar spent and whether there are more cost-effective policies. Again, this is beyond the scope of the current report.

In summary, just because a policy reduces recidivism, as highlighted in this report, does not mean that it should be implemented. I urge policy makers—and researchers—to pay attention to the whole picture, including the monetary costs and other unintended costs (or benefits) on those directly and/or indirectly treated.

Looking Forward in the Swedish Criminal Justice System

As crime rates and public concern about crime have increased in recent years, criminal justice policy has become an active part of the Swedish debate. Some changes have already occurred, either in laws and regulations or in criminal justice practices, while others are still on the table. It is generally too soon to evaluate their effects. This section highlights some of these reforms, with an emphasis on what the above cited literature implies about their potential effects.

Most of the legislative changes are geared towards increasing punishment severity. More people are being sentenced to prison and sentences, for certain categories of offenders, are longer. Prison populations are forecasted to grow by Kriminalvården by upwards of 20 percent between 2022 and 2026 (Kriminalvården, 2023). The quickly growing prison population has led Kriminalvården to take both short-term and long-term measures to meet the (forecasted) changes in the prison population.

In the short term, Kriminalvården has instituted the practice of housing two inmates in a cell (*beredskapsplatser*). While such a practice is common in other criminal justice systems around the world, and in especially the US, a historical hallmark of Swedish prison placement policies was single person cells. Breaking with this tradition in 2022, Kriminalvården assigned 2,200 inmates to double up in cells designed for one person (Kriminalvården, 2023). How will this impact recidivism? There is to my knowledge no research estimating the causal effect of sharing a cell versus having a private cell. But, as noted above, better prison conditions have been shown to reduce recidivism. If the sharing of a single person cell is a decline in prison conditions (arguably it is, given the small space per person), then one may expect recidivism to increase.

Another step to immediately deal with the prison capacity crisis is to increase the use of electronic monitoring. As mentioned earlier in the report, Kriminalvården has been given the ability to increase the use of electronic monitoring for those who qualify but do not apply themselves. While electronic monitoring has generally been found to either decrease or have no effect on recidivism, it is not obvious that this effect will be seen for such offenders, who may be more negatively

selected than others.

Another short-term solution is the assignment of inmates to prisons that are not their designated class. This solution arises from the fact that capacity constraints and over-crowding are not equally distributed across security classes. To the extent that higher security prisons tend to have worse conditions and worse peers, it is important to consider how this affects the prison experience for those in the correctly designated security class. If, for instance, more minor offenders are being exposed to worse peers, there could be unintended spill-over effects onto the recidivism behavior of this group.

Finally, to deal with the forecasted growth in the medium and long-term, old prisons are being re-opened and new prisons are being built. Placement in these facilities can impact recidivism behavior, depending on the conditions and environment. As seen earlier in the report, new prisons tend to have better conditions.

Let us now turn to the policies that in part underlie the forecasted growth in the prison population. One of the most significant sentencing reforms to date is the abolition of Sweden's *punishment rebate for young offenders* (the *ungdomsreduktion* or youth rebate) on January 2, 2022. Prior to the reform, young adult offenders (ages 18–21) were not treated as adults in sentencing, but instead received only a proportion of the adult sentence. This proportion was generally smaller for younger individuals. After the reform, these individuals now receive the adult sentence. Will this harsher punishment decrease recidivism? This is indeed not a straightforward question to answer, in part because of the black box of prison. While research has found that more time in prison decreases recidivism, the effects of prison in general are more mixed for juveniles than adults. Moreover, research has shown that prison conditions matter; if these longer prison sentences are served in facilities with worse conditions, then any deterrence effect (or rehabilitation effect) of the longer sentence can be cancelled out. But, such a reform can also impact crime through channels other than the recidivism behavior of those directly treated by the reform. It can, for instance, also reduce crime by both incapacitating young offenders for longer (i.e., explicitly preventing crime while they are incarcerated) and by deterring young offenders from committing crime at all. At the same time, even younger persons may be pushed into criminality by gangs recruiting members who are not subject to the harsher sanctions.

Another reform that may have a significant impact on prison populations is changes in sentencing policies for offenders convicted of multiple offenses (SOU 2023:1). If this reform is passed, it will be enacted in 2025. For individuals convicted of three or more crimes at once, the length of the sentence will increase by 25 percent. As noted above, longer prison sentences have been seen to reduce recidivism, for at least some subsamples, in the US and Sweden. But it is important to caveat this finding. The underlying Swedish analysis was conducted using reforms in the 1990s, a time period when prison was below capacity. If growing prison populations lead to worse conditions (e.g., overcrowding, less treatment, or more violent peers), it is not guaranteed that these effects will generalize to contemporaneous reforms.

Finally, though most of the reforms on the table relate to punishment severity, it should not be forgotten that recidivism behavior is not only determined by sanctions but also significantly impacted by treatment and labor market opportunities. A better understanding is needed of the barriers to employment that workers with criminal records face in Sweden and how to reduce these barriers.

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SOU 2023:1. *Skärpta straff för flerbrottslighet.* Stockholm: Justitiedepartementet.

Repeat offenders commit a disproportionate share of crimes. Societies aiming to reduce crime thus must not only prevent individuals from committing their first crime but also prevent known offenders from returning to crime. This report reviews the evidence related to how four broad factors impact recidivism or repeat offending: (i) the probability of getting caught, (ii) diversion from the criminal justice system and prison, (iii) prison, and (iv) post-prison environments.

Given increasing concerns about crime and forecasted growth of Swedish prison populations, there is an active public debate in Sweden on how to handle the situation. Many of the topics discussed in the report—and especially the effects of electronic monitoring, juvenile incarceration, longer sentences, and prison conditions—are highly relevant to the current Swedish context.

Thus, the goal of the report is to shed some light on this debate and highlight the channels through which the four factors can impact recidivism, the reasons why it is hard to identify a causal effect, and the approach and findings of the quasi-experimental research studying these issues. With this focus on causal evidence, the report can inform policy makers and authorities on the potential impacts on recidivism of reforms.

The report's emphasis on causal evidence is especially relevant as Swedish authorities have an ambition for using an evidence-based approach to criminal justice policy.

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ISBN 978-91-89754-54-6