Formal Child Care and Later-in-Life Delinquency

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Abstract

Formal child care has been shown in a wide set of studies to affect long-term outcomes, such as education, income and health, but does it also have an impact on later-in-life delinquency? In this paper, I investigate how formal child care affects criminal charges in youth and early adulthood. I use three reforms connected to formal child care in Norway, together with rich Norwegian register data, identifying differential access or incentives to use formal child care. The first reform introduced formal child care to children aged 3-6, which induced variation in the expansion of child care across municipalities. Using a difference-in-differences approach, I compare affected and non-affected cohorts in low- and high-expanding municipalities. In the second reform, a compulsory year of child care for 6-year-olds was implemented. This affected children born after January 1, 1991, which I use in a difference-in-regression-discontinuity design. Finally, in the third reform, a cash-for-care benefit was introduced for parents of 1- and 2-year-olds not attending formal child care, and hence de-incentivizing child care use. In a difference-in-differences design, I exploit the variation in treatment across cohorts. The results from the three reforms indicate that formal child care decreases criminal charges, and that effects are driven by men. There is no clear pattern in which type of offenses are affected by formal child care.

Keywords: child care, early childhood, crime
JEL codes: J13, I21, I38, K42

Acknowledgments: This research was conducted as part of the Starting Grant project 'Criminality, Victimization and Social Interactions' (CIVICS 757279) funded by the European Research Council. I thank seminar participants at Stockholm University, the Norwegian School of Economics and the Scandinavian PhD Seminar Applied Economics for helpful comments. The paper substantially improved as a result of conversations with Katrine V. Løken, Aline Bütikofer, Matthew Lindquist and Manudeep Bhuller.

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1 Introduction

The cost of crime to society is large, including costs of prevention (e.g., police), punishment (e.g., courts and prison) and rehabilitation (e.g., prison and rehabilitation programs). In the US, there were over 1.4 million people in prison in 2019 (U.S. Bureau of Justice Statistics, 2020) and the total yearly cost of crime has been estimated to 1.7 trillion USD (Anderson, 2012). There are also large costs for victims, including decreases in earnings and increases in benefit receipt (Bindler and Ketel, 2020).

Policies directed at prevention of crime have mainly focused on direct measures such as police presence, higher sentences or youth interventions. The evidence suggests that police presence (absence) can reduce (increase) criminal acts in the affected geographic areas (e.g., Cheng and Long, 2018; Mello, 2019; Vollaard and Hamed, 2012; Weisburd, 2021). A number of papers show that increased sentences do not only reduce crime by incapacitation, but also through deterrence (see e.g., Bell et al., 2014; Drago et al., 2009; Abrams, 2012; Kessler and Levitt, 1999). Interventions or events during the teenage years can also be effective in reducing crime in the short run, but there is little evidence on the long-term effects (see e.g., Heller et al., 2017; Eren et al., 2017; Anderson, 2014). There has been much less work on early interventions and crime. One reason is that this requires following cohorts over a long time period as the time between interventions for young children and the time they start committing crime (youth and early adulthood) spans at least 10 years.

A broad literature across different fields provides evidence that non-cognitive skills are strong determinants of criminal behavior (see e.g., Caspi et al., 1994; Agnew et al., 2002; Pratt and Cullen, 2000), and that these skills are determined to a large extent in early childhood (Heckman et al., 2006). Similarly, high-quality subsidized child care has proven to induce positive changes in non-cognitive skills, especially among children from disadvantaged families (Heckman et al., 2013; Felfe et al., 2015). There is also evidence that child care can affect long-term outcomes of children, such as education, income and health (e.g., Campbell et al., 2014; Conti et al., 2016; Garces et al., 2002; Havnes and Mogstad, 2011b, 2015; Heckman et al., 2010, 2013).

It has been proven difficult to identify the effects of attending formal child care on youth or adult delinquency. One reason is data availability. This is because it (ideally) requires a long and representative panel data set with information on individuals' child care enrollment, as well as their criminal behavior. Another problem are the threats to identification caused by endogenous enrollment in child care. Child care enrollment is correlated with other factors such as maternal employment, which may also impact later-in-life outcomes. My paper addresses both the data and identification issues, and provides new evidence on the impacts of formal child care on criminal behavior.

I use three reforms affecting the take-up of universal child care in Norway, together with rich individual level register data, to study the effects on delinquency. The reforms differ in multiple ways; they affect different-aged children, vary in how they affect incentives for take-up of child care, and have different compliers. By using different reforms, we get a more complete picture of the effects of child care on crime within one country where other institutional settings are similar across reforms.

As previous literature has shown that child care has positive effects on long-term outcomes, the expectation is that it will also reduce crime. The mechanisms are potentially both through improvements in cognitive or non-cognitive skills, but also indirectly through the effects on e.g., education or earnings, which we know are factors strongly correlated with criminality. The vast majority of offenders are men, and it is therefore reasonable to expect that male delinquency will drive the results. Since reforms differ by target age groups, compliers, counterfactual care options and time of implementation, effects could differ across reforms. These differences can occur both in effect sizes, but we might also expect different subgroups to respond differently, in terms of, for example, socio-economic background.

The first reform was introduced in 1975, and introduced universal and subsidized child care for children aged 3-6. It regulated the authorization and running of child care centers, including quality measures. The municipalities were given the responsibility for establishing and running the centers. I use the empirical strategy from Havnes and Mogstad (2011b) to estimate the effects of the reform. The idea is that the expansion of child care differed across municipalities, and this is used in a difference-in-differences setting, comparing cohorts in the same municipality, and similar cohorts across municipalities, affected differently by the reform. The results show that the reform decreased the likelihood of being charged as an adult. The decrease is driven by men, and by traffic offenses. Suggestive evidence indicates that children of fathers with low income are impacted to a higher degree.

In the second reform, a compulsory year-long program was introduced for six-year-olds, effectively lowering school starting age from 7 to 6. It was implemented in 1997. The compulsory program was set up as a child care program, but conducted in schools, with the goal of preparing children for school. Children born in 1991 were the first cohort affected by the reform. I use this in a difference-in-regression-discontinuity (DiRD) strategy, using the fact that children born before new year in 1990/91 were not affected, while those after were, and compare this to the equivalent for the previous year. This takes into account that children born before new year may differ from those born after due to seasonality effects of births (Black et al., 2011). I find that the reform decreased the number of charges, driven by men. The effect is driven by decreases in narcotics offenses. The results suggest that effects are larger for children of mothers with only high school education, and fathers with low income.

The third reform introduced a cash-for-care benefit to parents of 1- and 2-year-olds in the end of the 1990s. Parents were eligible for the benefit if their child was not enrolled in subsidized full-time child care, and the monthly payment was equivalent to a state subsidy for a place in formal child care. This implies that the benefit served as an incentive to substitute formal child care with parental care or informal care. Different cohorts were affected differently by the reform, and I use this in a difference-in-differences setting, comparing the outcomes in the years 2010-2018 of 18-year-olds, who were partly or fully treated for later years, and 23- to 27-year-olds, who were never treated. The results suggest that the cash-for-care benefit increased the likelihood of being charged and the number of charges, driven by men. Charges for all types of offenses increased, and effect sizes are larger for children of mothers with a high school diploma or less, and of fathers with low income.

To summarize, the results from all three reforms point in the same direction - enrollment in formal child care seems to decrease the risk of criminal behavior later in life. The effect is driven by men. There is no clear pattern in the type of crime over the three reforms, which could be due to differences in compliers, different ages of outcome measurement and different ages of the children affected by the reforms. Suggestive evidence indicates that effects are larger for children from lower socio-economic backgrounds.

The most prominent literature related to the effect of formal child care on youth and adult outcomes comes from four programs in the US: the Abecedarian, the Perry Preschool, Project CARE, and Head Start.

The Abecedarian, Perry and CARE projects were all three randomized control trials (RCTs), directed at children from disadvantaged families. While the Perry Preschool Program randomly assigned children to high-quality child care, the Abecedarian and CARE projects also included health care and nutritional components (Conti et al., 2016; Campbell et al., 2014). Project CARE also had one treatment group with only home visits (Campbell et al., 2014). All three were small scale projects with 111 (Abecedarian), 123 (Perry) and 66 (CARE) participants in total.

The reduction in youth and adult crime has been high-lighted as the possibly most important benefit of the Perry Preschool Project (Schweinhart et al., 2005; Heckman et al., 2010). The program also had a positive impact on education, income, health and healthy behavior (Schweinhart et al., 2005; Heckman et al., 2010; Conti et al., 2016). The results from Heckman et al. (2013) suggest that changes in personality skills, rather than cognitive skills, can explain (parts) of the adult treatment effects.

The results from the Abecedarian Project, suggest that there is no significant effect on crime at age 21 (Clarke and Campbell, 1998; Campbell et al., 2002). For Project CARE, there are no studies related to delinquency to my knowledge. Both the Abecedarian Project and Project CARE, had positive effects on educational attainment, as well as reducing marijuana use and an increase in the adoption of an active life style (Campbell et al., 2008). The Abecedarian Project also seems to improve health, with the treated having a lower prevalence of risk factors for cardiovascular and metabolic diseases in their mid-30s (Campbell et al., 2014).

Head Start is a larger scale program than those presented above. It provided child care/pre school of a lower quality than, for example, the Perry Preschool (Garces et al., 2002). The evidence suggests that participating in Head Start reduces the risk of being charged in the early 20s (Garces et al., 2002; Carneiro and Ginja, 2014). There are also positive effects on educational attainment, income, health and behavior (Garces et al., 2002; Thompson, 2018; Carneiro and Ginja, 2014).

To the best of my knowledge, there are two previous studies of the effect of large scale universal childcare on delinquency. Baker et al. (2019) study the long-term effects of introduction of universal child care in Quebec, Canada. They use a difference-in-differences estimation, comparing outcomes of pre- and post-reform cohorts in Quebec to the equivalent in other Canadian provinces. They find that the cohort-crime rate in Quebec increased as a result of the reform. Other studies have shown that this reform mainly increased child care use for children of highly educated mothers, and the child care was of lower quality (Haeck et al., 2015) and that the effects were positive for children from single-parent households (Kottelenberg and Lehrer, 2017). This is very different from the Norwegian setting where the quality of child care is good. Brutti and Montolio (2021) study the effect of a reform expanding formal child care access for 3-year-olds in Spain. They use the difference in roll-out rate across regions, in a difference-in-differences framework, together with cohort and region level crime. The findings suggest that a 1 percentage point increase in preschool access for 0-3-year-olds decreases the number of reported crime actions by 1.6 percent. They find larger effects on impulsive-crime categories. My main contribution relative to this paper is that I study multiple reforms that affected child care use in different ways and affected different age groups. Contrary to

Brutti and Montolio (2021), I use detailed individual level register data, with the possibility to look at heterogeneous effects with regards to e.g., gender and socio-economic background, as well as in the type of offense committed.

There are several papers on the effect of universal child care on other outcomes (Berlinski et al., 2009; Havnes and Mogstad, 2011b, 2015; Datta Gupta and Simonsen, 2010; Dumas and Lefranc, 2012; Cornelissen et al., 2018; Felfe et al., 2015; Felfe and Lalive, 2018; Magnuson et al., 2007; Black et al., 2014; Loeb et al., 2007). The main takeaways from these papers are that universal childcare on average has long-term effects on education and income and that effects are larger for children from low-socio economic backgrounds. The evidence on short- to medium-term outcomes, mainly related to school outcomes, suggests that effects are positive, but possibly fading. There are a few exceptions, finding no or even negative effect (Baker et al., 2008, 2019; Haeck et al., 2015; Carta and Rizzica, 2018; Fort et al., 2020), and this seems to be related to the programs being of lower quality and directed or used by high SES families.

Finally the paper is related to studies on effects of other types of early life conditions and education on crime. There is evidence that conditions in early life, such as exposure to lead (Aizer and Currie, 2018; Billings and Schnepel, 2018), neighborhood crime (Damm and Dustmann, 2014), and illegal labor markets (Sviatschi, 2018), as well as family background (Eriksson et al., 2016) and childhood maltreatment (Currie and Tekin, 2012), affects youth and adult delinquency. Studies also suggest that for children on the margin, being placed in foster care increases crime (Doyle, 2007; Doyle Jr., 2008), while a recent study finds an imprecise decrease in delinquency (Gross and Baron, 2021). There is a consensus in the literature that both the quality and quantity of education affects crime (Anderson, 2014; Bell et al., 2016; Bennett, 2018; Berthelon and Kruger, 2011; Deming, 2011; Fella and Gallipoli, 2014; Machin et al., 2011). The effect is driven both by incarceration and behavioral changes, and the effect is heterogeneous across individual observables and education levels.

The remainder of the paper proceeds as follows. In section 2, I discuss the expected effects of universal child care on delinquency. Section 3 provides a background of the criminal justice system in Norway. Section 4 describes the data. In section 5, I present the institutional setting, identification and results for the introduction reform. Sections 6 and 7 provide the equivalent for the reforms of compulsory child care year and cash-for-care. The final section includes a discussion and conclusion.

2 Expected Effects

Previous literature has provided evidence that formal child care affects cognitive and non-cognitive skills (e.g., Heckman et al., 2010, 2013; Berlinski et al., 2009). Cognitive skills consists of factors promoting e.g., information acquisition, processing and problem-solving. Non-cognitive skills can include personality traits, persistence, motivation and charm (Heckman et al., 2006). The effects of child care on cognitive skills is seemingly fading (e.g., Magnuson et al., 2007; Blanden et al., 2016) but the effects on non-cognitive skills has proven to be persistent (e.g., Heckman et al., 2013). The improvement in skills is driven by children from disadvantaged families (e.g., Felfe et al., 2015; Cornelissen et al., 2018), and there is even some evidence of negative effects for children from advantaged families (e.g., Fort et al., 2020). The mechanisms behind both the positive and negative effects could be e.g., interaction with trained staff, less one-to-one interaction with adults, and interaction with other or different children. The results from previous research also suggests that the quality of care and the alternative to formal child care is important in interpreting effects (e.g., Haeck et al., 2015). Both cognitive and non-cognitive skills can affect delinquency both directly and indirectly, through e.g., education, income and (mental) health.

Heckman et al. (2006) study the predictive power of cognitive and non-cognitive skills on labor market outcomes and social behavior. They find that while both types of skills are of importance to the risk of delinquency, non-cognitive skills hold a larger predictive power for males.[1] If a man in the lowest decile of cognitive ability is moved from the lowest to the highest decile of non-cognitive ability, the risk of incarceration decreases significantly, while doing the equivalent for cognitive ability only decreases the risk slightly. This suggest that the link between child care and delinquency operates through effects on non-cognitive skills.

Formal child care generally has a positive effect on educational attainment (e.g., Havnes and Mogstad, 2011b; Heckman et al., 2010; Dumas and Lefranc, 2012). For income, the evidence suggests that there is an equalizing effect, increasing the income in the lower part of the distribution, while the higher part of the distribution may suffer from a decrease in income (e.g., Havnes and Mogstad, 2011b, 2015). The literature suggests that both higher educational attainment and income equality leads to lower delinquency (e.g., Hjalmarsson and Lochner, 2012; Choe, 2008). There is also evidence that formal child care can improve mental health in the long-run (e.g., Breivik et al., 2019). While I have found little evidence of the causal impact of mental health on crime, the literature suggests

[1] Incarceration is not an empirically important phenomenon for females(Heckman et al., 2006).

that mental illness is more common among offenders than in the general population (e.g., Gottfried and Christopher, 2017). Child care also seems to decrease marijuana use (e.g., Campbell et al., 2008). Since this is an offense in Norway, and might also be connected to use of heavier substances, this might be a mechanism for the results as well.

Finally, since children in formal child care are more likely to come in contact with other/more adults, including trained staff, problems in the home might be easier to detect, and contacts with social services could increase as a result. Two studies from the US have shown that foster care can increase criminal behavior for marginal individuals (Doyle, 2007; Doyle Jr., 2008). A recent study by Gross and Baron (2021), finds an imprecise decrease in delinquency, and positive impacts of foster care on educational outcomes. Foster care might have different impacts on different populations, and is also not the only outcome of contact with social services. It is therefore difficult to conclude how this may affect adult delinquency.

Most of the factors discussed above suggest that formal child care should reduce crime, and that it is most likely driven by children from disadvantaged families. One should note that most of this evidence comes from interventions that were successful in the short run, i.e., with promoting children's cognitive or non-cognitive skills. If child care is of lower quality than the alternative, the effect might be reversed, especially for children with highly educated parents. However, these are children with low risk of committing crime at the outset.

