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Welfare Reform: Consequences for the Children

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Abstract

This paper uses register-based data to analyze the consequences of a recent major Danish welfare reform for child human capital and well-being. In addition to work requirements, the reform introduced an upper limit on welfare benefits. The upper limit on welfare benefits was estimated to reduce disposable income for welfare recipients by between five and 20 percent depending on family type. We implement a comparative event study that contrasts individuals on welfare at the time of reform announcement before and after the implementation of the reform with the development in outcomes for an uncontaminated comparison group, namely those on welfare exactly one year prior. Our analysis documents that mothers' propensity to receive welfare only decreased slightly as a consequence of the reform, just as we only observe a small increase in hours worked. We find a similar increase in hours worked among the oldest children too. We then show small but negative effects on children's school well-being, as measured by individual-level nationally administered well-being surveys and small increases in absence from school because of the reform. Short-run child academic performance, in contrast, was not affected.

1. Introduction

Over the last few decades, many governments – including the Anglo-Saxon and the Nordic welfare states – have reformed welfare systems with the purpose of promoting work (Grogger and Karoly, 2005; Mogstad and Prozanto, 2012), for example by introducing work requirements backed up by sanctions or by providing financial work incentives. Concerns are raised, however, that while welfare reforms do incentivize some to work, others may simply experience an income loss. Moreover, it has long been recognized (e.g., Holmes and Rahe, 1967; Milligan and Stabile, 2011) that both parents and children tend to find shocks to the family's financial status particularly stressful. As such, welfare reform could have broader implications and affect family members, most prominently children, too. The direction of the effects on child outcomes is ex ante unclear: several papers document that higher income levels (e.g., Dahl and Lochner, 2012; Aizer et al., 2016; Akee et al., 2018) positively affect children's emotional and behavioral health as well as later life human capital outcomes and that early stressors (Almond and Currie, 2010) work in the opposite direction. At the same time, it has proven difficult to establish a causal link between parental (maternal) employment and child outcomes. The empirical evidence on this aspect is non-uniform and varies considerably with age of the child as well as socio-economic background (e.g., Berger et al., 2005; Ruhm, 2004; Ruhm, 2008).

This paper uses population-wide register data informative about parents' welfare participation coupled with a range of additional parental and child outcomes across several domains to study a welfare reform that introduced both an upper limit on welfare benefits as well as work requirements. Our starting point is a recent major Danish reform that was passed into law in March 2016 and implemented in October 2016. The reform imposed a substantial change to the policy at that time: if work requirements were not fulfilled, welfare recipients could lose all or part of their benefits. At the same time, the upper limit on welfare benefits was estimated to reduce disposable income for welfare recipients by between five and 20 percent depending on family type (The Danish Ministry of Employment, 2015). The reform not only represented a drastic change, it was also wide-ranging: roughly, 170,000 individuals received welfare benefits at the time of the announcement and more than 30,000 children subsequently experienced a reduction in their families' welfare benefits (Statistics Denmark, 2017).

Uncovering effects of various welfare reforms on child outcomes has been challenged by a lack of consistent data on relevant child outcomes across age, time, and geographic space; by access to small samples and issues with survey attrition; and by the occurrence of simultaneous reforms. Relative to

the existing literature in the area, we utilize high-frequent panel data on a rich palette of outcomes (including measures of well-being) in a comparative event study that exploits the exact timing of the reform. Specifically, to avoid issues with anticipation we start by selecting the group of mothers on welfare in the month at which the reform was passed into law. Our strategy then compares outcomes for individuals in the reform group in a given period with their own outcomes immediately before the reform was passed into law with the corresponding development in outcomes for the group of individuals on welfare exactly one year prior. This allows us to speak to the effects of the reform for group of children whose mothers were on welfare at the time of the reform. The key advantages to this approach is that it balances out within-calendar year dynamics in welfare participation. Zeroing in on variation surrounding the timing of the reform also minimizes the role of other, concurrent factors.

To illustrate the immediate workings of the reform, Figure 1 shows receipt of benefits,[1] for the population of mothers on welfare benefits in March 2016 and compares this to the population of mothers on welfare in March 2015. The benefit levels of the two populations track each other closely from January until October. In October, in contrast, we observe a sudden decrease of more than 20 percentage points in the share of individuals who receive this type of support.

Figure 1

Receipt of benefits, DKK

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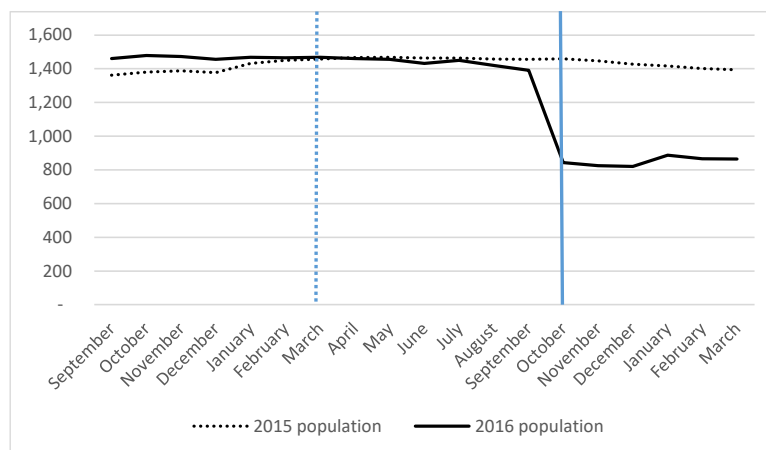
[1] In practice, the reform affected welfare benefits related to shelter and supplements. See Section 2 for further details.

Our analysis documents that mothers' propensity to receive welfare only decreased slightly as a consequence of the reform, just as we only observe a small increase in hours worked. We find a similar increase in hours worked among the oldest children too. We then show small but negative effects on children's self-reported school well-being, as measured by individual-level nationally administered well-being surveys and small increases in absence from school because of the reform. We also document uptics in reports to municipal social services for children exposed to the reform. Reports to social services were primarily due to child externalizing behaviors, insufficient care by parents, or high levels of conflict in the family. This particular outcome should, however, be interpreted with caution; because the data source is relatively new, we are unable to explore differences in pre-trends. Short-run child academic performance, in contrast, was not affectedOur strategy relies, among other things, on the absence of other concurrent changes. Having first confirmed that the any effects on the inflow into welfare were miniscule, we therefore investigate trends in outcomes in the non-welfare population. Importantly, we find no evidence of deviations in trends in connection with the reform. Lastly, we explore whether results are driven by other mechanisms such as changes in help-seeking behaviors (visits to primary care physicians, psychologists or psychiatrists and ER visits) and school switching and find no evidence of this.

Because of the nature of the effects on maternal outcomes, the consequences for child outcomes are likely a combination of lower access to resources useful for child development (the "resources" channel) and worse emotional well-being (the "family process" channel); see Mayer (1997), Yeung et al. (2002), and Milligan and Stabile (2011). Our large samples naturally allow for a range of subgroup analyses that to some extent can speak to mechanisms behind our findings. We find, for example, that effects on child wellbeing are larger (more detrimental) for children if the mother had a very low attachment to the labor market (<10 hours of work) in the 12 months prior to the reform. At the very least, this group of mothers would have to exercise considerable effort to avoid any sanctions associated with the hours requirement and certainly for them to leave welfare entirely. Effects are also larger for children who live in less stable families (single-parent or non-married households).

Our paper relates directly to a literature concerned with the link between the tightening of welfare services and child human capital development. To the best of our knowledge, most of these are based

on the 1990s US welfare reforms that introduced a combination of time limits on welfare receipt, job subsidies, and work requirements. At the same time, this period also saw considerable expansions of the Earned Income Tax Credit that has been shown to independently affect adult labor supply (e.g. Eissa and Hoynes, 2004).[2] Some of the US welfare reforms did undergo experimental evaluation but data on child outcomes were not universally available and there is evidence of both positive and negative effects on child well-being; see Grogger and Karoly (2005) for an overview. Miller and Zhang (2012) were the first to measure the impact of the 1990s welfare reforms in the US on the educational attainment of male and female children in low-income families using large, nationally representative samples. To estimate net effects of the reforms they use versions of a difference-in-differences strategy that compares children of low-income parents with children of higher income parents before and after the reforms. Their results show that income gaps in school enrollment and drop-out rates narrowed by more than 20 percent as a consequence of the reforms. Experimental results from the Canadian Self-Sufficiency project that offered an earnings supplement with full time work, on the other hand, show mixed results that vary with child age (Morris and Michalopoulos, 2000). Little evidence exists from outside of the North Americacan context with Løken et al. (2018) as an important exception. They investigate the consequences of a 1998 Norwegian reform implemented over a period of three years that imposed work requirements and reduced the maximum period of benefit receipt from nine to three years, but also introduced a simultaneous, slight increase in benefit levels. The analysis is based on a difference-in-difference strategy that compares single mothers with married mothers. Løken et al. (2018) find no effects on grades in the overall population but statistically significantly negative effects among children of younger mothers. To sum up, the existing evidence-base is small, based on varied policy-designs and levels, and results in conflict.[3]

Our paper also speaks to a broader literature that is concerned with the consequences of childhood access to (near) cash welfare via social safety net programs for child well-being and human capital accumulation (e.g., Aizer et al., 2016; Hoynes et al., 2016; Bastian and Michelmore, 2018; Bailey et al., 2020; Brown et al. 2020). This literature studies programs such as the Mothers' Pension Program,

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[2] Another issue that complicates evaluation is that welfare reform components are often implemented over long periods of time; see for example de Grendre et al. (2021).

[3] Recent research has studied welfare reforms related to the immigrant population. Andersen et al. (2019) study a Danish 2003 reform that reduced benefits to refugee immigrants by around 50 percent for those granted residency after the reform date. They show that childrens' performance in language tests as well as length of education decreased as a consequence of the reform, just as teenagers' crime rates increased.

Food Stamp, EITC, and Medicaid and tends to find gains from access in terms of child human capital accumulation.

We structure the remainder of the paper as follows: Section 2 describes the institutional setting as well as the content of welfare reform in question, Section 3 presents the data, and Section 4 explains our empirial strategy. Section 5 continues to show the results and Section 6 concludes.

2. *The October 2016 reform: Reducing the level of benefits and introducing work requirements*

According to the Danish Law of Active Social Policy ("Lov om Aktiv Socialpolitik"), individuals qualify for welfare benefits in case of job loss or prolonged sickness spells if they cannot provide for themselves and their families through other means of income, such as unemployment insurance, or by depleting their assets. Benefits include a support allowance for daily living costs ("kontanthjælp") but also a shelter allowance[4] and supplements ("særlig støtte") to individuals deemed by caseworkers to be in particular need. Benefits increase with age above 30 as well as family size and single providers receive a top-up. Monetary incentives to find a job are generally limited because the benefit offset is high.[5] To counteract this, there is considerable availability testing: individuals on welfare must actively apply for jobs and/or participate in training courses. Still, there has been political (and academic) concerns, that the level of benefits did not sufficiently incentivize labor market participation and this has led to series of reforms in the area, with the explicit purpose of promoting work.

We study a recent reform that was passed into law in March 2016 and implemented in October 2016. Importantly, the reform was introduced in a period with relatively low unemployment (4%) and steady GDP growth rates of around 2%. The reform had consequences for all welfare recipients and consisted of two key components: it imposed an upper limit on total transfers received while on welfare benefits (the sum of the support allowance, the shelter allowance, and supplements) and it required that welfare recipients had worked at least 225 hours (ordinary hours only; six weeks full time) during the last 12 months for them to remain eligible for benefits. In practice, the counting of hours started in April 2016. The support allowance remained the same as before the reform and de facto only the shelter allowance and supplements were affected. The upper limit on total benefits caused a nonnegligible decrease in benefits received: as seen in Table 1, a typical single parent on

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[4] This is a universally provided means-tested benefit that is not specific to individuals on welfare. It is not limited to specific types of social housing.
[5] 1:1 for hourly wages over and above DKK 25/USD 3.6 in 2015.

welfare with two children went from a disposable income of DKK 13,100 before the reform to DKK 10,500 after the reform, corresponding to a reduction of almost 20%. Clearly, a lower level of benefits lowered the attractiveness of being on welfare but the upper limit on total transfers also created a more subtle, additional incentive to work for those affected by the limit: it effectively cancelled the benefit offset for individuals with a low number of hours worked because it kept total transfers constant. In other words, any loss in welfare benefits associated with take-up of work was offset by an equivalent increase in housing and special support.