3 Crime in Norway

In this section I provide an overview of what happens when a report of criminal offense is made to the Norwegian Police. I also shortly discuss the development of crime in Norway over time, as well as present descriptive statistics.

When there is suspicion that a crime has been committed, a report is generally filed with the Police authority. Reports can be filed by the public or by the police. One example could be that an individual is stopped by the police for driving over the speed limit. The police would then file the report.

An illustration of the process after an offense is reported is presented in figure 1. The first thing that happens after reporting is that the police opens an investigation. In 2019, approximately 288 000 police investigations were conducted (Statistics Norway, 2020a).

When the investigation is completed, a decision is made on whether to press charges or not. Cases can be dismissed because they are unresolved. These cases stand for approximately half of the investigations. The most common reasons for unresolved cases are missing information about the perpetrator or a lack of evidence. The police and prosecuting authority can also dismiss a case because no punishable offense was committed. These cases are not a part of the statistics over investigated offenses.

Even if the case is solved, there are other reasons why charges are not made. If the perpetrator is younger than 15 years, he or she is not criminally liable, and will therefore not be charged. This stands for approximately 3 percent of all investigated cases. In other cases, the perpetrator is exempt from charges or there are other reasons why charges might not be filed. This could, for example, be cases of minor offenses committed by individuals without previous criminal records. These cases stand for approximately 8 percent of the total number of investigations.

In almost 40 percent of the cases, the prosecution will decide to press charges. Over half of these cases will go to court proceedings. This includes all serious offenses, including those where a prison sentence is a possible outcome. It also includes all cases where the perpetrator does not plead guilty.

In the vast majority of cases where the charge does not lead to trial, a fine is issued. This could be the case for our example with speeding, e.g., if the driver pleads guilty to driving only slightly above the speed limit and does not have a criminal record. These cases are not a part of the statistics on criminal charges. A small number of cases go to mediation.

In 2019, almost 280 000 punishments were set, of which a vast majority (78 percent) were on-the-spot fines (Statistics Norway, 2020b). Over 8600 prison sentences were set in 2019. There are also other possible punishments such as community service. In our example with speeding, the punishment of the driver will be dependent on factors such as how far above the speed limit he or she was driving and his or her criminal record. Punishments could then range from prison to a revoked driver's license to a single on-the-spot fine, depending on these factors.

Figure 2a displays the development in the number of reported offenses per 1000 inhabitants in Norway between 2003 and 2019. There is a clear downward trend in the number of charges. In 2003, the number of reported offenses per 1000 inhabitants was 92.4, while the same number in 2019 was under 58.2. The main driver is a large decrease in the reported offenses of property crime (Statistics Norway, 2021).

In figure 2b, I present the age and gender profiles of charged individuals in Norway in 2018. From the figure, we can see that the age of the charged individuals is concentrated among youth and young adults, especially around ages 18-25. The share is then quite steadily decreasing with age. Over 80 percent of the charged individuals are men.

4 Data

In this paper, I link several Norwegian administrative data sources using unique personal identifiers. I am also able to link the individuals to their family members.

From Statistics Norway, I have access to population panel data with individual demographics, including gender, immigrant status, education and family members. Furthermore, I use tax registers for information on income and social insurance registers including information on payments. Through the municipality database from the Norwegian Centre for Research Data (NSD), I have gathered information on child care coverage[2] on the municipality level.

From the Norwegian Police Directorate, I have access to data on all criminal charges between 1992 and 2018. The age of criminal responsibility in Norway is 15. The combination of the available years, the age of criminal responsibility and the timing of the reforms, imply that different ages are studied for different reforms. This will be explained further in the identification section of each reform, and is important for interpretation of the results.

The main outcomes are an indicator for being charged and the number of charges during the studied period. Furthermore, I have access to the type of offenses connected to the charges. The standard in Norway is to divide the type of offense into ten groups; economic offenses, other offenses for profit, violence offenses, sexual offenses, narcotics offenses, damage to property, environment offenses, work environment offenses, traffic offenses, and other offenses. As some of these groups of crimes have few observations in the data, I have taken outset in this grouping to limit the type of offense into 5 groups: offenses for profit (includes economic offenses and other offenses for profit), violent or sexual offenses, narcotics offenses, traffic offenses and other offenses (includes damage to property, environment offenses, work environment offenses, and other offenses). I use an indicator for being charged with an offense in each of these groups as outcomes.

Finally I have information on the set punishments. These are grouped into five categories; prison, probation, community service, fines and other.

[2] Child care coverage is the share of children within a certain age span (e.g., 3-6) enrolled in child care.

5 Introduction of Universal Child Care

Institutional Setting

In 1975, the Kindergarten Act was passed by the Norwegian Parliament, introducing universal and subsidized child care for children aged 3-6. Through the act, the municipalities were made responsible for the provision of child care. Furthermore, the act regulated prices, group size, educational content, physical environment and staff skill composition.

Women's entry in to the labor market, caused an increase in the demand for out-of-home child care in the 1950s and 1960s. A survey conducted in 1968 provided evidence of an unmet demand for formal child care (Norwegian Ministry of Administration and Consumer Affairs, 1972). Only 14 percent of children of respondents were in formal child care, while 32 percent of respondents expressed a demand for it.

The aim of the reform was to increase the number of child care spots up to 100 000 by 1981. In figure 3, the average municipal coverage rate is presented. In 1975, the average coverage rate was 4.3 percent. In the years following the reform, the number of children in child care increased substantially. In 1979, the average coverage rate was 25.2 percent. Government funding of child care also increased, from USD 34.9 million in 1975 to 85.8 million in 1977 (Havnes and Mogstad, 2015).

Havnes and Mogstad (2011a) provide evidence that the reform did not lead to an increase in maternal labor supply. This suggests that the reform induced a move from informal to formal child care, which is something we should have in mind when interpreting the results in this paper.

Previous studies of the effects of the reform also suggests that it lead to an increase in the educational attainment and labor market participation (Havnes and Mogstad, 2011b). The reform had an equalizing effect on income (Havnes and Mogstad, 2015). Finally, Breivik et al. (2019) find that it had a positive effect on long-term health.

For a more detailed description of the institutional setting, see Havnes and Mogstad (2011b) or Breivik et al. (2019).

Identification

To analyze the effect of the 1975 reform, I use the empirical strategy from Havnes and Mogstad (2011b). The main idea is that the roll-out rate differed across municipalities in the years following the reform. I focus on the expansion period between 1976 to 1979, and divide municipalities into treatment (control) if they were above (below) the median increase in coverage rate during the selected years.

Figure 4 displays the average municipal child care coverage rate for children aged 3-6, in treatment and control municipalities in the years 1973-1985. In the years leading up to the reform, the coverage rate was low, and similar in treated and control municipalities. Between 1976 and 1979, the coverage rate grew substantially in the treated municipalities, up to almost 40 percent in 1979. The growth in the control municipalities was much lower in these years. They caught up slightly in the early 1980s, but remained at a much lower level than the treated municipalities.

The sample consists of individuals born in 1967-1976. Individuals born before 1969, henceforth referred to as the pre-reform cohorts, where never affected by the reform, since they had already started compulsory schooling in 1976. Individuals born between 1970 and 1972, were between 3 and 6 years old when the expansion started, and therefore were partly treated. I refer to these individuals as belonging to the phase-in cohorts. Finally, individuals born in 1973-1976, were 3 or younger when the expansion started and therefore fully treated. They are referred to as the post-reform cohorts.[3]

I implement a difference-in-differences (DiD) strategy, using the differential treatment of municipalities and cohorts, with the following specification:

$$Y_{ijt} = \beta_1 + \beta_2(Phase - in_t \times Treat_j) + \beta_3(Post_t \times Treat_j) + \beta_4 X_i + \theta_j + \gamma_t + \varepsilon_{ijt} \quad (1)$$

where Y_{ijt} is the outcome for individual i in municipality j born in year t. $Phase - i_t$ ($Post_t$) is an indicator for being born in the phase-in (post) cohorts. $Treat_j$ is in indicator for individuals living in a treated municipality in 1976. X_i is a set of individual demographics including gender, number of older siblings, and parental education, birth year and age at birth. I also include municipality (θ_j) and cohort (γ_t) fixed effects.

The parameters of interest are β_2 and β_3, measuring the DiD coefficients for phase-in and post-reform cohorts respectively. Coefficients should be interpreted as intention to treat (ITT), as we cannot measure which children attended child care and not. Standard errors are clustered on the municipality level. Outcomes are measured in 1992-2006. This implies that criminal behavior is measured when the oldest cohort (born in 1967) is between 25 and 39, and the youngest cohort (born in 1976) is between 16 and 30 years old.

[3] I exclude individuals that have moved between a treated and control municipality between 1976 and 1979 from the estimation sample. This is approximately 5 percent of the sample. This exclusion has no substantial impact on the estimates.

The main identifying assumption is that the expansion of child care does not coincide with other municipality level changes affecting pre-reform, phase-in and post-reform cohorts differentially. In figures A.1-A.3 in appendix A, I provide evidence that municipality child care expansion does not correlate with other confounding factors such as changes in family structure, unemployment, total expenditure, expenditure on primary school and health expenditure.

Results

Table 1 displays the intention-to-treat (ITT) effects of the child care coverage change induced by the 1975 reform, on the likelihood of being charged and the number of criminal charges between 1992 and 2006. The estimating equation is described in the previous section, and the coefficient DiD is equivalent to β_3 in the equation. In panel A, the estimates for the full sample are presented, while the estimates for men (women) are displayed in panel B (C). For phase-in cohorts, there were no significant effects for the full sample or for men (see table A.6 in appendix A). For women, there was an increase in the likelihood of being charged (0.5 pp) for the phase-in cohorts.

In the full sample, the reform decreased the likelihood of being charged by around 0.8 percentage points. In the pre-reform cohorts, approximately 18 percent of the sample had been charged between 1992 and 2006. While the coefficient is not small for an intention to treat estimate, it is only significant at the 10 percent level when controls are included. The number of charges did not decrease significantly.[4]

The decrease in the likelihood of being charged is driven by men. When we include controls, the share of men being charged decreases by 1.5 percentage points, significant on the 10 percent level. For women, the likelihood of being charged is not significantly changed by the reform. There is no significant change in the number of charges for either women, or men.

In table 2, I present the ITT effects of the reform on the likelihood of being charged with the different types of offenses described in section 5, i.e., offenses for profit, violent or sexual offenses, narcotics offenses, traffic offenses, and other offenses. In panel A, the results for the full sample are shown. The reform induced a decrease in the likelihood of being charged with traffic offenses (0.8 percentage points) and other offenses (0.5 percentage points), significant on the 10 percent level. The likelihood of being charged with offenses for profit, violent offenses or narcotics offenses does not seem to be affected

[4]While we do not have data on individual take-up, we could scale by the average difference in change in coverage between treatment and control municipalities. This is about 17.62 percentage points and if we scale the ITT by this, the effect size is 4.3 pp.

by the access to child care.

For men, the decrease in the share being charged with traffic offenses increases even more, by approximately 1.6. For women there is no significant effect on any type of offense.

In table 3, I present the ITT effects of the reform on the likelihood of being charged and the number of charges by the mother's education level. Including controls, the decrease in the likelihood of being charged for children with a mother with a lower education level than high school is 0.8 percentage points, while the equivalent from high school or higher is 0.6 percentage points. For the number of charges, children with low-educated mothers have an slight increase in the number of charges (0.03), and children of high-educated mothers a decrease in the number of charges (0.06). None of the results are significantly different from zero, suggesting no clear pattern in the effect by mothers' education level.

Table 4 displays the ITT estimates of the effects by fathers' income quartile. The highest point estimates are found in the first quartile, with a decrease in the likelihood of being charged by 1.4 percentage points, and a decrease in the number of charges by 0.2. There is no clear pattern between the other income quartiles. While none of the results by fathers' income level are statistically significant, the pattern is consistent with previous research, showing that children from low SES families are the drivers of positive effects of formal child care.

A number of robustness checks are presented in appendix A. These include estimates by treatment intensity and tertile treatment groups. For treatment intensity, estimates point in the same direction as the main results, but the only significant estimate is for the likelihood of being charged for men. The tertile treatment groups suggest that it is the municipalities in the highest tertile treatment that are driving the effects. Furthermore, I have estimated the effects on the likelihood of being charged with more than 2, 5 and 10 charges, and find a significant effect on the likelihood of being charged at least 2 times, but no significant effect for 5 and 10 charges. I have also studied the effect on charges at age 25-30, and find similar, but not significant, results as in the main analysis. Finally, I have estimated the effects on the likelihood of being sentenced to different punishments, and find a decrease in the likelihood of getting a fine for men, but no other statistically significant effects. This is in line with traffic offenses being the driver of the decrease in delinquency. All in all, while I am lacking precision, the point estimates from the robustness checks support the results from the main analysis.

To summarize, the reform decreased the likelihood of being charged, driven by men. It did not affect the average number of charges significantly. The share charged with traffic offenses and other offenses decreased, but other type of offenses are not significantly affected by the reform. Suggestive evidence indicates that children of fathers with low income are affected to a larger extent.

6 Compulsory Child Care for Six-Year-Olds

Institutional Setting

In 1997, a reform that lowered the school age from 7 to 6 years was implemented in Norway. As a result, individuals born in 1991 or later also got an additional year of compulsory schooling.

Before the reform, 6-year-olds (and younger children) had access to high quality subsidized child care. 89 percent of non-immigrant 6-year-olds were enrolled in formal child care in 1996 (Drange et al., 2016).

The background for the reform was that children enrolled in formal child care could take part in school preparation, which was not available for others. The compulsory program for 6-year-olds, was to be child care like, preparing children for school. There was a social gradient in child care participation prior to the reform (Drange et al., 2016), and the hope was that the compulsory program would even out differences in learning outcomes.

The government proposed the reform in a White Paper in 1993 (Norwegian Ministry of Education, 1993) and the bill was passed in 1994 (Norwegian Ministry of Education, 1994). It was implemented in 1997, and the first cohort starting school at age 6 were those born in 1991.

Drange et al. (2016) uses a difference-in-differences approach, comparing the difference in schooling outcomes for children not attending formal child care at age 5 born before and after 1991, to the equivalent for children attending formal child care at age 5. They find no effect of the reform on schooling outcomes. Finseraas et al. (2017) use a regression discontinuity design to study the effects on mothers' labor supply, and find a positive effect, driven by mothers with low wage potential.

For a more detailed description of the institutional setting, see Drange et al. (2016).

Identification

In Norway, the calendar year serves as the cut-off for school cohorts. This implies that if a child is born on December 31 in one year, or January 1 in the following year, this will generally determine what school cohort the child belongs to. While it is possible to enroll one year ahead of time, or postpone the enrollment one year, this requires that parents formally apply for an exception, and approval from health and school specialists (see e.g., Norwegian Directorate for Education and Training, 2017). While there is no register data on children's age of enrollment in Norway, studies have found that only around 0.5 to 1.2 percent postpone enrollment, and that this is somewhat more frequent amongst boys (1.5 percent) (Cools et al., 2017; Gabrielsen and Lundetræ, 2017).

Normally, this rule implies that those born in December start when they are (almost) one year younger than those born in January. Black et al. (2011) analyze the effect of this difference in starting age, and find that starting at a younger age decreases IQ test scores and earnings until age 30. For the cut-off between children born in 1996 and 1997, the reform implied that children born in December 1996 started school at the same age as those born in January 1997, but one grade above.

With this as a background, I use a difference-in-regression-discontinuity (DiRD) design to analyze the effect of the compulsory program for 6-year-olds. The main idea is to compare the outcomes of children born around the cut-off December 31, 1990/January 1, 1991. The results from Black et al. (2011) suggest that using a simple RD to identify the effects of the reform will be problematic, as we will expect to find discontinuities around the cut-off regardless of the reform due to the age at school start effect.[5] I therefore take the difference between the discontinuity in the treated year and compare it to the same discontinuity in the year prior to the reform. It is estimated using the following baseline specification:

$$Y_{it} = \beta_1 + \beta_2(Z_{it} - c_t) + \beta_3 E_i + \beta_4 T_t + \beta_5(E_i \times T_t) + \beta_6(Z_i t - c_t)E_i + X_i + \epsilon_i \tag{2}$$

where Y_{it} is the outcome for individual i born around cut-off t. Z_{it} is the date of birth and c_t is the cut-off date. E_i is an indicator equal to one if the individual is born after the cut-off and T_t is an indicator equal to one if the individual is born around the treatment cut-off. X_i is a set of control variables including gender, immigrant background, birth municipality, parents' education, year of birth and age at birth. The interaction term $E_i \times T_t$ is then equal to one if the individual is born around and after the reform cut-off, that is, after December 31, 1990, and therefore β_5 is the coefficient of interest.