If the 225 hour work requirements were not fulfilled, participants could lose part or all of their benefits.[6] The strictness of the new policy varied considerably with marital status: if one individual in a couple did not fulfill the work requirements, the individual would not receive welfare. If both did not fulfill the requirements, benefits for one individual would be withdrawn. Once both fullfil their work requirements, they would receive benefits again. A single individual, in contrast, would face a reduction in benefits of DKK 1,000 in case the work requirements were not fulfilled.

Table 1

Predicted pre- and post-reform disposable income absent behavioral changes, by family types

	Pre-reform disposable income	Post-reform disposable income	Percentage change
Singles			
No children	5,300	5,300	0%
One child	9,700	7,400	-24%
Two children	13,100	10,500	-20%
Three children	16,500	13,100	-21%
Four children	20,500	16,200	-21%
Cohabiting or married couples			
No children	10,700	9,200	-14%
One child	14,500	13,800	-5%
Two children	15,600	14,400	-8%
Three children	17,600	15,300	-13%
Four children	19,500	15,600	-20%

Notes: The table shows disposable monthly income after housing costs for individuals aged 30 or older, DKK 2015. Calculation assumes monthly rent for single without children of DKK 2,801 and DKK 6,138 for other family types. Calculation assumes other costs of housing to amount to DKK 761 and DKK 1,296. In families with one child, the child is assumed to be five years old; in families with two children five and ten years old; in families with three children five,

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[6] Municipalities had some leeway in this matter. Individuals considered by their caseworker to have limited ability to engage in a gainful activity were exempt from the work requirements.

ten, and 14 years old; and in families with four children one, five, ten, and 14 years old. Source: The Danish Ministry of Employment, The Family Type Model, October 2015.

3. *Empirical strategy*

The goal of the paper is to study the consequences of the welfare reforms for mothers' and ultimately children's outcomes. The starting point for the analysis is the population of mothers who received welfare in March 2016, corresponding to the time at which the reform was passed into law.[7] This choice is made to guard against issues with anticipatory behaviors but of course, it is conservative because some individuals do leave welfare in the period between the announcement and the actual implementation. To learn about the effects of the reform, we exploit variation in outcomes around the introduction of the reform in a comparative event approach that explicitly allows the effects to vary with the temporal distance to the reform. The basic idea is to compare outcomes for individuals in the reform group (mothers or their children depending on the outcome under study) in a given period with their own outcomes immediately before the reform (i.e. before March 2016). However, as we document below, there are clear within-calendar-year dynamics in welfare participation for that are not related to the reform. To account for these and the role of outcome dynamics more generally, we establish a comparison group consisting of the population of individuals on welfare exactly one year prior, in March 2015. In this comparison group, we subsequently compare outcomes immediately *before* March 2015 with outcomes in other time periods. Note that by construction, our set-up never suffers from issues with contaminated comparison groups; see de Chaisemartin and D'Haultfœuille (2020).

For each individual in the data, we denote the time at which the reform was passed into law by $t = 0$, and index all periods relative to that point in time. We start our analysis in the fall prior to the reform and continues until a year after the passing of the reform. The former choice is made to avoid interference from other, prior labor market reforms; the latter is made because this is when the comparison group gets exposed to the reform. Our ideal baseline specification considers a balanced panel of individuals who we observe in a period before and after the reform. Our main estimating equation is the following:

$$Y_{it} = \alpha + \beta \cdot reformpop_i + \sum_{j \neq -1} \delta_j I[j = t] + \sum_{j \neq -1} \gamma_j I[j = t] \cdot reformpop_i + \varepsilon_{it} \quad (1)$$

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[7] We exclude the immigrant population because of concurrent reform of welfare benefits available to this particular group (announced March 2016, implemented July 2016).

where Y is the outcome of interest, δ_j are event time dummies, and $reformpop$ indicates that individuals belong to the cohort exposed to the reform. ε is an error term, i indicates individuals, and t indicates time. γ are the parameters of interest; they represent the effects of the reform in the population of welfare participants. Note that with individual level panel data, (1) essentially corresponds to a fixed effects analysis with time varying effects of the reform; see Blundell and Costa Dias (2009) and Lechner (2011). As also pointed out by Lechner (2011), if covariates are not included, then estimation of the effects in designs using the linear regression framework in (1) is fully nonparametric. Our main results do not cluster standard errors; we explore the importance of clustering in sensitivity analyses.

The key identifying assumptions associated with our approach are 1) no anticipation, 2) parallel trends in outcomes in the absence of the reform, and 3) no other concurrent changes. By anchoring the population prior to the passage of the reform, we limit issues with anticipation. Figure 2 shows that this is a real concern: Google trends data illustrate that the reform received the most attention already at the time at which it was passed. There was some interest in the fall of 2015 (due to discussions in parliament) but not comparable to the level of attention in the spring of 2016 or the fall of 2016 when the reform was actually implemented.

Figure 2

Google trends data, "Upper limit on welfare benefits" ("Kontanthjælpsloft")
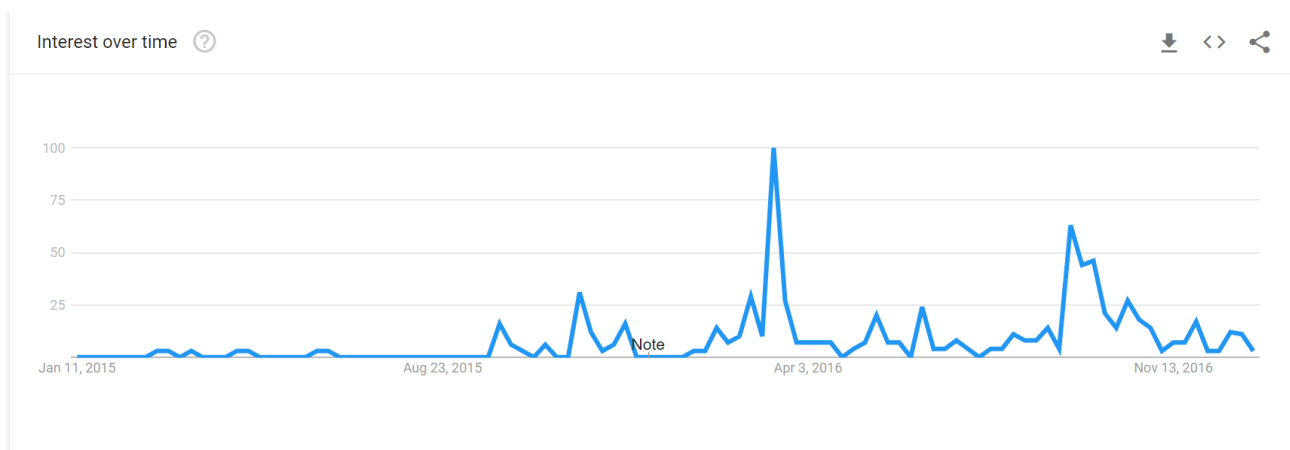
Notes: The figure shows google trends data for the search term "Upper limit on welfare benefits" ("Kontanthjælpsloft"). The numbers represent search interest relative to the highest point on the chart. A value of 100 is the peak popularity for the term. A value of 50 means that the term is half as popular. A score of 0 means there was not enough data for this term.