This approach differs from previous work by Drange et al. (2016), who use a differences-in-differences design, comparing children not enrolled in child care at age 5 to those enrolled, in cohorts starting school before and after the implementation of the reform. The reason I could use this new identification strategy is that the outcome variable is a monthly panel

[5]This strategy is also consistent with the results from (Landersø et al., 2017), suggesting that the age at school start effect is also present for patterns of criminal behavior.

while their school outcomes are one time measures. Their measure captures the difference of attending the compulsory program for those who would likely not have been enrolled in formal child care otherwise, to those who would have. The measure in this paper instead captures the difference between the compulsory program and all alternatives for care of 6-year-olds. The approach in this paper is more similar to Finseraas et al. (2017), who uses a regression discontinuity design around the cut-off, to study maternal outcomes.

In the baseline estimations, I include linear trends that are separate for each side of the cut-offs, but equal for individuals born in different years on the same side of the cut-offs[6]. I use 180 day bandwidth and triangular weights, and cluster standard errors on the municipality level. The outcomes are measured in 2008-2018. This implies that the criminal behavior is measured at ages 19-29 for the oldest individuals in the sample (born in 1989), and at ages 17-27 for the youngest individuals in the sample (born in 1991).

Results

In figure 5, the regression discontinuity figures for the share being charged and the average number of charges are presented. In each figure, I show the binned 7 day average (the scatter plot), and the linear trend with 95 percent confidence interval for each side of the cut-offs. The bandwidth is 180 days. In panels (A) and (C), the RD plots for the treatment cut-off (January 1, 1991) are presented, and in panels (B) and (D) the equivalent is presented for the control cut-off (January 1, 1990).

In panels (A) and (C), we can see that the share being charged and the average number of charges drops around the cut-off, giving us a first indication that the reform affected criminal behavior. But as discussed in the identification section above, we need to take into account the potential age of school start effect that occurs every year. Since there is no clear drop around the cut-off for the control group (see panels (B) and (D)), it is likely that the reform is causing the drop in delinquency.

Table 5 displays the DiRD estimates of the effect of the reform on the likelihood of being charged and the number of charges. In panel A, I present the estimates for the full sample, while panel B and panel C show the results for men and women respectively.

In the full sample, the estimate for the likelihood of being charged is negative (0.5 percentage points), but not significant, while the number of charges seem to decrease by approximately 0.1 as a result of the reform, from an average of 0.9. For men, the likelihood of being charged decreases, but not significantly, while the number of charges

[6] Another option would be using local linear regression. However, results from local linear regression are imprecise and sensitive to choice of bandwidth, see figure B.3 in appendix B.

decreases by 0.3 and is significant on the 5 percent level when controls are included. For women, the effect on both the likelihood of being charged is small and positive, but not significant.

In table 6, I present the estimates of the compulsory year of pre-school on the likelihood of being charged with different types of offenses. In panel A, we can see that in the full sample the reform seemingly decreased the likelihood of being charged with narcotics offenses and other offenses, but did not seem to affect offenses for profit, violent or sexual offenses, or traffic offenses significantly. For men (see panel B), the reform significantly decreased the likelihood of being charged with all types offenses, except for traffic offenses, that did not significantly change. For women (see panel C), the likelihood of being charged with other offenses decreased slightly. This effect is significant on the 10 percent level. For the other types of offenses, the effect is small, positive but not significant for women.

Table 7 displays the estimates by mothers' education divided in to three categories; less than high school (low), high school and some higher education (university or college). There are no statistically significant results on the likelihood of being charged. From the estimates on the number of charges, while children of mothers with low or higher education have no significant effect on the number of charges, children of high school (only) educated mothers displays a decrease of approximately 0.2, suggesting that they are driving the results in our main analysis.

In table 8, the results are divided by fathers' income quartile. Again, we find no significant effect on the likelihood of being charged. The largest decrease in the number of charges is in the 1st quartile, by approximately 0.3, but this decrease is only marginally significant. For quartiles 2 and 3, there are no significant effects, but for the last quartile we see a significant decrease in the number of charges by approximately 0.1.

In appendix B, I provide a number of robustness checks. First, I study the effects on indicators for more than 2, 5 and 10 charges. In total, as well as for men, I find significant negative effects on the likelihood of being charged at least two times, and negative but non-significant effects for five and 10 charges. I also study the effects on different types of punishments, and find significant decreases in the likelihood of getting community service for the full sample and for men. Furthermore, I study the effects on charges at age 18-25, and find significant decreases in the likelihood of being charged as well as the number of charges, for both the full sample and for men. I run a number of specifications testing different bandwidths and trends. While the precision and point estimates vary somewhat, the results from the specification support the results from the main analysis.

To summarize, the compulsory child care year for 6-year-olds significantly decreases the number of charges, driven by men. The effects seem to come mainly from a decrease in narcotics offenses and other offenses. Effect sizes are larger for children of mothers with high school only, and children of fathers with low income.

7 Cash-for-Care

Institutional Setting

In August 1998, the cash-for-care benefit was introduced for 1-year-olds, and in January 1999 for 2-year-olds. It gave parents of 1- and 2-year-olds a tax-free benefit, given that they did not use subsidized full-time child care (more than 32 hours per week). The benefit was equivalent to a state subsidy for a place in formal child care when it was introduced (Rønsen, 2009). In the first years after the introduction, around 80 percent of parents of 1- and 2-year-olds received the benefit (Statistics Norway, 2019).

In 2012, the benefit was removed for 2-year-olds (Statistics Norway, 2019). As per 2020, the full monthly benefit is 7500 NOK ($\approx$ 835 USD) per child (Norwegian Labour and Welfare Administration, 2020). It is possible to receive a share of the benefit if the child is in part-time subsidized child care.

The cash-for care benefit gives incentive to substitute formal child care for either parental care or informal child care. For never-takers of formal child care, the benefit only serves as an increase in income. In addition, since supply of child care slots for 1-2-year-olds is still limited in this period, it serves as an extra parental leave benefit for those who need to wait for formal child care.

Rønsen (2001) provided evidence that the reform lead to increases in both parental care, and informal care. The evidence from previous studies suggests that maternal labor supply decreased both in the short- and long-run (Rønsen, 2001; Schøne, 2004; Rønsen, 2009; Drange and Rege, 2013). The results from Drange and Rege (2013) indicate that mothers with low education or low pre-reform earnings are driving the effects, and that effects fade out when the child is around age 6. Previous research also suggests that older siblings' 10th grade GPA increased as a result of the reform, driven by a decrease in maternal labor supply (Bettinger et al., 2014).

For a more detailed decription of the institutional setting, see Rønsen (2009) and Bettinger et al. (2014).

Identification

Different cohorts were affected differently by the reform. Parents of individuals born in 1995 or earlier never had access to the cash-for-care benefit, and were therefore not treated. Parents of individuals born in 1996-1997, could have as much as 24 months eligibility or as little as 1 month, and I therefore refer to them as partly treated. Parents of individuals born in 1998 or later were eligible for 24 months of cash-for-care benefit, and are referred to as fully treated.

In 2010-2018, there are 18-year-olds belonging to all three treatment groups, while individuals 23 years or older in the same time period were only born in the pre-reform cohorts. I use this in a difference-in-differences (DiD) framework, which is illustrated in figure 6. This figure displays the share of 18-year-olds and 23-year-olds being charged in each year between 2010-2018. In years 2010-2013, neither the 18-year-olds, nor the 23-year-olds belong to cohorts affected by the cash-for-care reform. In 2014 and 2015, the 18-year-olds were in the partly treated cohorts, while the 23-year-olds were still in the non-treated cohorts. In the years 2016-2018, the 18-year-olds were in the fully treated cohorts, while the 23-year-olds were still not treated.

The estimation sample consists of 18-year-olds (treated), and individuals ages 23-27 (control group). The choice of 18-year-olds as the treatment group is based on two factors. The first factor is the number of post-reform years observed given the data limitations.[7] For 18-year-olds, three post-reform years are observed, while for e.g., 19-year-olds we only observe two. Second, 18-year-olds, are amongst the age groups with the highest prevalence of delinquency (see figure 2b in section 3). As outcomes are measured yearly in my estimation, using one of the peak ages of criminal behavior is likely to more accurately reflect the overall likelihood of being charged in early adulthood.

Two aspects have been the focus in the choice of control group. First, it is likely that age groups that are closer together have more similar criminal patterns, and it is therefore preferable to include ages as close to 18 as possible. Second, there may be birth cohort specific shocks that could affect the outcomes.[8] To reduce this risk, I therefore choose to include five ages/cohorts per year in the control group.

To formalize, I use the following difference-in-difference specification as my baseline esti-

[7] The last observed year is 2018.

[8] An example: The compulsory year of child care discussed in section 6 affected cohorts born from 1991. This implies that for 23-year-olds, outcomes may be affected from 2014, for 24-year-olds from 2015 etc. By including multiple cohorts yearly, I hope to reduce the risk of this bias.

mation:

$$Y_{it} = \beta_1 + \beta_2 Treat_i + \beta_3(Partly_t \times Treat_i) + \beta_4(Fully_t \times Treat_i) + \beta_5 X_i + \theta_t + \epsilon_{it} \quad (3)$$

where Y_{it} is the outcome for individual i in year t. $Treat_i$ is equal to one if the observation is for an 18-year-old. $Partly$ ($Fully$) is an indicator for years 2014-2015 (2016-2018). X_i is a set of control variables including gender, immigrant background, birth municipality, parents' education, year of birth and age at birth. θ_t are year fixed effects.

The coefficients of interest are β_3 and β_4, which is the mean difference in outcomes between 18- and 23- to 27-year-olds in 2014-2015 and 2016-2018, minus the equivalent for years 2010-2013. Since take-up of the benefit was not compulsory, estimates presented will be intention-to-treat (ITT).[9]

For this analysis to be credible, the common trend assumption needs to hold, implying the the trends in criminal behavior for 18- and 23- to 27-year-olds would have been similar if the cash-for-care reform had not been implemented. Since the outcomes are measured yearly, the likelihood of being charged will be lower then in the analysis of the previous two reforms, where we measure the outcomes over multiple years and ages. To assess the common trend assumption, I provide yearly DiD estimates in figure 7, which are further discussed in the following section.

My strategy is somewhat similar to Drange and Rege (2013), who compares outcomes for mothers having same-aged children that were or were not affected by the reform, to mothers of older children who were never affected. My analysis differs from the other previous research of this reform, where Rønsen (2001, 2009) compares the outcomes of mothers giving birth before the reform to mothers giving birth after as a main approach. Bettinger et al. (2014) compares the outcomes of children with siblings born after the reform, to those with siblings born before.

Results

In table 9, the difference-in-differences (DiD) estimates for the effect of cash-for-care on the likelihood of being charged and the number of criminal charges are presented, for the post-reform cohorts. The equivalent for the phase-in cohorts is presented in table C.3 in appendix C.

Panel A displays the results for the full sample. The likelihood of being charged in a given year increases by 0.1 percentage points without controls, and 0.4 percentage points with

[9]For cohorts born in 1998-2000, the average number of months with take-up was 18.2, which corresponds to 76 percent of the total possible months with take-up (24 months).

controls, as a result of the reform, from a pre-reform level of 4 percent. The results with controls, indicate an increase of approximately 10.5 percent from the pre-reform level of 18-year-olds. The number of charges is estimated to increase by 0.01 (0.02) without (with) controls, from a pre-reform mean of 0.07. The results are significant on the 1 percent level when including controls.

For men (panel B), the increases in the likelihood of being charged and the number of charges is even larger, with an increase of 1 percentage points in the likelihood of being charged and of 0.04 number of charges, when controls are included. For women, we see a small decrease in the likelihood of being charged by approximately 0.02 percentage points. Results for phase-in cohorts show an increase in the the number of charges in the full sample, but no significant effect on the likelihood of being charged. The increase is smaller than for the post cohorts.

In figure 7, I present the yearly DiD estimates for the likelihood of being charged and the number and charges for the full sample (panels A and B), men (panels C and D) and women (panels E and F). First, there is little evidence of pre-trends. In the full sample, the outcomes for the non-treated years are significantly different from the base year 2013.[10] The overall pattern for the full sample and for men, is a small increase for phase-in/partly treated years as well as for the first fully treated year, and then an increasing effect for 2017 and 2018. While this may be a result of increasing compliance, we need to have some concern that other factors might give rise to this pattern, and should therefore interpret results with some caution. For women, there are no clear effects to read out from these graphs.

In table 10, the DiD estimates for the effect of cash-for-care on the likelihood of being charged with different types of offenses are presented. In panel A, I show the results for the full sample, while the estimates for men (women) are presented in panel B (C).

In the full sample, we see an increase in offenses for profit, violent or sexual offenses, narcotics offenses, and traffic offenses, as a possible result of the reform. There is no significant effect on other offenses. For men, the results are similar, but also with a significant increase in other offenses. For women, we see marginally significant increases in offenses for profit and narcotics, as well as traffic offenses, while we see decreases in violent or sexual offenses and other offenses. These results for women should be interpreted with special caution, as very few women are charged with the different types of offenses.

[10]For the number of charges for men, 2010 is marginally significantly different from 2013, and for women the coefficient for likelihood of being charged in 2011 is significanttly different. While this of course is unwanted, there are no clear patterns in the pre-trends overall.

Table 11 displays the effects of cash-for-care by mothers' education divided in to three groups; lower than high school (low), high school and higher education (some university or college). Children of mothers' with low or high school education only display a significant increase in the likelihood of being charged by 0.3 and 0.5 percentage points (including controls), as well as an increase in the number of charges of approximately 0.02. For children of mothers with higher education, there are no significant effects on the likelihood of being charged, and an increase in the number of charges by approximately 0.007.

In table 12, I display the effects by fathers' income quartile. There is a significant increase in the likelihood of being charged for children of fathers' in income quartiles 1, 2 and 4 (when we include controls), and no significant effect for quartile 3. The effect on the number of charges is positive in all quartiles. Point estimates are larger in quartiles 1 and 2, which is consistent with the previous literature that children from lower SES backgrounds have larger benefits from attending formal child care.

In appendix C, I provide a couple of additional robustness checks. First, the effects of the reform on the likelihood of being charged at least 2, 5 and 10 times are presented, suggesting an increase in all levels for both the full sample and men, and an increase in the likelihood for more then 5 and 10 charges for women. Second, I provide estimates on the likelihood of being sentenced to different punishments, and find increases in prison sentences, probation and fines.

To summarize, the introduction of cash-for-care benefit increased delinquent behavior. While men are driving these results, there are some indications that the likelihood of being charged decreased for women. The increase in delinquency is visible in all types of offenses, and effects are larger for children from low SES backgrounds.

8 Conclusion

In this paper, I investigate whether formal child care can affect long-term criminal behavior. To overcome identification and data challenges, I use variation in formal child care use created by three different reforms, together with detailed register data including all criminal charges in Norway between 1992 and 2018. I find that child care enrollment can decrease youth or adult delinquency, an effect driven by men. There is no clear pattern in what type of offenses that are driving the results. Indicative results suggest that children from lower socio-economic backgrounds are affected to a larger extent, which is in line with the previous literature.

While the three reforms all affected enrollment in formal child care, they also had substantial differences. First, the target age groups vary from 1 to 6 years old. Second, the reforms affected child care enrollment differently; one through increased access, one by compulsory enrollment and one by de-incentivizing use of formal child care. They were therefore likely to have different complier groups. Finally, they were implemented at different points in time. Despite these differences, the effects of the reforms point in the same direction. Common factors such as the surrounding institutional setting, as well as the high quality of formal child care in Norway, are likely important contributors to the outcomes.

These findings can have important implications for policies focusing on crime prevention. My results suggest that early childhood interventions such as child care not only increases education and equalizes income, but can also be a tool in decreasing youth or adult delinquency. While policies aimed at young children might not have an immediate affect on crime, they can have long-term benefits which are important to account for in policy-makers' decisions.