By anchoring the comparison group to March 2015, we also minimize any differential within-calendar-year outcome dynamics between the two groups. Our specification (1) allows us to directly

investigate differences in pre-trends; these will also yield insights into any anticipatory behaviors in the months leading up to the reform.[8] To explore concurrent changes, we consider the development over time in outcomes for the non-welfare population.

For child outcomes, we sometimes have to deviate from our ideal baseline specification that employs a balanced panel of individual level outcomes. This is because some outcomes are closely tied to the age or grade of the child. In these cases, we instead consider the relevant cohorts at each point in time.

4. Data, samples, and descriptive statistics

Data sources and outcomes

Our analyses make use of population-wide Danish register-based data. A unique identification number (the central personal register number; CPR) allows us to link individuals across registers and also parents to children. We construct a series of outcomes based on these registers. Oftentimes, the underlying data are available at a higher frequency than we can meaningfully analyze. In these case, we aggregate to a higher level. Key to our project is, of course, monthly information about welfare participation, benefit payments, and labor market outcomes such as earnings and hours worked, which we draw from the National Income Register. Labor market outcomes are available for the adults but also for their children. To shed light on potential mechanisms behind our main findings, we supplement our data with information about health care use, including visits to primary care physicians and psychologist. Like labor market outcomes, we analyze these on a monthly level.

In terms of distinct child outcomes, we analyze several domains indicative of child well-being, risky behaviors, and academic performance. All outcomes reflect aspects of child cognitive and noncognitive skills (e.g. Carneiro and Heckman, 2003) as well as the quality of the home environment. We deliberately explore a range of risky behaviors that vary in their severity. This to understand at which margins the reform may have the largest impacts. Our first key measure relates to children's risky behaviors as measured by school absenteeism. The data are provided by the Ministry of Education and include information about children enrolled in public schools. Since the

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[8] A standard approach to handling deviations from the parallel trends assumption has been to rely on a conditional version. As also discussed by Lechner (2011), it is not straightforward how to include covariates in the differences-in-differences setup but recent work by Sant'Anna and Zhao (2020) proposes a solution that combines outcome regression and inverse probability weighting.

vast majority of the children in our sample is enrolled in public schools, this is not a major issue.[9] In practice, we consider an indicator for any absence as well as days absent in a given quarter. The second key outcome uses indicators of children's wellbeing from the nationally administered school-based individual level well-being surveys developed by the Danish Ministry of Education. These surveys are collected in the spring of each year. In our analyses, we focus on the social well-being scale that is collected for children in grades 4-9 and ranges from 0-50.[10] Our final key outcome considers children's academic outcomes. We base these on standardized versions of the nationally administered performance tests in Danish reading and math. The national tests are IT-based, self-scoring, and adaptive. The test score is based on a measure of pupil ability; instead of giving all pupils the same questions and summing up the number of correct answers, the software calculates an ability measure after each question and then finds a question with a difficulty level that matches the contemporary measure of the pupil's ability level. The tests are carried out each spring in primary and lower secondary schools starting from grade 2. Danish reading is tested in grades 2, 4, 6, and 8, while math abilities are tested in grades 3 and 6. See Beuchert and Nandrup (2018) for more information.

In addition to the three primary outcomes, we analyse reports to municipal social services ("underretninger") because of concerns for the child in question. These data include both the date, the reason for the concern, and the type of informant. Anyone can express concern, and the report can be anonymous. Because the data is only available from 2015 and onwards, we are unfortunately unable to characterize differences in pre-trends across exposed and comparison cohorts. We study quarterly incidences of reports to social services. Finally, to gain insights into other adaptive behaviors that may drive our main findings, we exploit information about monthly school switching.

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[9] Out of the 18246 children aged 6-16 enrolled in school and born to mothers on welfare in March 2016, absence information is available for 17888; or 98%.

[10] The social well-being scale for children enrolled in grades 4-9 consists of the following ten questions/statements:
  a) How well do you like your school?
  b) How well do you like the other children in your classroom?
  c) Do you feel lonely?
  d) Are you afraid of being ridiculed at school?
  e) Do you feel safe at school?
  f) Since the start of the school year, did anyone bully you?
  g) I feel I belong at my school.
  h) I like the breaks at school.
  i) Most of the pupils in my classroom are kind and helpful.
  j) Other pupils accept me as I am.
The responses to all questions are coded to range from one to five, with five being the most positive. For positive questions like "Do you feel safe at school?" the value five is equivalent to "very often". For negative questions like "Do you feel lonely?" five means "never". In this sense, five is always the best outcome. In line with the ministry, we consider the sum of the responses. See Larsen et al. (2020) for a descriptive analysis of the link between social well-being and family, teacher, and peer characteristics.

For both mothers and children, we match all of these types of outcomes with rich demographic data from various administrative registers.

Samples and descriptive statistics

From the National Income Register, we first select the 24,396 ethnic Danish mothers on welfare in March 2016 – our reform group – and link these mothers to their 43,732 biological children aged 0-18. Table 2 shows how this population compares to the overall population of Danish women and their children and clearly documents that the former group is severely disadvantaged in terms of background characteristics and child outcomes. The mothers in are our data are clearly younger, less likely to be married, and more likely to be unskilled. They are also much more likely to have been overweight and to have smoked during pregnancy. We subsequently select the corresponding set of mothers on welfare in March 2015 – our comparison group – as well as their children aged 0-18. Just above 50 % of the women appear in both groups. Mothers on welfare in March 2015 and 2016 resemble each other closely in terms of observable characteristics, as do their children. There is, for example, a 15 gram difference in their average birth weigh and a one percentage point difference in the propensity for mothers to have smoked during pregnancy.

Table 2

Descriptive statistics, mothers and their children

|	Women 2016	Danish mothers on welfare in March 2016	Danish mothers on welfare in March 2015
Panel A. Adult population			
Age	50.5	35.6	36.8
	(18.8)	(7.8)	(8.6)
Married	0.47	0.16	0.18
Education:			
unskilled	0.47	0.64	0.61
vocational degree	0.31	0.27	0.29
short further	0.04	0.01	0.02
medium length further	0.21	0.06	0.07
long further	0.08	0.01	0.02
# observations	2,237,819	24,396	27,587

	All children aged 0-18 2016	Children aged 0-18 of Danish mothers on welfare in March 2016	Children aged 0-18 of Danish mothers on welfare in March 2015
Panel B. Children's early life			
Birth weight (gram)	3,479	3,349	3,360
	(611)	(637)	(635)
Birth weight < 2500 gram	0.05	0.08	0.08
Gestational age (days)	278	276	276
	(14)	(15)	(15)
Gestational age < 224 days (32 weeks)	0.009	0.013	0.013
# prenatal visits to midwife	4.9	4.8	4.9
	(2.1)	(2.4)	(2.3)
Mother BMI prior to pregnancy	24.5	25.7	25.7
	(8.0)	(9.2)	(9.1)
Mother BMI > 30	0.12	0.22	0.22
Mother smoker around time of pregnancy	0.18	0.53	0.52
# observations	1,094,875	43,732	48,323

Notes: The table shows descriptive statistics for Danish mothers on welfare and their children and compares these to the overall population of Danish women and their children. The adult population of women consists of females aged 18 or above. Own calculations based on analysis data.

To grasp the potential importance of the 225 hours requirement for the population under study, Figure 3 documents prior labor supply for mothers in the reform group and compares this to that of mothers in the comparison gorup. Firstly, mothers on welfare in March 2015 and March 2016 are highly similar in terms of their previous labor market attachment; and secondly, a substantial share works very little; about 80% has worked less than 10 hours during the last 12 months. Accordingly, substantial effort would be required for these women to meet the 225 hours requirement.

Figure 3

Distribution of hours worked during last 12 months
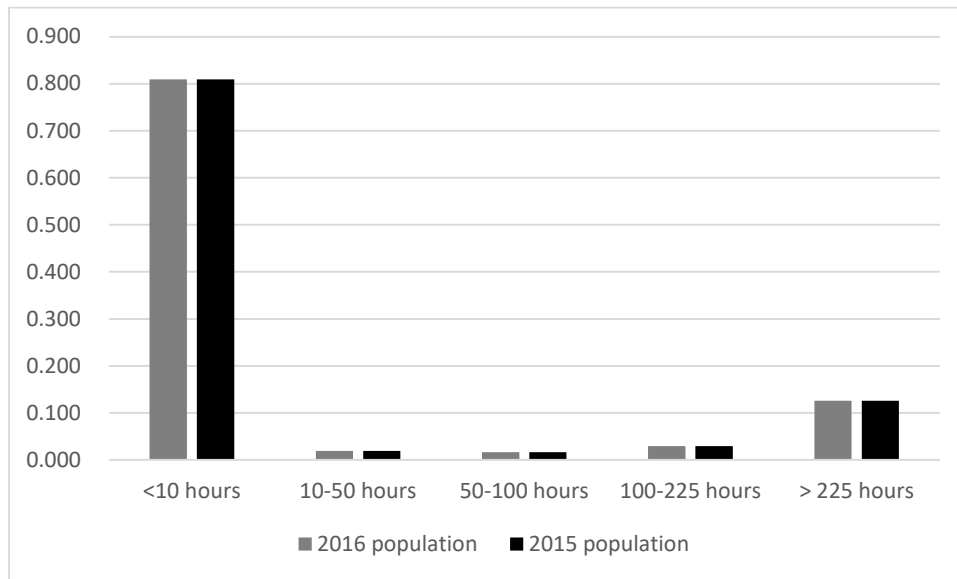
Notes: The table shows the distribution of hours worked during the last 12 months. 2016 (2015) population consists of women of Danish origin on welfare benefits in March 2016 (2015).

5. Consequences of welfare reform

This section shows our empirical findings. Since the overall purpose of the reform was to incentivize labor market participation, and ultimately lower levels of welfare participation, we start by exploring such margins for mothers and their older children. Equipped with these insights, we then move to explore the consequences for child wellbeing, risky behaviors, and academic outcomes. We finally perform a range of robustness analyses and investigate heterogeniety as well as possible mechanisms behind our results.

5.1 Effects of the reform on labor market outcomes and welfare participation

Results on mothers' hours worked appear in Figure 4. The left-most panel shows the differences in hours worked over time for our reform and comparison groups. The right-most figure shows the estimated effects of the reform from the comparative event study estimation anchored in February just prior to the passing of the reform. Note first that the estimated effects of the reform are very close to zero in the months prior to its passing and all estimates are statistically insignificant too. This assures us that our estimation approach actually manages to balance pre-trends and that welfare participants did not anticipate the reform before it was passed into law. We estimate small, positive

effects on hours worked as a consequence of the reform. In March 2017, for example, the estimated effect is 1.5 hours, corresponding to 7% of the comparison mean. To put this into perspective, Figure A1 shows that around 20% of the population of mothers on welfare benefits in March 2016 manage to accumulate at least 225 hours in March 2017.