My paper is the first to use multiple reforms connected to child care use together with the possibility to follow individuals over an extended period of time. This contributes to the previous literature by showing how different early childhood interventions can impact criminal behavior in the long-run. However, several questions remain. What mechanisms are driving the results? What child care policies are most efficient for prevention? Do the results from this paper transfer to other settings, e.g., where quality of child care is lower? These are interesting questions for future research.

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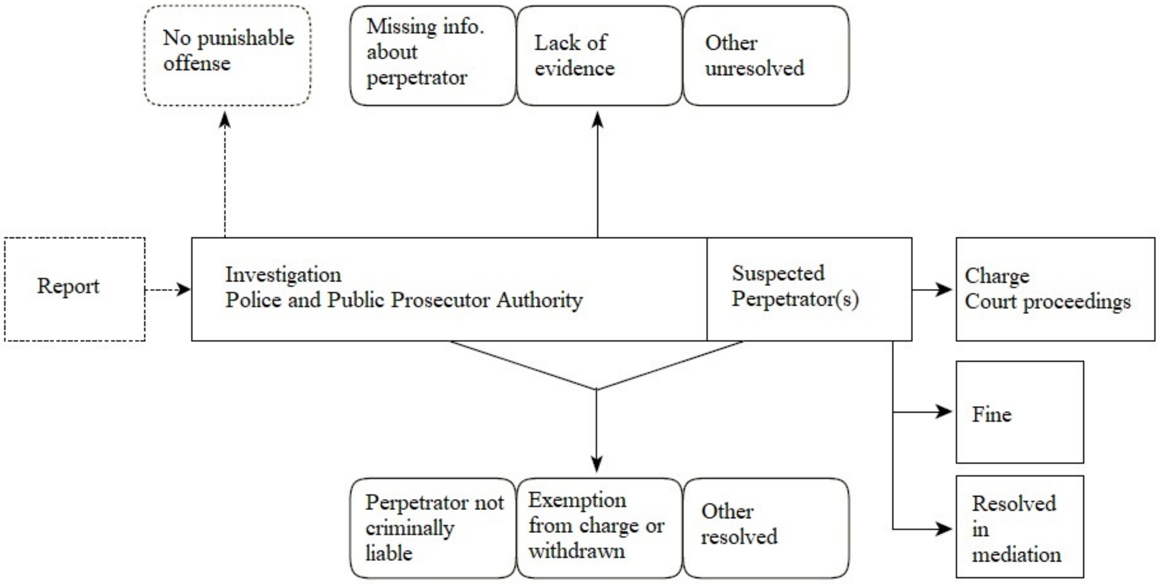
Figures

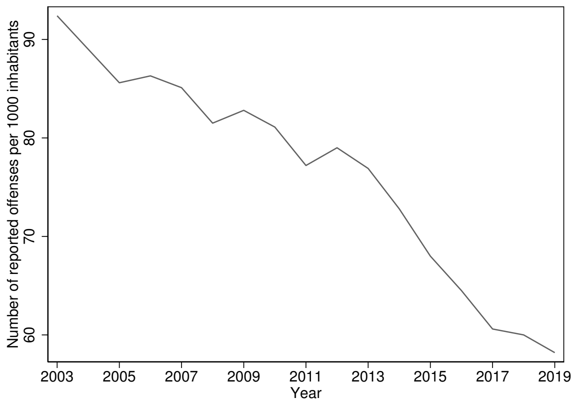
Crime in Norway

Figure 1: Criminal Offenses and Prosecution Decisions

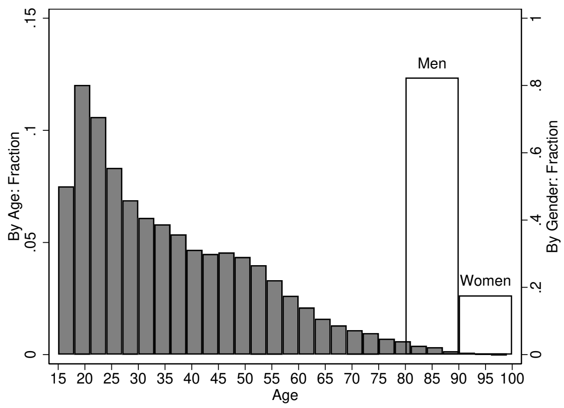


Source: Statistics Norway (2009)
Notes: This figure is an illustration of the criminal offenses and prosecution decisions in the Norwegian context.

Figure 2: Descriptive Figures, Crime in Norway



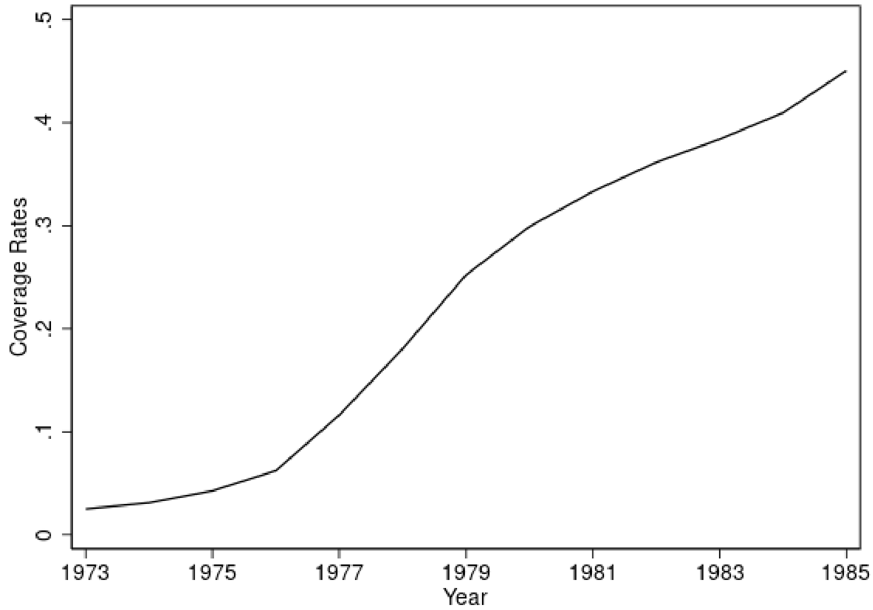
(A) Number of Reported Offenses per 1000 inhabitants



(B) Charged Individuals by Age and Gender

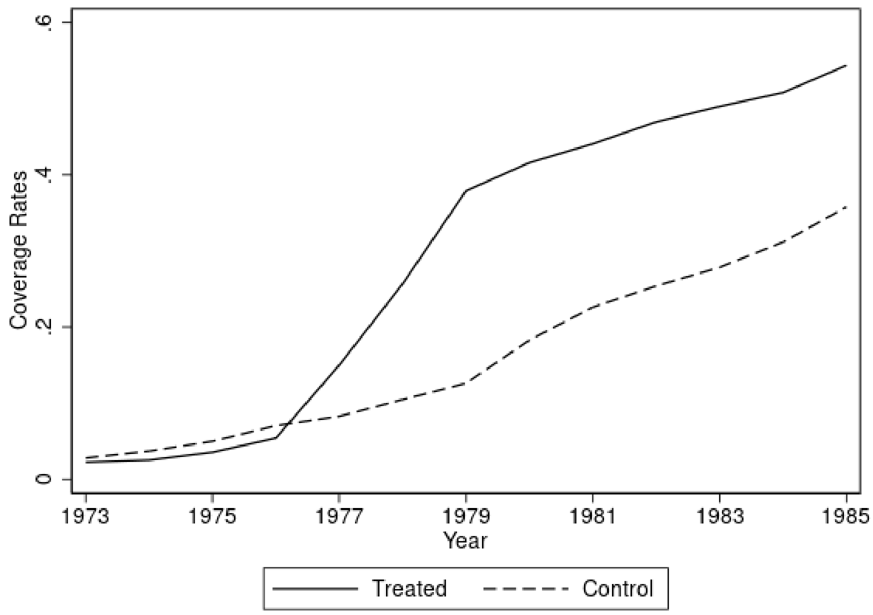
Notes: Panel (A) displays the number of reported offenses per 1000 inhabitants between 2003 and 2019. The data comes from official statistics from Statistics Norway. Panel B displays the age distribution and gender of charged individuals in 2018, based on own calculations of register data.

Figure 3: Average Child Care Coverage



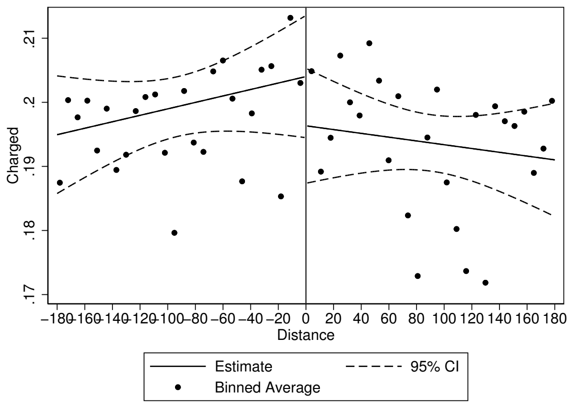
Notes: This figure displays the average municipal child care coverage rate for children ages 3-6 between 1973 and 1985.

Figure 4: Average Child Care Coverage, Treatment and Control Municipalities, 1973-1985

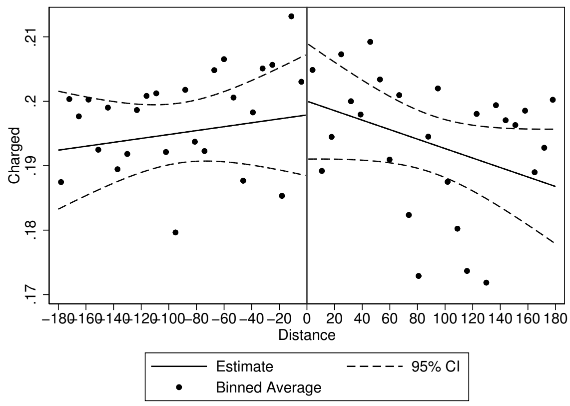


Notes: This figure displays the average municipal child care coverage rate for children ages 3-6 between 1973 and 1985 in treatment and control municipalities. Treatment (control) municipalities are defined as having an above (below) median increase in the child care coverage rate between 1976 and 1979.

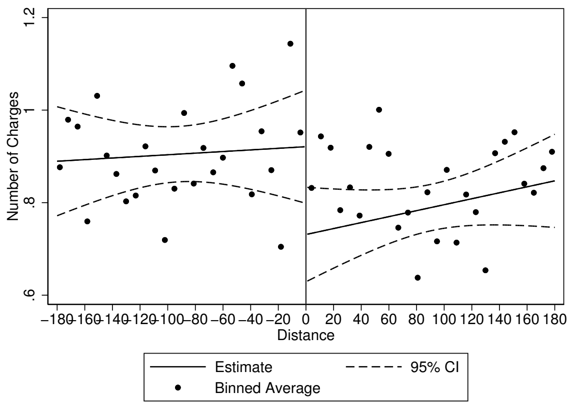
Figure 5: Regression Discontinuity Figures, Criminal Charges



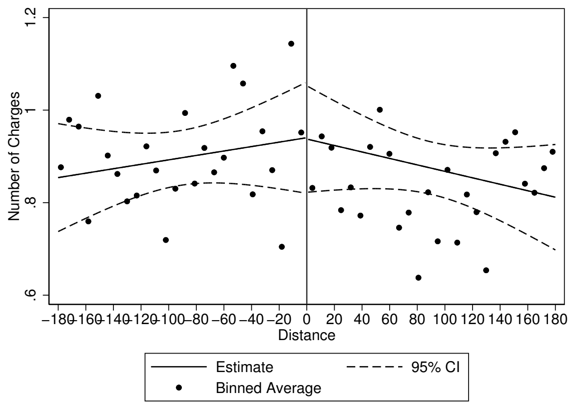
(A) Charged,
Cutoff: January 1, 1991



(B) Charged,
Cutoff: January 1, 1990



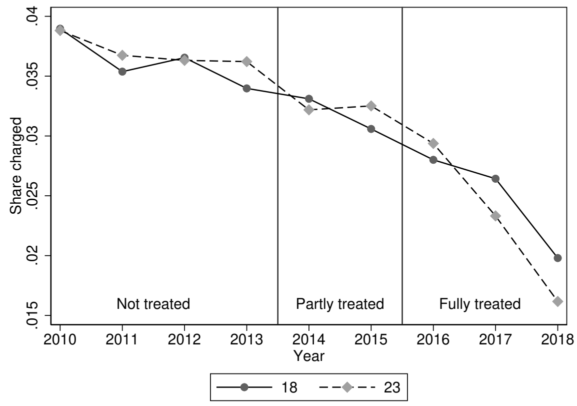
(C) Number of Charges,
Cutoff: January 1, 1991



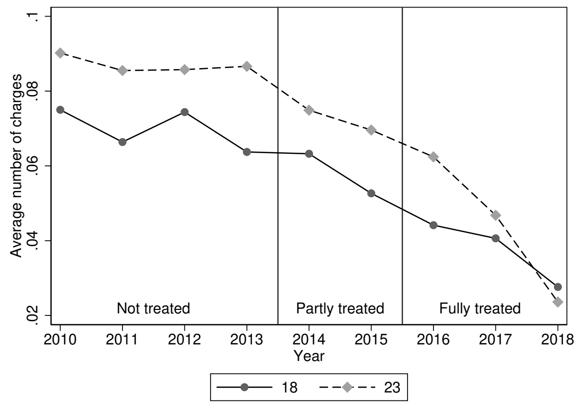
(D) Number of Charges,
Cutoff: January 1, 1990

Notes: This figure displays the regression discontinuity figures for the treatment group (born around January 1, 1991) for an indicator of being charged (panel (A)) and number of charges(panel (C)), and the equivalent for control group (born around January 1, 1990) in panels (b) and (d). I use linear trends on each side of the cutoffs, and a bandwidth of 180 days. Outcomes are measured between 2008 and 2018.

Figure 6: Criminal Charges, 18- vs 23-Year-Olds



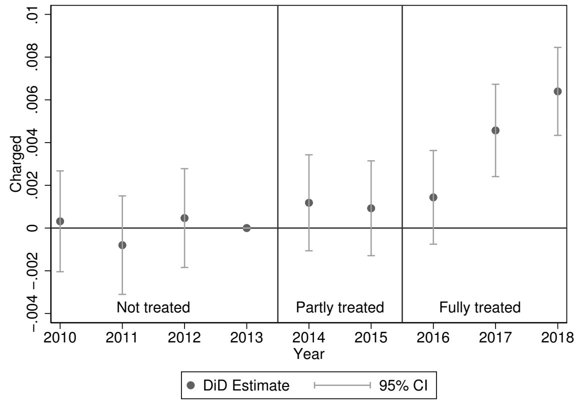
(A) Share Charged



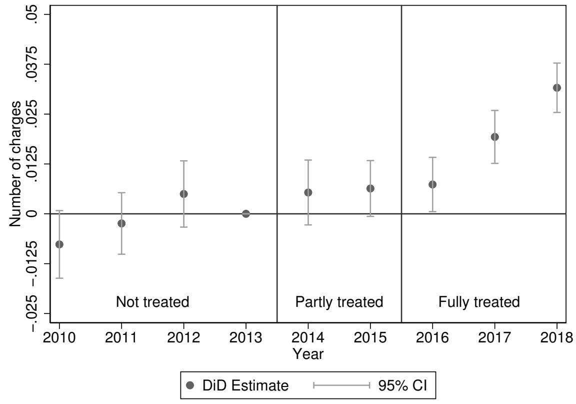
(B) Number of Charges

Notes: This figure is an illustration of the difference-in-differences strategy discussed in section 6. I display the share charged yearly in panel (A), and average yearly number of charges in panel (B), for 18- and 23-year-olds between 2010 and 2018.

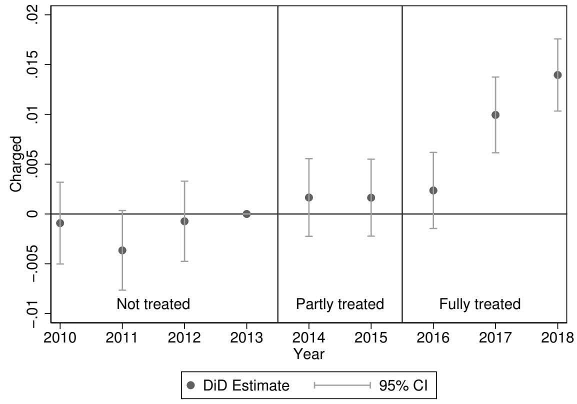
Figure 7: The Effects of Cash for Care on Criminal Charges, Yearly Difference-in-Differences Estimates, ITT, 2010-2018



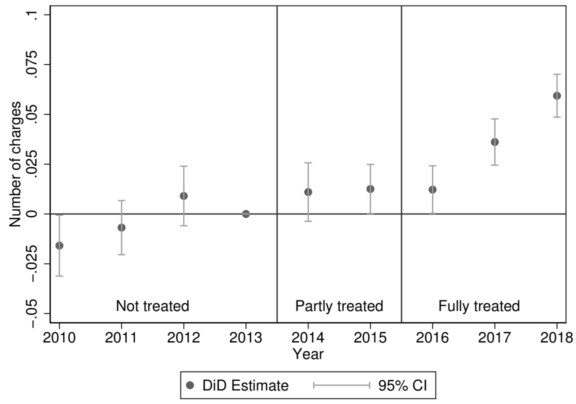
(A) All: Charged



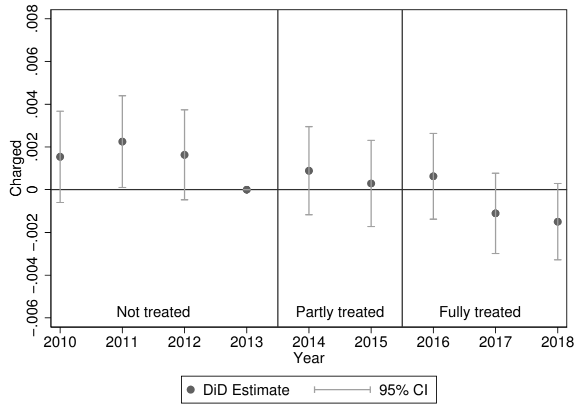
(B) All: Number of charges



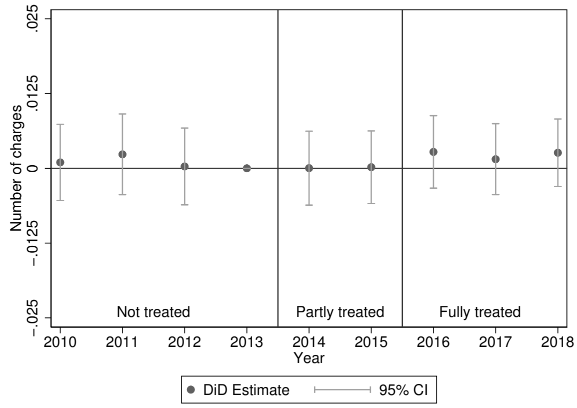
(C) Males: Charged



(D) Males: Number of charges



(E) Females: Charged



(F) Females: Number of charges

Notes: This figure displays the β_k coefficients along with 95% confidence intervals from the specification $Y_{it} = \beta_1 + \beta_2 Treat_i + \sum_{k=2010}^{2018} \beta_k (Treat_i \times Year_k) + \beta_4 X_i + \theta_t + \epsilon_{it}$, with 2012 as the base year (see section 7 for more information). The DiD estimator is the difference in mean between 18-year-olds and 23-27-year-olds in the given year, relative to the same difference in 2013. Estimates are intention-to-treat (ITT). The outcomes are an indicator for being charged in a specific year, and the yearly total number of charges. Robust standard errors are used to calculate confidence intervals.

Tables

Introduction of Subsidized Child Care

Table 1: Effects of Child Care Coverage on Criminal Charges, ITT

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: All				
DiD	-0.0085	-0.0075*	-0.0482	-0.0382
	(0.0052)	(0.0045)	(0.0747)	(0.0698)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.18	0.18	0.96	0.96
Observations	561039	561039	561039	561039
Panel B: Males				
DiD	-0.0167**	-0.0148*	-0.0982	-0.0855
	(0.0083)	(0.0076)	(0.1450)	(0.1430)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.28	0.28	1.65	1.65
Observations	285694	285694	285694	285694
Panel C: Females				
DiD	-0.0014	-0.0004	-0.0073	-0.0006
	(0.0029)	(0.0028)	(0.0297)	(0.0303)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.07	0.07	0.24	0.24
Observations	275345	275345	275345	275345

Notes: This table displays the β_3 coefficient from equation (1). The DiD estimate is the difference in mean outcomes between post-reform (born in 1973-1976) and pre-reform cohorts (born in 1967-1969) in treated municipalities, relative to the same difference in control municipalities. Results for phase-in cohorts (born in 1970-1973) from the same estimations are showed in table A.6. Estimates are intention-to-treat. Outcomes are measured between 1992 and 2006. Columns (1) and (2) shows the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 2: Effects of Child Care Coverage on Criminal Charges, Type of Offenses, ITT

	Offenses for profit	Violent or sex. offenses	Narcotics offenses	Traffic offenses	Other offenses
	(1)	(2)	(3)	(4)	(5)
Panel A: All					
DiD	0.0006	0.0007	0.0009	-0.0084*	-0.0052*
	(0.0017)	(0.0013)	(0.0013)	(0.0044)	(0.0029)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.05	0.03	0.02	0.11	0.07
Observations	561039	561039	561039	561039	561039
Panel B: Males					
DiD	-0.0008	0.0015	0.0019	-0.0161**	-0.0086
	(0.0031)	(0.0026)	(0.0023)	(0.0074)	(0.0053)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.07	0.05	0.04	0.18	0.12
Observations	285694	285694	285694	285694	285694
Panel C: Females					
DiD	0.0020	-0.0004	-0.0003	-0.0009	-0.0019
	(0.0013)	(0.0007)	(0.0010)	(0.0022)	(0.0012)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.02	0.00	0.01	0.04	0.02
Observations	275345	275345	275345	275345	275345

Notes: This table displays the β_3 coefficient from equation (1). The DiD estimate is the difference in mean outcomes between post-reform (born in 1973-1976) and pre-reform cohorts (born in 1967-1969) in treated municipalities, relative to the same difference in control municipalities. Estimates are intention-to-treat. The outcomes are indicators for being charged with a specific type of offense at any time between 1992 and 2006. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 3: Effects of Child Care Coverage on Criminal Charges, ITT, Heterogeneity by Mothers' Education

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: Less than High School				
DiD	-0.0073	-0.0079	0.0350	0.0313
	(0.0067)	(0.0060)	(0.1543)	(0.1490)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.20	0.20	1.25	1.25
Observations	207847	207847	207847	207847
Panel B: High School or More				
DiD	-0.0077	-0.0061	-0.0621	-0.0594
	(0.0051)	(0.0042)	(0.0602)	(0.0556)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.15	0.15	0.73	0.73
Observations	353192	353192	353192	353192

Notes: This table displays the β_3 coefficient from equation (1), separately for each level of maternal education. The DiD estimate is the difference in mean outcomes between post-reform (born in 1973-1976) and pre-reform cohorts (born in 1967-1969) in treated municipalities, relative to the same difference in control municipalities. Estimates are intention-to-treat. Outcomes are measured between 1992 and 2006. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 4: Effects of Child Care Coverage on Criminal Charges, ITT, Heterogeneity by Fathers' Income

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: 1st Quartile				
DiD	-0.0158	-0.0135	-0.2679	-0.2328
	(0.0106)	(0.0083)	(0.1969)	(0.1737)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.20	0.20	1.23	1.23
Observations	135807	135807	135807	135807
Panel B: 2nd Quartile				
DiD	-0.0043	-0.0033	-0.0490	-0.0367
	(0.0063)	(0.0062)	(0.0896)	(0.0931)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.17	0.17	0.88	0.88
Observations	138139	138139	138139	138139
Panel C: 3rd Quartile				
DiD	-0.0116	-0.0107	0.0476	0.0520
	(0.0079)	(0.0066)	(0.0987)	(0.0927)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.16	0.16	0.88	0.88
Observations	136542	136542	136542	136542
Panel D: 4th Quartile				
DiD	-0.0074	-0.0058	-0.0473	-0.0445
	(0.0058)	(0.0055)	(0.0851)	(0.0880)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.15	0.15	0.67	0.67
Observations	135509	135509	135509	135509

Notes: This table displays the β_3 coefficient from equation (1), estimated separately for each paternal income quartile. The DiD estimate is the difference in mean outcomes between post-reform (born in 1973-1976) and pre-reform cohorts (born in 1967-1969) in treated municipalities, relative to the same difference in control municipalities. Estimates are intention-to-treat. Outcomes are measured between 1992 and 2006. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Compulsory Child Care for Six-Year-Olds

Table 5: Effects of Compulsory Child Care for Six-Year-Olds on Criminal Charges

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: All				
DiRD	-0.0059	-0.0053	-0.1233**	-0.1140*
	(0.0055)	(0.0052)	(0.0625)	(0.0606)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.19	0.19	0.89	0.89
Observations	117152	117152	117152	117152
Panel B: Males				
DiRD	-0.0138	-0.0110	-0.2886**	-0.2561**
	(0.0089)	(0.0086)	(0.1150)	(0.1129)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.30	0.30	1.49	1.49
Observations	60152	60152	60152	60152
Panel C: Females				
DiRD	0.0013	0.0012	0.0448	0.0510
	(0.0052)	(0.0051)	(0.0427)	(0.0470)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.08	0.08	0.25	0.25
Observations	57000	57000	57000	57000

Notes: This table displays the β_5 coefficient from equation (2), using linear trends, 180 days bandwidth and triangular weights (see section 6 for more information). The DiRD estimate is the difference in mean outcomes between those born just after December 31, 1990, relative to the same difference in the previous year. Outcomes are measured between 2008 and 2018. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 6: Effects of Compulsory Child Care for Six-Year-Olds on Criminal Charges, Type of Offense

	Offenses for profit	Violent or sex. offenses	Narcotics offenses	Traffic offenses	Other offenses
	(1)	(2)	(3)	(4)	(5)
Panel A: All					
DiRD	-0.0031	-0.0040	-0.0075**	0.0036	-0.0120***
	(0.0021)	(0.0025)	(0.0032)	(0.0037)	(0.0038)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.04	0.04	0.06	0.10	0.08
Observations	117152	117152	117152	117152	117152
Panel B: Males					
DiRD	-0.0074**	-0.0086**	-0.0160***	0.0061	-0.0174**
	(0.0035)	(0.0044)	(0.0057)	(0.0064)	(0.0067)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.05	0.06	0.09	0.16	0.14
Observations	60152	60152	60152	60152	60152
Panel C: Females					
DiRD	0.0017	0.0015	0.0018	0.0010	-0.0061*
	(0.0026)	(0.0019)	(0.0024)	(0.0031)	(0.0032)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.02	0.01	0.02	0.03	0.03
Observations	57000	57000	57000	57000	57000

Notes: This table displays the β_5 coefficient from equation (2), using linear trends, 180 days bandwidth and triangular weights (see section 6 for more information). The DiRD estimate is the difference in mean outcomes between those born just after December 31, 1990, relative to the same difference in the previous year. Outcomes are measured between 2008 and 2018. The outcomes are indicators for being charged with a specific type of offense at any time between 2008 and 2018. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 7: Effects of Compulsory Child Care for Six-Year-Olds on Criminal Charges, Heterogeneity by Mothers' Education

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: Less than High School				
DiRD	-0.0102	-0.0105	-0.0250	-0.0531
	(0.0118)	(0.0104)	(0.1412)	(0.1405)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.25	0.25	1.44	1.44
Observations	36936	36936	36936	36936
Panel B: High School				
DiRD	-0.0048	-0.0032	-0.2349***	-0.2177***
	(0.0078)	(0.0077)	(0.0763)	(0.0746)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.19	0.19	0.73	0.73
Observations	48230	48230	48230	48230
Panel C: Higher Education				
DiRD	0.0055	0.0033	-0.0340	-0.0387
	(0.0096)	(0.0093)	(0.0860)	(0.0833)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.12	0.12	0.38	0.38
Observations	30510	30510	30510	30510

Notes: This table displays the β_5 coefficient from equation (2), separately for each level of mothers' education. I use linear trends, 180 days bandwidth and triangular weights (see section 6 for more information). The DiRD estimate is the difference in mean outcomes between those born just after December 31, 1990, relative to the same difference in the previous year. Outcomes are measured between 2008 and 2018. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 8: Effects of Compulsory Child Care for Six-Year-Olds on Criminal Charges, Heterogeneity by Fathers' Income

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: Quartile 1				
DiRD	-0.0173	-0.0120	-0.3533*	-0.2985
	(0.0115)	(0.0113)	(0.1890)	(0.1929)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.25	0.25	1.43	1.43
Observations	28815	28815	28815	28815
Panel B: Quartile 2				
DiRD	-0.0023	-0.0038	-0.0787	-0.1193
	(0.0102)	(0.0099)	(0.1207)	(0.1156)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.20	0.20	0.87	0.87
Observations	28630	28630	28630	28630
Panel C: Quartile 3				
DiRD	0.0022	0.0006	0.1339	0.1623
	(0.0109)	(0.0106)	(0.1063)	(0.1059)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.17	0.17	0.64	0.64
Observations	28777	28777	28777	28777
Panel D: Quartile 4				
DiRD	-0.0054	-0.0055	-0.1717**	-0.1411**
	(0.0096)	(0.0095)	(0.0734)	(0.0694)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.15	0.15	0.48	0.48
Observations	28819	28819	28819	28819

Notes: This table displays the β_5 coefficient from equation (2), separately for each quartile of fathers' income. I use linear trends, 180 days bandwidth and triangular weights (see section 6 for more information). The DiRD estimate is the difference in mean outcomes between those born just after December 31, 1990, relative to the same difference in the previous year. Outcomes are measured between 2008 and 2018. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 9: Effects of Cash-for-Care on Criminal Charges, ITT

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: All				
DiD	0.0010*	0.0042***	0.0082***	0.0205***
	(0.0006)	(0.0006)	(0.0018)	(0.0020)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.04	0.04	0.07	0.07
Observations	2943581	2943581	2943581	2943581
Panel B: Males				
DiD	0.0049***	0.0100***	0.0172***	0.0388***
	(0.0010)	(0.0011)	(0.0032)	(0.0036)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.06	0.06	0.11	0.11
Observations	1514156	1514156	1514156	1514156
Panel C: Females				
DiD	-0.0028***	-0.0019***	-0.0007	0.0015
	(0.0005)	(0.0006)	(0.0013)	(0.0014)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.02	0.02	0.02	0.02
Observations	1429425	1429425	1429425	1429425

Notes: This table displays the β_4 coefficient from equation (3). The DiD estimate is the difference in mean outcomes in between 18-year-olds and 23-27-year-olds in 2016-2018, relative to the same difference in 2010-2013. Results for phase-in years 2014-2015 from the same estimations are showed in table C.3. Estimates are intention-to-treat. Outcomes are measured yearly. Columns (1) and (2) show the likelihood of being charged in given year, and columns (3) and (4) the yearly total number of charges. Standard errors are robust and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 10: Effects of Cash-for-Care on Criminal Charges, ITT, Type of Offense

	Offenses for profit	Violent or sex. offenses	Narcotics offenses	Traffic offenses	Other offenses
	(1)	(2)	(3)	(4)	(5)
Panel A: All					
DiD	0.0014***	0.0005**	0.0038***	0.0021***	0.0002
	(0.0002)	(0.0002)	(0.0003)	(0.0004)	(0.0003)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, 18-year-olds	0.01	0.01	0.01	0.01	0.01
Observations	2943581	2943581	2943581	2943581	2943581
Panel B: Males					
DiD	0.0022***	0.0014***	0.0070***	0.0036***	0.0029***
	(0.0004)	(0.0004)	(0.0006)	(0.0007)	(0.0006)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, 18-year-olds	0.01	0.01	0.01	0.02	0.02
Observations	1514156	1514156	1514156	1514156	1514156
Panel C: Females					
DiD	0.0005*	-0.0004*	0.0005*	0.0006**	-0.0026***
	(0.0003)	(0.0002)	(0.0003)	(0.0003)	(0.0003)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, 18-year-olds	0.00	0.00	0.00	0.00	0.01
Observations	1429425	1429425	1429425	1429425	1429425