Figure 4
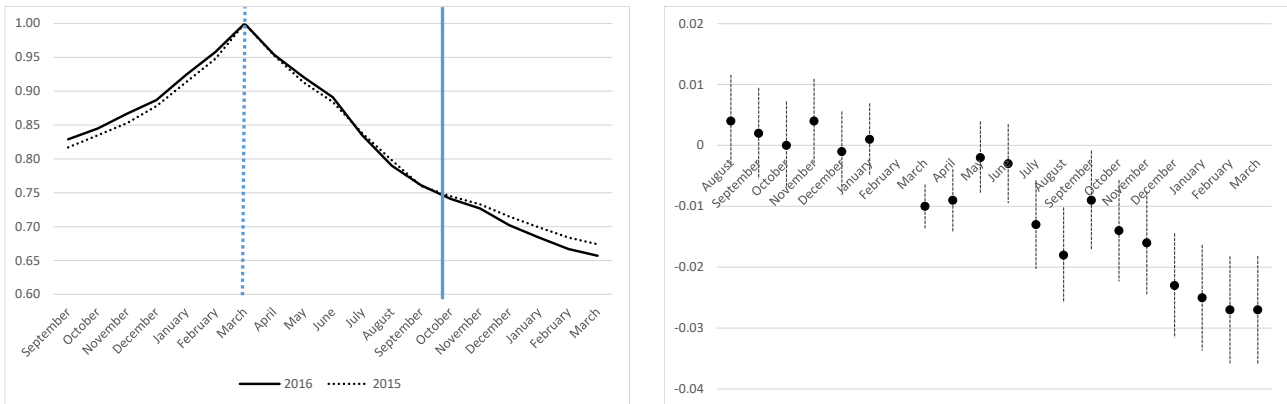
Estimated effects of the reform on mothers' hours worked
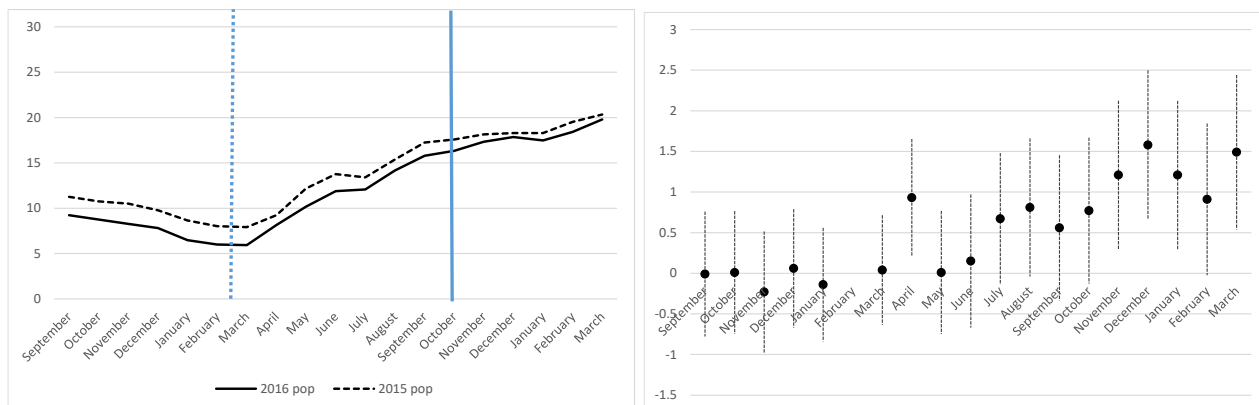
Note: The left-most figure shows hours worked; the right-most figure shows the estimates and 95%-confidence intervals from an event study estimation anchored in February, just prior to the passing of the reform. 2016 (2015) population consists of mothers of Danish origin on welfare benefits in March 2016 (2015). The dashed vertical line indicates the passing of the reform; the fully drawn vertical line indicates the timing of the reform implementation. The full set of estimates is shown in Table A1.

Figure 5 is instead concerned with mothers' welfare participation. From April and onwards, and particularly from October when the reform was fully implemented, we detect significantly negative effects on welfare participation. Effects are small however; the estimate in March 2017 is -0.027, corresponding to 4% of the comparison group mean.

In short, only a few mothers in this population managed to leave welfare entirely and only a smaller share increased their labor supply to an exent sufficient to avoid the monetary sanctions associated with the 225 hours requirement. The combination of the reduction in benefits through the upper limit, the risk of monetary sanctions, and the lack of any substantial increases in earnings through hours worked *de facto* meant that most families had fewer means available after the reform.

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Figure 5

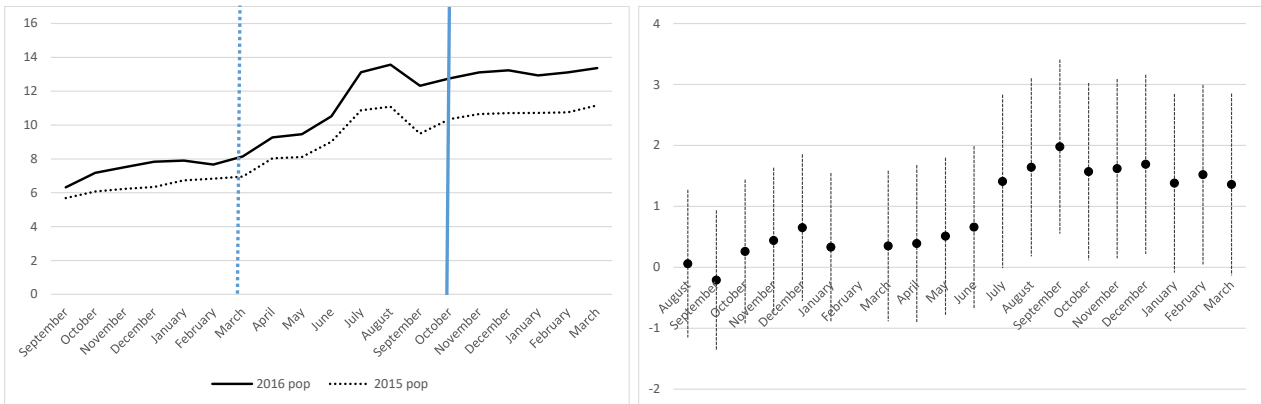Estimated effects of the reform on mothers' welfare participation

Notes: The left-most figure shows the share receiving any welfare benefits; the right-most figure shows the estimates and 95%-confidence intervals from an event study estimation anchored in February, just prior to the reform. 2016 (2015) population consists of mothers of Danish origin on welfare benefits in March 2016 (2015). A dashed vertical line indicates the passing of the reform; a fully drawn vertical line indicates the timing of the reform implementation. The full set of estimates is shown in Table A1.

Figure 6 instead shows results for hours worked among the oldest children in the families, namely those aged 15-18.[11] In parallel to the adults, we consider children born to mothers on welfare in March of 2016 (2015) to be the reform (comparison) cohort. Since the reform resulted in fewer means available to mothers, the reform created incentives for their children to generate their own income. This is exactly what we observe in Figure 6: in the fall months, starting already in August, children in the reform group increase their hours worked by around 1.5-2 hours per month. Both in an absolute and in a relative sense, effects sizes for these teenagers actually correspond to those of their mothers.

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[11] Children under the age of 13 are not allowed to work for pay and there are considerable restrictions on hours worked for children aged 13-17. These restrictions depend on whether work takes place on school days and on school enrolment.