Notes: This table displays the β_4 coefficient from equation (3). The DiD estimate is the difference in mean outcomes in between 18-year-olds and 23-27-year-olds in 2016-2018, relative to the same difference in 2010-2013. Estimates are intention-to-treat. The outcomes are indicators for being charged with a specific type of offense in a given year. Standard errors are robust and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 11: Effects of Cash-for-Care on Criminal Charges, ITT, Heterogeneity by Mothers' Education

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: Lower than High School				
DiD	0.0010	0.0030**	0.0073	0.0196***
	(0.0014)	(0.0015)	(0.0046)	(0.0048)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.05	0.05	0.12	0.12
Observations	935894	935894	935894	935894
Panel B: High School				
DiD	0.0029***	0.0049***	0.0087***	0.0160***
	(0.0009)	(0.0009)	(0.0025)	(0.0030)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.03	0.03	0.06	0.06
Observations	1213490	1213490	1213490	1213490
Panel C: Higher Education				
DiD	-0.0001	0.0009	0.0052***	0.0066***
	(0.0008)	(0.0009)	(0.0017)	(0.0020)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.02	0.02	0.03	0.03
Observations	758044	758044	758044	758044

Notes: This table displays the β_4 coefficient from equation (3), estimated separately for each level of mothers' education. The DiD estimate is the difference in mean outcomes in between 18-year-olds and 23-27-year-olds in 2016-2018, relative to the same difference in 2010-2013. Estimates are intention-to-treat. Outcomes are measured yearly. Columns (1) and (2) show the likelihood of being charged in given year, and columns (3) and (4) the yearly total number of charges. Standard errors are robust and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 12: Effects of Cash-for-Care on Criminal Charges, ITT, Heterogeneity by Fathers' Income

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: Quartile 1				
DiD	-0.0003	0.0056***	0.0046	0.0272***
	(0.0014)	(0.0015)	(0.0050)	(0.0051)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.05	0.05	0.11	0.11
Observations	720458	720458	720458	720458
Panel B: Quartile 2				
DiD	0.0011	0.0050***	0.0111***	0.0233***
	(0.0012)	(0.0013)	(0.0035)	(0.0039)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.04	0.04	0.07	0.07
Observations	733062	733062	733062	733062
Panel C: Quartile 3				
DiD	0.0004	0.0004	0.0071**	0.0114***
	(0.0011)	(0.0012)	(0.0029)	(0.0038)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.03	0.03	0.05	0.05
Observations	731816	731816	731816	731816
Panel D: Quartile 4				
DiD	0.0026***	0.0039***	0.0085***	0.0107***
	(0.0010)	(0.0011)	(0.0024)	(0.0029)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.02	0.02	0.04	0.04
Observations	725632	725632	725632	725632

Notes: This table displays the β_4 coefficient from equation (3), estimated separately for each quartile of fathers' income. The DiD estimate is the difference in mean outcomes in between 18-year-olds and 23-27-year-olds in 2016-2018, relative to the same difference in 2010-2013. Estimates are intention-to-treat. Outcomes are measured yearly. Columns (1) and (2) show the likelihood of being charged in given year, and columns (3) and (4) the yearly total number of charges. Standard errors are robust and reported in parentheses.

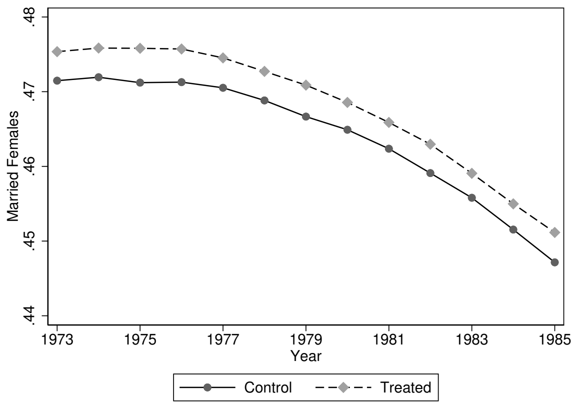
$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Appendices

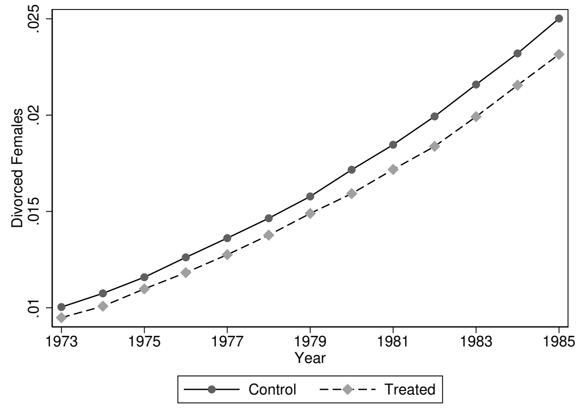
A Introduction of Formal Child Care

A.1 Figures

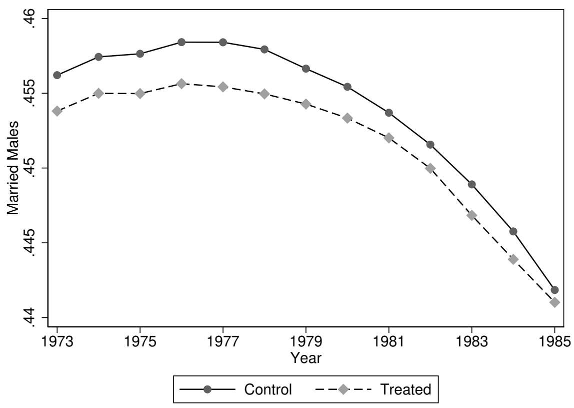
Figure A.1: Municipality Characteristics 1973-1985 1



(a) Married Females

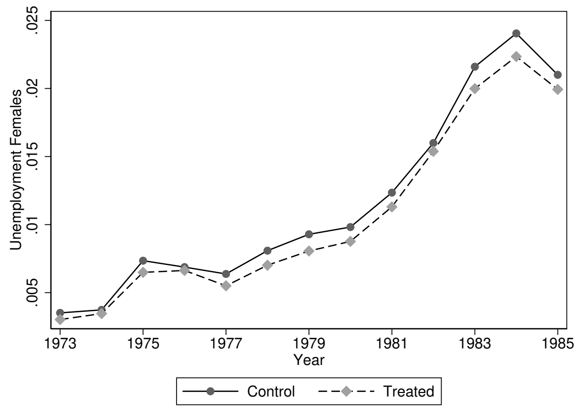


(b) Divorced Females

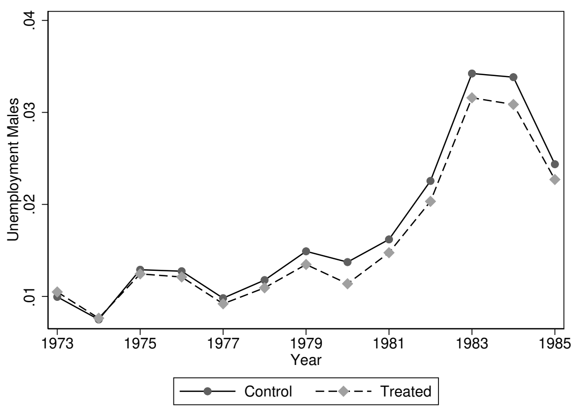


(c) Married Males

(d) Divorced Males



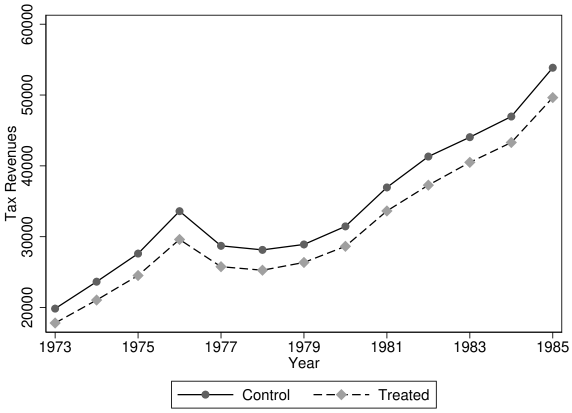
(e) Unemployed Females



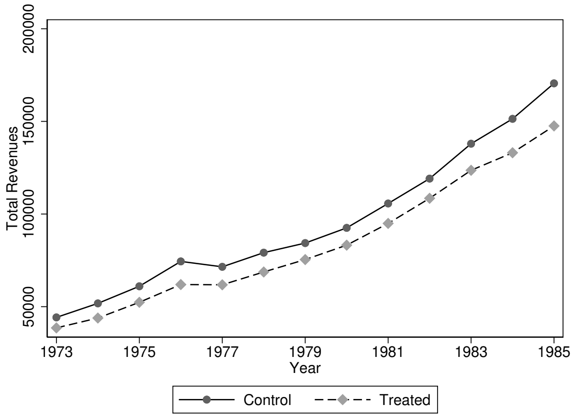
(f) Unemployed Males

Notes: These figures display trends in municipality characteristics in 1973-1975 for treatment and control municipalities (see section 5 for definition). Data source: Norwegian Centre for Research Data (NSD).

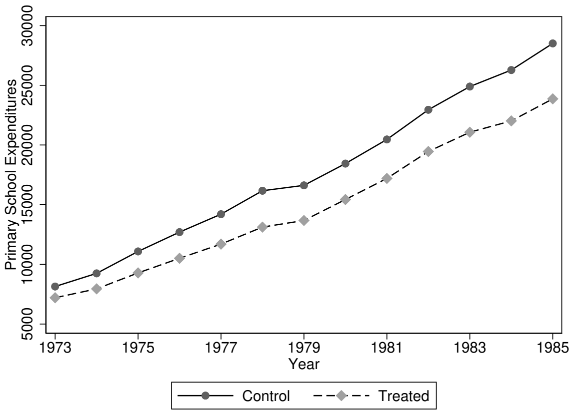
Figure A.2: Municipality Characteristics 1973-1985 2



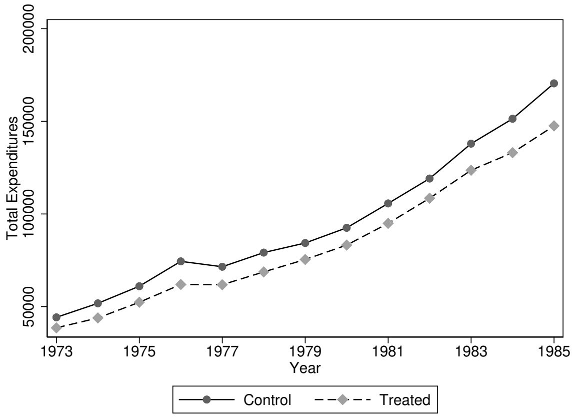
(a) Tax Revenues



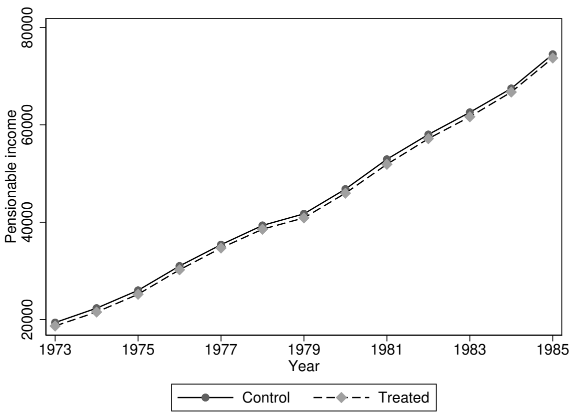
(b) Total Revenues



(c) Primary School Expenditures



(d) Total Expenditures

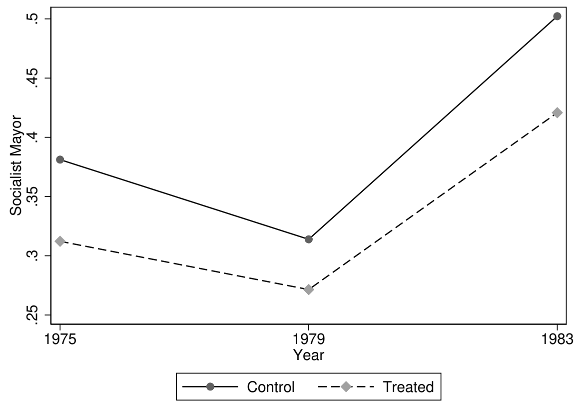


(e) Pensionable Income

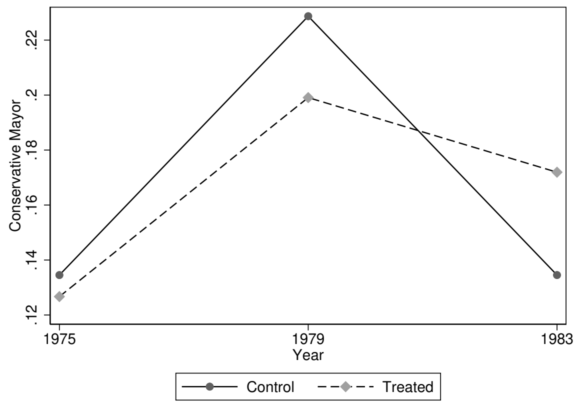
(f) Higher Education

Notes: These figures display trends in municipality characteristics in 1973-1975 for treatment and control municipalities (see section 5 for definition). Data source: Norwegian Centre for Research Data (NSD) (Panels A-D) and Norwegian Administrative Registers (Panels E-F).

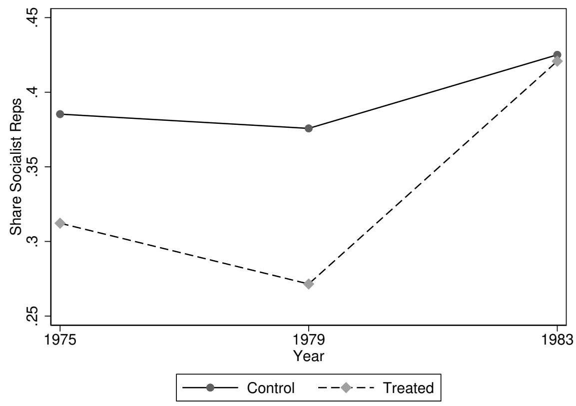
Figure A.3: Municipality Characteristics 1973-1985 3



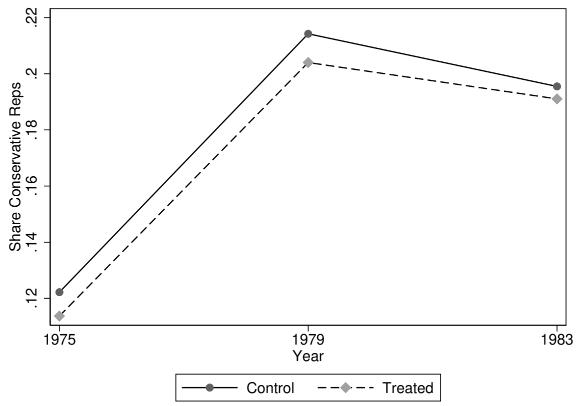
(a) Socialist Mayor



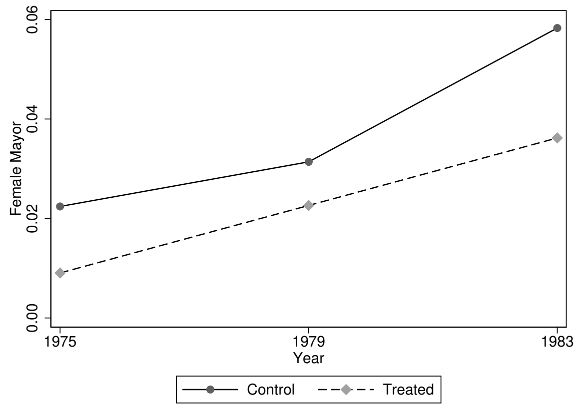
(b) Conservative Mayor



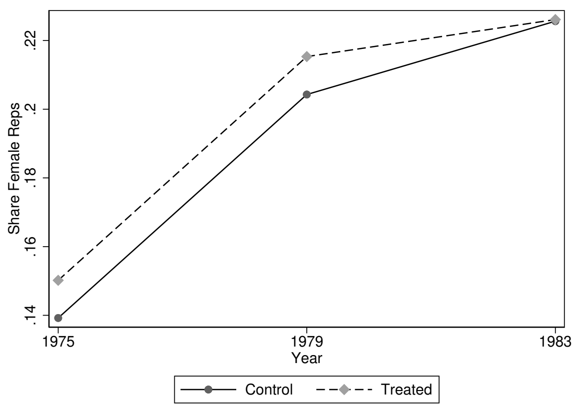
(c) Socialist Share of Elected Representatives



(d) Conservative Share of Elected Representatives



(e) Female Mayor



(f) Female Share of Elected Representatives

Notes: These figures display trends in municipality characteristics in 1973-1975 for treatment and control municipalities (see section 5 for definition). Data source: Norwegian Centre for Research Data (NSD).

A.2 Tables

Table A.1: Effects of Child Care Coverage on Criminal Charges, ITT, Treatment Intensity

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: All				
DiD	-0.0002	-0.0002	-0.0020	-0.0015
	(0.0001)	(0.0001)	(0.0018)	(0.0018)
Controls	No	Yes	No	Yes
Pre-reform mean	0.17	0.17	0.96	0.96
Observations	561039	561039	561039	561039
Panel B: Males				
DiD	-0.0004*	-0.0004*	-0.0042	-0.0038
	(0.0002)	(0.0002)	(0.0035)	(0.0036)
Controls	No	Yes	No	Yes
Pre-reform mean	0.28	0.28	1.63	1.63
Observations	285694	285694	285694	285694
Panel C: Females				
DiD	0.0000	0.0000	0.0001	0.0003
	(0.0001)	(0.0001)	(0.0010)	(0.0010)
Controls	No	Yes	No	Yes
Pre-reform mean	0.07	0.07	0.26	0.26
Observations	275345	275345	275345	275345

Notes: This table displays the β_3 coefficient from equation $Y_{ijt} = \beta_1 + \beta_2(Phase{-}in_t \times Treat_j) + \beta_3(Post_t \times Treat_j) + \beta_4 X_i + \theta_j + \gamma_t + \varepsilon_{ijt}$, where $Treat_j$ is the increase in the municipal child care coverage rate, and the other factors are explain in section (5). The DiD estimate is the difference in mean outcomes between post-reform (born in 1973-1976) and pre-reform cohorts (born in 1967-1969) in the same municipality, relative to the equivalent in municipalities with other treatment intensities. Estimates are intention-to-treat. Outcomes are measured between 1992 and 2006. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

TABLE A.2: Effects of Child Care Coverage on Criminal Charges, ITT, Tertile Treatment

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: All				
DiD Medium	-0.0000	0.0009	0.0033	0.0142
	(0.0064)	(0.0056)	(0.0794)	(0.0733)
DiD High	-0.0039	-0.0028	-0.0343	-0.0228
	(0.0052)	(0.0048)	(0.0799)	(0.0804)
Controls	No	Yes	No	Yes
Pre-reform mean control	0.18	0.18	0.98	0.98
Observations	561039	561039	561039	561039
Panel B: Males				
DiD Medium	-0.0004	0.0014	0.0296	0.0424
	(0.0103)	(0.0094)	(0.1530)	(0.1476)
DiD High	-0.0094	-0.0093	-0.1000	-0.1011
	(0.0080)	(0.0077)	(0.1505)	(0.1534)
Controls	No	Yes	No	Yes
Pre-reform mean control	0.28	0.28	1.67	1.67
Observations	285694	285694	285694	285694
Panel C: Females				
DiD Medium	-0.0004	0.0005	-0.0286	-0.0217
	(0.0032)	(0.0031)	(0.0283)	(0.0291)
DiD High	0.0013	0.0025	0.0329	0.0394
	(0.0036)	(0.0036)	(0.0440)	(0.0445)
Controls	No	Yes	No	Yes
Pre-reform mean control	0.07	0.07	0.26	0.26
Observations	275345	275345	275345	275345

Notes: This table displays the $\beta_3 1$ and $\beta_3 2$ coefficients from equation $Y_{ijt} = \beta_1 + \beta_2 1(Phase - in_t \times TreatMedium_j) + \beta_2 2(Phase - in_t \times TreatHigh_j) + \beta_3 1(Post_t \times TreatMedium_j) + \beta_3 1(Post_t \times TreatHigh_j) + \beta_4 X_i + \theta_j + \gamma_t + \varepsilon_{ijt}$, where $TreatMedium_j$ ($TreatHigh_j$) is in the middle (highest) tertile of the increase in the municipal child care coverage rate, and the other factors are explain in section (5). The DiD Medium (High) estimate is the difference in mean outcomes between post-reform (born in 1973-1976) and pre-reform cohorts (born in 1967-1969) in the middle (highest) treatment tertile, relative to the equivalent in municipalities in the lowest treatment tertile. Estimates are intention-to-treat. Outcomes are measured between 1992 and 2006. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table A.3: Effects of Child Care Coverage on Number of Charges, ITT

	≥ 2	≥ 5	≥ 10
	(1)	(2)	(3)
Panel A: All			
DiD	-0.0051*	-0.0017	0.0012
	(0.0028)	(0.0017)	(0.0011)
Controls	Yes	Yes	Yes
Pre-reform mean, control	0.08	0.03	0.02
Observations	561039	561039	561039
Panel B: Males			
DiD	-0.0106**	-0.0038	0.0016
	(0.0052)	(0.0033)	(0.0020)
Controls	Yes	Yes	Yes
Pre-reform mean, control	0.14	0.06	0.03
Observations	285694	285694	285694
Panel C: Females			
DiD	0.0002	0.0004	0.0007
	(0.0015)	(0.0008)	(0.0006)
Controls	Yes	Yes	Yes
Pre-reform mean, control	0.02	0.01	0.00
Observations	275345	275345	275345

Notes: This table displays the β_3 coefficient from equation (1). The DiD estimate is the difference in mean outcomes between post-reform (born in 1973-1976) and pre-reform cohorts (born in 1967-1969) in treated municipalities, relative to the same difference in control municipalities. Estimates are intention-to-treat. The outcomes are indicators for being charged at least 2, 5 and 10 times at any time between 1992 and 2006. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table A.4: Effects of Child Care Coverage on Criminal Charges, ITT, Ages 25-30

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: All				
DiD	-0.0020	-0.0014	-0.0175	-0.0123
	(0.0030)	(0.0026)	(0.0344)	(0.0322)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.10	0.10	0.40	0.40
Observations	561039	561039	561039	561039
Panel B: Males				
DiD	-0.0049	-0.0039	-0.0244	-0.0176
	(0.0054)	(0.0050)	(0.0666)	(0.0649)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.17	0.17	0.69	0.69
Observations	285694	285694	285694	285694
Panel C: Females				
DiD	0.0003	0.0008	-0.0155	-0.0123
	(0.0016)	(0.0016)	(0.0156)	(0.0159)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.03	0.03	0.09	0.09
Observations	275345	275345	275345	275345

Notes: This table displays the β_3 coefficient from equation (1). The DiD estimate is the difference in mean outcomes between post-reform (born in 1973-1976) and pre-reform cohorts (born in 1967-1969) in treated municipalities, relative to the same difference in control municipalities. Estimates are intention-to-treat. Outcomes are measured at ages 25-30. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table A.5: Effects of Child Care Coverage on Punishments, ITT

	Prison	Probation	Community service	Fine	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: All					
DiD	-0.0009	-0.0000	-0.0000	-0.0103	0.0000
	(0.0013)	(0.0013)	(0.0005)	(0.0067)	(0.0001)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.02	0.02	0.00	0.41	0.00
Observations	561039	561039	561039	561039	561039
Panel B: Males					
DiD	-0.0022	-0.0004	0.0002	-0.0204**	0.0001
	(0.0024)	(0.0026)	(0.0009)	(0.0081)	(0.0001)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.04	0.04	0.01	0.56	0.00
Observations	285694	285694	285694	285694	285694
Panel C: Females					
DiD	0.0004	0.0003	-0.0003	-0.0002	0.0000
	(0.0006)	(0.0008)	(0.0004)	(0.0065)	(0.0000)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.01	0.01	0.00	0.25	0.00
Observations	275345	275345	275345	275345	275345

Notes: This table displays the β_3 coefficient from equation (1). The DiD estimate is the difference in mean outcomes between post-reform (born in 1973-1976) and pre-reform cohorts (born in 1967-1969) in treated municipalities, relative to the same difference in control municipalities. Estimates are intention-to-treat. The outcomes are indicators for being convicted to a certain type punishment at any time between 1992 and 2006. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table A.6: Effects of Child Care Coverage on Criminal Charges, ITT, Phase-In Cohorts

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: All				
DiD phase-in	0.0006	-0.0002	0.1128	0.1051
	(0.0036)	(0.0034)	(0.0731)	(0.0707)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.18	0.18	0.96	0.96
Observations	561039	561039	561039	561039
Panel B: Males				
DiD phase-in	-0.0059	-0.0059	0.1770	0.1663
	(0.0061)	(0.0059)	(0.1392)	(0.1353)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.28	0.28	1.65	1.65
Observations	285694	285694	285694	285694
Panel C: Females				
DiD phase-in	0.0054**	0.0056**	0.0266	0.0284
	(0.0026)	(0.0026)	(0.0291)	(0.0295)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.07	0.07	0.24	0.24
Observations	275345	275345	275345	275345

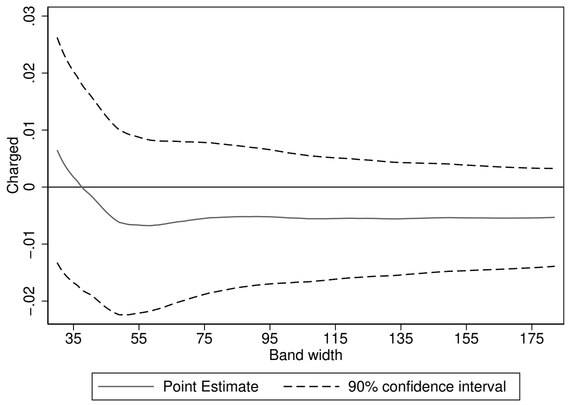
Notes: This table displays the β_2 coefficient from equation (1). The DiD estimate is the difference in mean outcomes between phase-in (born in 1973-1976) and pre-reform cohorts (born in 1967-1969) in treated municipalities, relative to the same difference in control municipalities. Results for post-reform cohorts (born in 1970-1973) from the same estimations are showed in table 1. Estimates are intention-to-treat. Outcomes are measured between 1992 and 2006. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

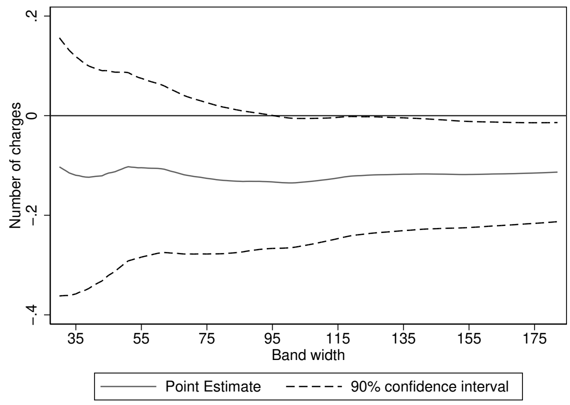
B Compulsory year of Child Care for Six-Year-Olds

B.1 Figures

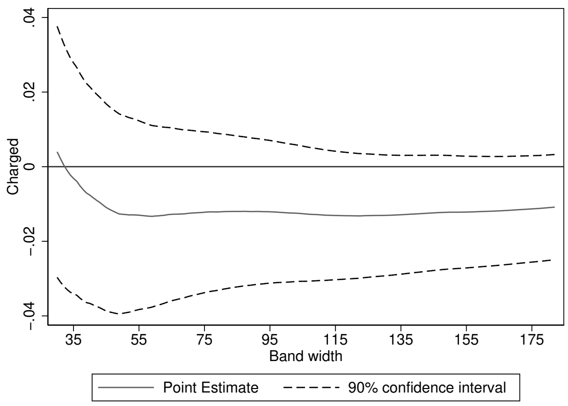
Figure B.1: Effects of Compulsory Year of Child Care on Criminal charges, Main Specification with Different Bandwidths



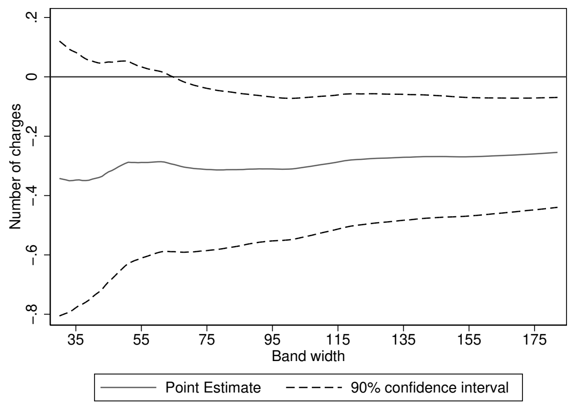
(a) All: Charged



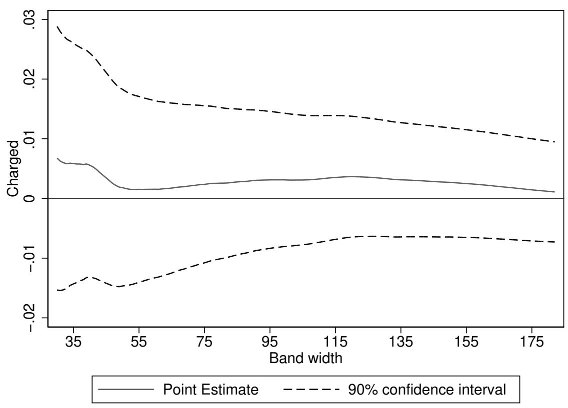
(b) All: Number of charges



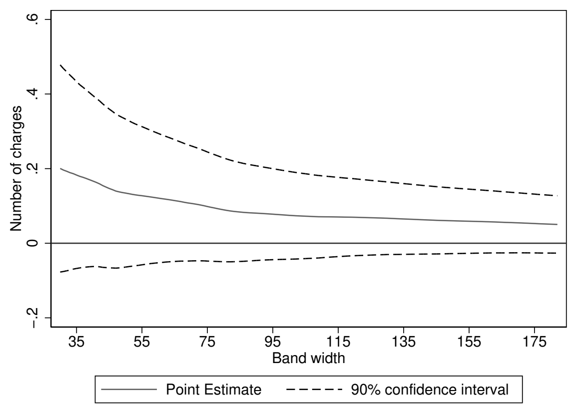
(c) Males: Charged



(d) Males: Number of charges



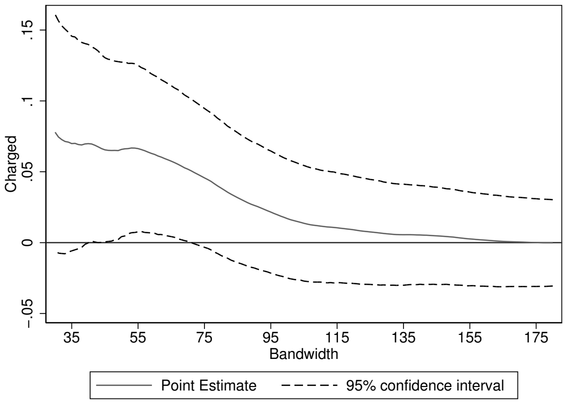
(e) Females: Charged



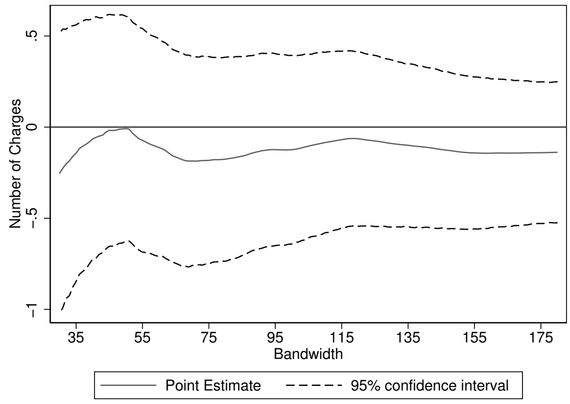
(f) Females: Number of charges

Notes: These figures display the β_5 coefficients from equation 2 along with 95% confidence intervals, using bandwidths between 30 and 180 days, together with linear trends, separate on each side of the cut-off but equal for individuals born in different year on the same side of the cutoff, and triangular weights. The difference-in-regression-discontinuity (DiRD) estimates is the estimated difference in mean outcomes between those born just after December 31, 1990 and those born just before, relative to the same difference in the previous year. Outcomes are measured between 2008 and 2018. Panels (A), (C) and (E) show the likelihood of being charged during this time period, and panels (B), (D) and (E) the total number of charges in this time period. Standard errors are clustered on the municipality level.