Figure 6

Estimated effects of the reform on hours worked, children aged 15-18

Note: The left-most figure shows hours worked; the right-most figure shows the estimates and 95%-confidence intervals from an event study estimation anchored in February, just prior to the passing of the reform. The 2016 (2015) population consists of children of mothers of Danish origin on welfare benefits in March 2016 (2015). The dashed vertical line indicates the passing of the reform; a fully drawn vertical line indicates the timing of the reform implementation. The full set of estimates is shown in Table A1.

5.2 Effects of the reform on school absence, child wellbeing, and academic outcomes

Given the labor market related findings, we move on to investigate effects on child outcomes indicative of wellbeing, risky behaviors, and academic performance. Our starting point is a measure of risky behaviors, namely a quarterly indicator of any school absence. Being present at school is a prerequisite for learning, of course. Carlsson et al. (2015), for example, document that an extra ten days of school instruction raises cognitive skills as measured by scores on intelligence tests (synonyms and technical comprehension tests) by approximately 1% of a standard deviation. Figures 7-8 shows the formal results for any absence and days absent from school. Again, in parallel to the adults, we consider children enrolled in public school born to mothers on welfare in March of 2016 (2015) to be the treated (comparison) cohort. We find no reactions in terms of any absence for the youngest children in our sample, while the older children in grades 4-9 increase absence slightly. We also detect rather substantial increases in days absent due to the reform, regardless of age. For children in grades 4-9, for example, the estimated effects one year after the passing of the reform corresponds to about 15% of the mean in the comparison group. This suggests to us that effects are driven by children who already have issues with school absenteeism.

Figure 7

Estimated effects of the reform on any school absence
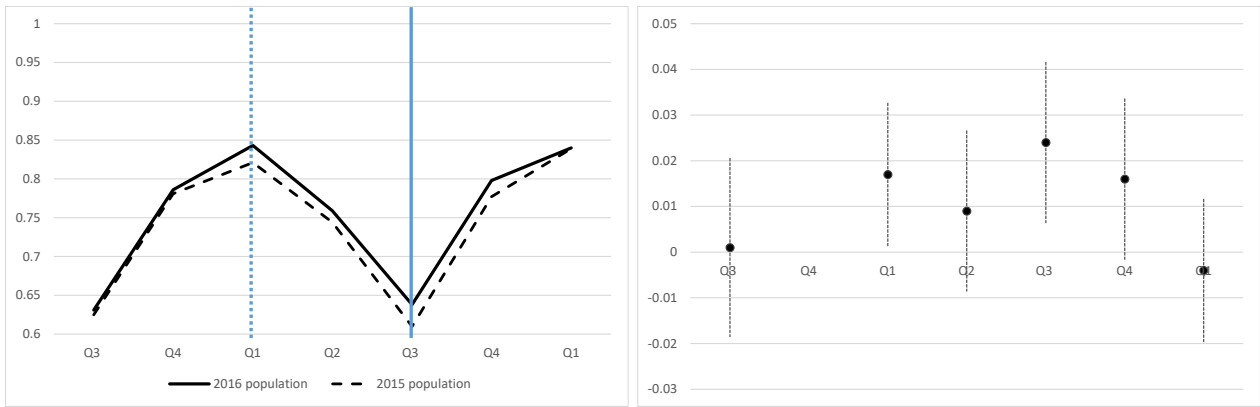
Panel A: Any absence, children in kindergarten – 3rd grade
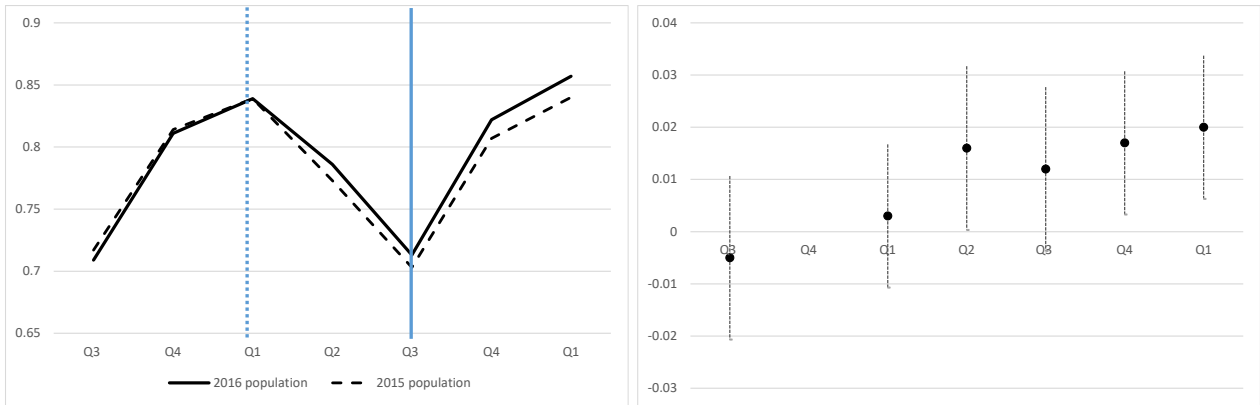
Panel B: Any absence, children in 4th – 9th grade

Note: The left-most figures show indicator for any absence; the right-most figures show the estimates and 95%-confidence intervals from event study estimation anchored in Q4 2015 (2014), just prior to the passing of the reform. The 2016 (2015) population consists of children of mothers of Danish origin on welfare benefits in March 2016 (2015). The dashed vertical line indicates the passing of the reform; a fully drawn vertical line indicates the timing of the reform implementation. The full set of estimates is shown in Table A2.