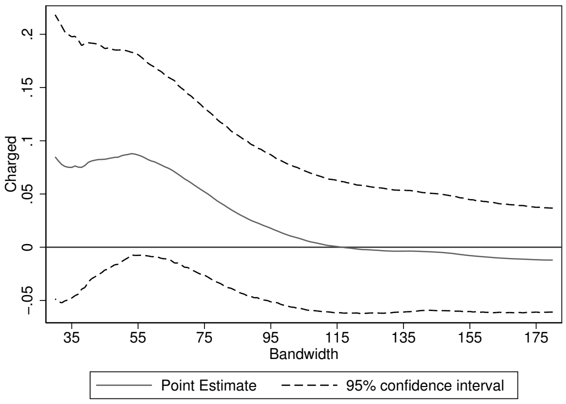
Figure B.3: Effects of Compulsory Year of Child Care on Criminal Charges, Local Linear Regression, Different Bandwidths



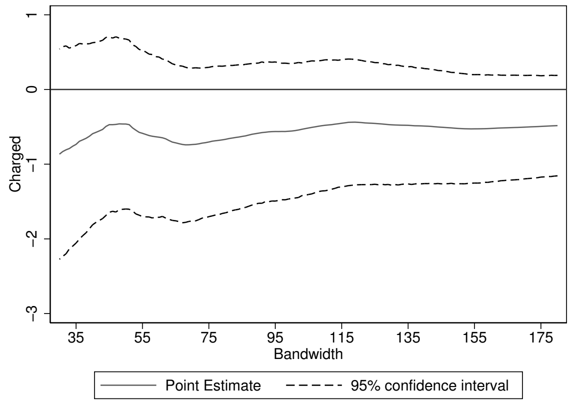
(A) All: Charged



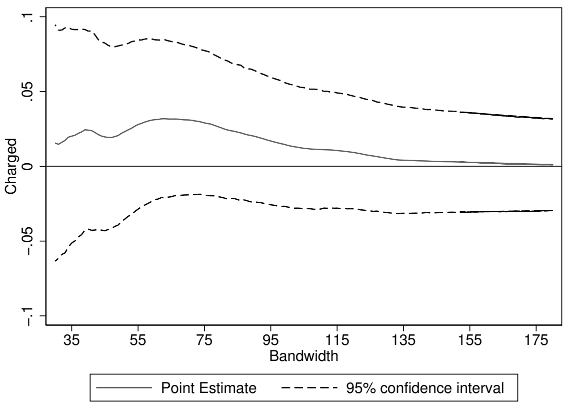
(B) All: Number of charges



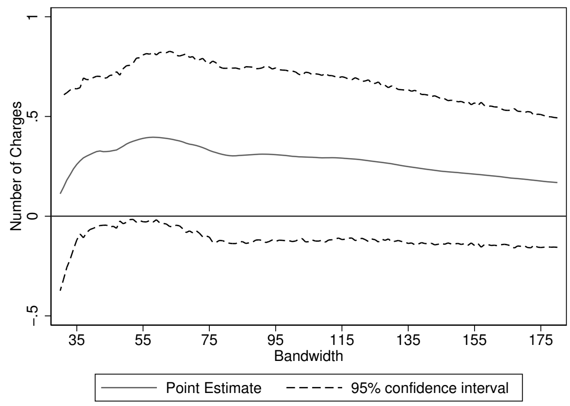
(C) Males: Charged



(D) Males: Number of charges



(E) Females: Charged



(F) Females: Number of charges

Notes: These figures display the β_5 coefficients from equation 2 along with 95% confidence intervals, using bandwidths between 30 and 180 days and local linear regression with triangular weights. The difference-in-regression-discontinuity (DiRD) estimates are the estimated difference in mean outcomes between those born just after December 31, 1990 and those born just before, relative to the same difference in the previous year. Outcomes are measured between 2008 and 2018. Panel (A) shows the likelihood of being charged during this time period, and panel (B) the total number of charges in this time period. Standard errors are robust and calculated in line with Calonico et al. (2014).

B.2 Tables

Table B.1: Effects of Compulsory Child Care for Six-Year-Olds on Number of Charges

	≥ 2	≥ 5	≥ 10
	(1)	(2)	(3)
Panel A: All			
DiRD	-0.0077**	-0.0040	-0.0011
	(0.0039)	(0.0025)	(0.0019)
Controls	Yes	Yes	Yes
Pre-reform mean, control	0.10	0.04	0.02
Observations	117152	117152	117152
Panel B: Males			
DiRD	-0.0120*	-0.0075	-0.0034
	(0.0068)	(0.0047)	(0.0032)
Controls	Yes	Yes	Yes
Pre-reform mean, control	0.17	0.07	0.03
Observations	60152	60152	60152
Panel C: Females			
DiRD	-0.0027	0.0002	0.0020*
	(0.0032)	(0.0017)	(0.0012)
Controls	Yes	Yes	Yes
Pre-reform mean, control	0.03	0.01	0.00
Observations	57000	57000	57000

Notes: This table displays the β_5 coefficient from equation (2), using linear trends, 180 days bandwidth and triangular weights (see section 6 for more information). The DiRD estimate is the difference in mean outcomes between those born just after December 31, 1990, relative to the same difference in the previous year. The outcomes are indicators for being charged at least 2, 5 and 10 times at any time between 2008 and 2018. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table B.2: Effects of Compulsory Child Care for Six-Year-Olds on Punishments

	Prison	Probation	Community service	Fine	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: All					
DiRD	-0.0001	-0.0022	-0.0030**	-0.0081	0.0002
	(0.0025)	(0.0025)	(0.0014)	(0.0058)	(0.0001)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.03	0.03	0.02	0.39	0.00
Observations	117152	117152	117152	117152	117152
Panel B: Males					
DiRD	-0.0019	-0.0046	-0.0052*	-0.0157*	0.0001
	(0.0045)	(0.0043)	(0.0027)	(0.0081)	(0.0002)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.06	0.05	0.02	0.52	0.00
Observations	60152	60152	60152	60152	60152
Panel C: Females					
DiRD	0.0025	0.0002	-0.0004	-0.0011	0.0002*
	(0.0015)	(0.0018)	(0.0014)	(0.0078)	(0.0001)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, control	0.01	0.01	0.00	0.24	0.00
Observations	57000	57000	57000	57000	57000

Notes: This table displays the β_5 coefficient from equation (2), using linear trends, 180 days bandwidth and triangular weights (see section 6 for more information). The DiRD estimate is the difference in mean outcomes between those born just after December 31, 1990, relative to the same difference in the previous year. Outcomes are measured between 2008 and 2018. The outcomes are indicators for being convicted to a specific type of punishment at any time between 2008 and 2018. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table B.3: Effects of Compulsory Child Care for Six-Year-Olds on Criminal Charges, Different Trends, All

	Linear	Squared		Cubed	
	(1)	(2)	(3)	(4)	(5)
Panel A: Charged					
DiRD	-0.0060	-0.0053	-0.0065	-0.0053	-0.0068
	(0.0095)	(0.0052)	(0.0072)	(0.0052)	(0.0093)
Differencing trends	Yes	No	Yes	No	Yes
Observations	117152	117152	117152	117152	117152
Panel B: Number of charges					
DiRD	-0.1439	-0.1140*	-0.1332	-0.1138*	-0.1357
	(0.1099)	(0.0606)	(0.0836)	(0.0607)	(0.1091)
Differencing trends	Yes	No	Yes	No	Yes
Observations	117152	117152	117152	117152	117152

Notes: This table displays the β_5 coefficient from equation (2) for the full sample, using 180 days bandwidth and triangular weights (see section 6 for more information). In column (1) linear trends are included, in columns (2) and (3) quadratic trends and in columns (4) and (5) cubic trends. In columns (2) and (4), trends are equal for both the treatment and control group on each side of the cutoff, while in columns (1), (3) and (5) trends are allowed to differ between the treatment and control group. The DiRD estimate is the difference in mean outcomes between those born just after December 31, 1990, relative to the same difference in the previous year. Outcomes are measured between 2008 and 2018. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table B.4: Effects of Compulsory Child Care for Six-Year-Olds on Criminal Charges, Different Trends, Males

	Linear	Squared		Cubed	
	(1)	(2)	(3)	(4)	(5)
Panel A: Charged					
DiRD	-0.0176	-0.0110	-0.0169	-0.0110	-0.0150
	(0.0156)	(0.0086)	(0.0117)	(0.0086)	(0.0154)
Differencing trends	Yes	No	Yes	No	Yes
Observations	60152	60152	60152	60152	60152
Panel B: Number of charges					
DiRD	-0.3679*	-0.2562**	-0.3127**	-0.2558**	-0.3407*
	(0.2006)	(0.1129)	(0.1561)	(0.1131)	(0.1970)
Differencing trends	Yes	No	Yes	No	Yes
Observations	60152	60152	60152	60152	60152

Notes: This table displays the β_5 coefficient from the equation (2) for males, using 180 days bandwidth and triangular weights (see section 6 for more information). In column (1) linear trends are included, in columns (2) and (3) quadratic trends and in columns (4) and (5) cubic trends. In columns (2) and (4), trends are equal for both the treatment and control group on each side of the cutoff, while in columns (1), (3) and (5) trends are allowed to differ between the treatment and control group. The DiRD estimate is the difference in mean outcomes between those born just after 31 December 1990, relative to the same difference in the previous year. Outcomes are measured between 2008 and 2018. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table B.5: Effects of Compulsory Child Care for Six-Year-Olds on Criminal Charges, Different Trends, Females

	Linear	Squared		Cubed	
	(1)	(2)	(3)	(4)	(5)
Panel A: Charged					
DiRD	0.0067	0.0012	0.0052	0.0012	0.0017
	(0.0096)	(0.0051)	(0.0071)	(0.0051)	(0.0092)
Differencing Trends	Yes	No	Yes	No	Yes
Observations	57000	57000	57000	57000	57000
Panel B: Number of charges					
DiRD	0.1108	0.0511	0.0757	0.0511	0.0932
	(0.0995)	(0.0470)	(0.0692)	(0.0469)	(0.0984)
Differencing trends	Yes	No	Yes	No	Yes
Observations	57000	57000	57000	57000	57000

Notes: This table displays the β_5 coefficient from the equation (2) for females, using 180 days bandwidth and triangular weights (see section 6 for more information). In column (1) linear trends are included, in columns (2) and (3) quadratic trends and in columns (4) and (5) cubic trends. In columns (2) and (4), trends are equal for both the treatment and control group on each side of the cutoff, while in columns (1), (3) and (5) trends are allowed to differ between the treatment and control group. The DiRD estimate is the difference in mean outcomes between those born just after December 31, 1990, relative to the same difference in the previous year. Outcomes are measured between 2008 and 2018. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

TABLE B.6: Effects of Compulsory Child Care for Six-Year-Olds on Criminal Charges, Ages 18-25

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: All				
DiRD	-0.0208***	-0.0204***	-0.2008***	-0.1928***
	(0.0053)	(0.0051)	(0.0541)	(0.0529)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.18	0.18	0.73	0.73
Observations	117152	117152	117152	117152
Panel B: Males				
DiRD	-0.0293***	-0.0266***	-0.4000***	-0.3713***
	(0.0085)	(0.0082)	(0.0990)	(0.0977)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.27	0.27	1.22	1.22
Observations	60152	60152	60152	60152
Panel C: Females				
DiRD	-0.0129***	-0.0130***	0.0043	0.0106
	(0.0048)	(0.0048)	(0.0404)	(0.0448)
Controls	No	Yes	No	Yes
Pre-reform mean, control	0.07	0.07	0.21	0.21
Observations	57000	57000	57000	57000

Notes: This table displays the β_5 coefficient from equation (2), using linear trends, 180 days bandwidth and triangular weights (see section 6 for more information). The DiRD estimate is the difference in mean outcomes between those born just after December 31, 1990, relative to the same difference in the previous year. Outcomes are measured between ages 18-25. Columns (1) and (2) show the likelihood of being charged during this time period, and columns (3) and (4) the total number of charges in this time period. Standard errors are clustered on the municipality level and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

C Cash-for-Care

C.1 Tables

Table C.1: Effects of Cash-for-Care on Number of Charges, ITT

	≥ 2	≥ 5	≥ 10
	(1)	(2)	(3)
Panel A: All			
DiD	0.0024***	0.0015***	0.0006***
	(0.0004)	(0.0002)	(0.0001)
Controls	Yes	Yes	Yes
Pre-reform mean, 18-year-olds	0.01	0.00	0.00
Observations	2943581	2943581	2943581
Panel B: Males			
DiD	0.0049***	0.0025***	0.0011***
	(0.0006)	(0.0003)	(0.0001)
Controls	Yes	Yes	Yes
Pre-reform mean, 18-year-olds	0.02	0.00	0.00
Observations	1514156	1514156	1514156
Panel C: Females			
DiD	-0.0001	0.0005***	0.0002***
	(0.0003)	(0.0001)	(0.0001)
Controls	Yes	Yes	Yes
Pre-reform mean, 18-year-olds	0.00	0.00	0.00
Observations	1429425	1429425	1429425

Notes: This table displays the β_4 coefficient from equation (3), estimated separately for each quartile of fathers' income. The DiD estimate is the difference in mean outcomes in between 18-year-olds and 23-27-year-olds in 2016-2018, relative to the same difference in 2010-2013. Estimates are intention-to-treat. The outcomes are indicators for being charged at least 2, 5 and 10 times in a given year. Standard errors are robust and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table C.2: Effects of Cash-for-Care on Punishments, ITT

	Prison	Probation	Community service	Fine	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: All					
DiD	0.0023***	0.0005***	-0.0004***	0.0133***	-0.0000
	(0.0001)	(0.0002)	(0.0001)	(0.0007)	(0.0000)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, 18-year-olds	0.00	0.00	0.00	0.04	0.00
Observations	2943581	2943581	2943581	2943581	2943581
Panel B: Males					
DiD	0.0041***	0.0008***	-0.0006**	0.0219***	-0.0000
	(0.0002)	(0.0003)	(0.0002)	(0.0012)	(0.0000)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, 18-year-olds	0.00	0.00	0.00	0.06	0.00
Observations	1514156	1514156	1514156	1514156	1514156
Panel C: Females					
DiD	0.0003***	0.0002	-0.0002**	0.0043***	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0007)	(0.0000)
Controls	Yes	Yes	Yes	Yes	Yes
Pre-reform mean, 18-year-olds	0.00	0.00	0.00	0.02	0.00
Observations	1429425	1429425	1429425	1429425	1429425

Notes: This table displays the β_4 coefficient from equation (3). The DiD estimate is the difference in mean outcomes in between 18-year-olds and 23-27-year-olds in 2016-2018, relative to the same difference in 2010-2013. Estimates are intention-to-treat. The outcomes are indicators for being sentenced to a specific type of punishment in a given year. Standard errors are robust and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table C.3: Effects of Cash-for-Care on Criminal Charges, ITT, Phase-in

	Charged		Number of charges	
	(1)	(2)	(3)	(4)
Panel A: All				
DiD Phase-in	-0.0005	0.0010	0.0010	0.0070***
	(0.0007)	(0.0007)	(0.0024)	(0.0025)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.04	0.04	0.07	0.07
Observations	2943581	2943581	2943581	2943581
Panel B: Males				
DiD Phase-in	0.0002	0.0029**	0.0038	0.0146***
	(0.0012)	(0.0012)	(0.0044)	(0.0045)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.06	0.06	0.11	0.11
Observations	1514156	1514156	1514156	1514156
Panel C: Females				
DiD Phase-in	-0.0012*	-0.0007	-0.0018	-0.0007
	(0.0007)	(0.0007)	(0.0016)	(0.0016)
Controls	No	Yes	No	Yes
Pre-reform mean, 18-year-olds	0.02	0.02	0.02	0.02
Observations	1429425	1429425	1429425	1429425

Notes: This table displays the β_3 coefficient from equation (3). The DiD estimate is the difference in mean outcomes in between 18-year-olds and 23-27-year-olds in 2014-2015, relative to the same difference in 2010-2013. Results for post years 2016-2018 from the same estimations are shown in table 9. Estimates are intention-to-treat. Outcomes are measured yearly. Columns (1) and (2) show the likelihood of being charged in given year, and columns (3) and (4) the yearly total number of charges. Standard errors are robust and reported in parentheses.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$