Figure 8

Estimated effects of the reform on days absent from school

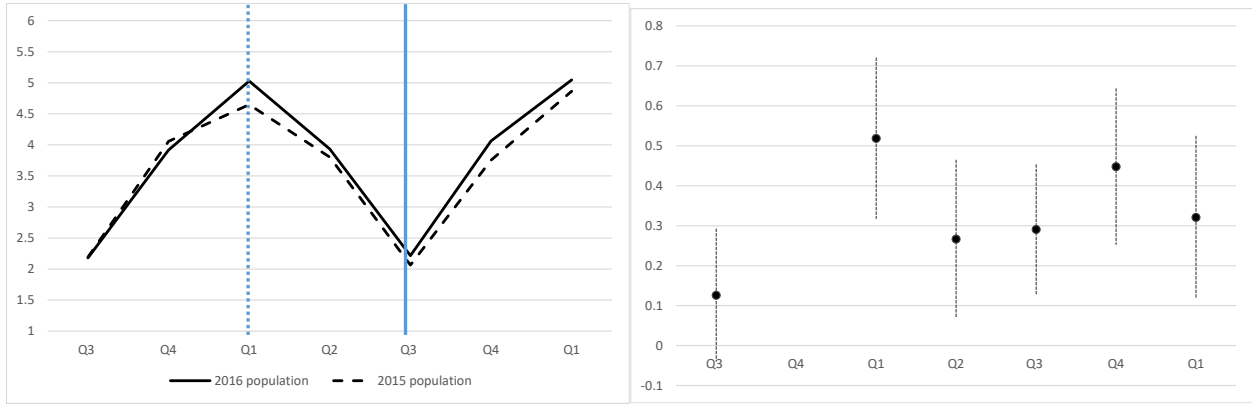Panel A: Days absent, children in kindergarten – 3rd grade
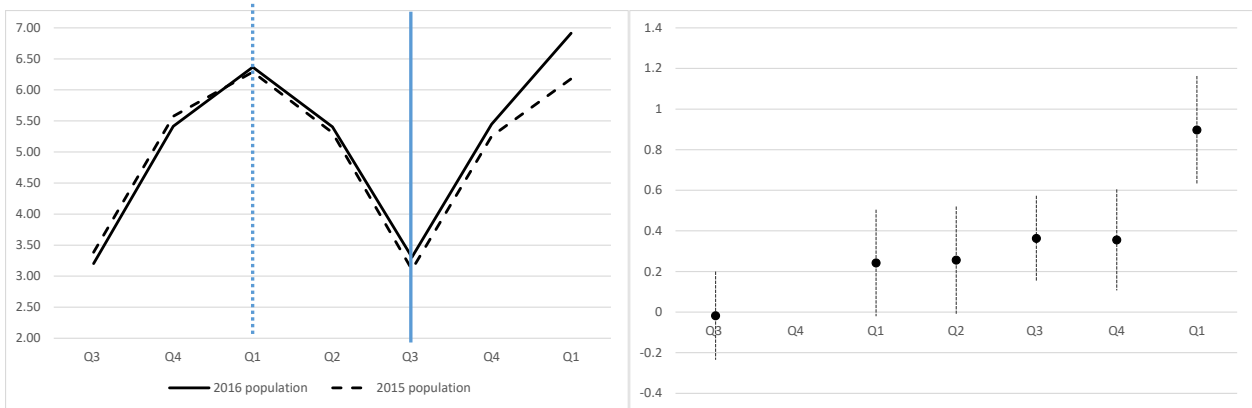

Panel B: Days absent, children in 4th – 9th grade

Note: The left-most figures show days absent; the right-most figures show the estimates and 95%-confidence intervals from an event study estimation anchored in Q4 2015 (2014), just prior to the passing of the reform. The 2016 (2015) population consists of children of mothers of Danish origin on welfare benefits in March 2016 (2015). The dashed vertical line indicates the passing of the reform; a fully drawn vertical line indicates the timing of the reform implementation. The full set of estimates is shown in Table A2.

19

Table 3 explores effects on self-reported social well-being, constructed from the nationally administered well-being surveys developed by the Danish Ministry of Education. Note that this survey is gathered each spring. This implies that we consider the spring of 2016 (2015) as the pre-period for the reform (comparison) population. Social wellbeing is our only measure that relies entirely on children's own reports and informs us about how they perceive to be thriving. Remember, however, that the questions asked primarily concern children's wellbeing at school, not at home. Moreover, though the school management and teacher do not have access to the individual level responses, children may still be concerned that their reports will reflect negatively on their parents and this loyalty conflict may impact their responses.[12] For these reasons, we do not expect substantial effects of the policy change – and the reform may even decrease response rates. This is indeed what we find: the propensity to respond decreases with just below six percentage points and social well-being decreases with about .7 points (conditional upon responding). The effect is not large but at the same time also not negligible; it corresponds to just below ten percent of a standard deviation (in the comparison group), or about 25% of the difference in social wellbeing between the overall population of Danish children and the comparison group. Since individual and family level proxies for prior disadvantage have previously been shown to predict lack of survey response on this particular measure (Larsen et al., 2020) it is reasonable to expect a negative selection in this case too. Accordingly, we most likely underestimate the effect on social wellbeing.

Appendix Table A3 shows the results for each of the subquestions included in the social wellbeing score. Except for the subquestions concerning loneliness and a sense of belonging, we observe statistically significant negative effects across all questions, including two of those that load on emotional stability ("Do you feel safe?" and "Other pupils accept me as I am") but not the third ("Do you feel lonely?"); see Andersen et al. (forthcoming).

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[12] This hypothesis was also brought up in personal communication with the previous chair of the Social Workers' Union Majbrit Berlau (January 12, 2021).

Table 3

Estimated effects of the reform on self-reported social wellbeing, grades 4-9

Variable	Coefficient Estimate	Standard Error
Survey participation (0/1) among 11-15 year olds		
Effect of reform	**-0.057**	(0.006)
Post	0.005	(0.006)
Reform population	0.005	(0.006)
Pre-mean in comparison group	0.659	
N	58,960	
Social wellbeing score		
Effect of reform	**-0.673**	(0.154)
Post	0.039	(0.159)
Reform population	-0.024	(0.117)
Pre-mean (std. dev) in comparison group	37.94 (7.93)	
Diff in means, overall pop and comp group	2.66	
N	49,858	

Notes: The table shows the results from comparative event study estimation using 2015-2017 data for reform cohort and 2015-2016 data for comparison cohort. Model controls for linear time trend. **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

We then move on to show the results for academic performance as measured by the national test scores; see Table 4. Note first that test taking is not an issue in this case; about 0.7% of children do not sit the age appropriate test. Moreover, in contrast to our measures of risky behaviors and wellbeing, we detect no negative effects on test scores.[13] This is maybe not surprising since test scores to a higher degree than social well-being and absence from school reflect skills that accumulate over time.[14]

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[13] Test taking is not an issue in this case; about 0.7% of children do not sit the test.

[14] Landersø et al. (forthcoming) do find that 9th grade test scores are sensitive to the school start age of younger siblings; another type of stressor. The authors argue that this is likely because delaying the school start of a younger sibling allows parents to redirect resources towards the dimensions in older siblings' upcoming exams that are most easily improved.

Table 4

Estimated effects of the reform on test scores

	Coefficient Estimate	Standard Error
Test-taking, reading		
(age appropriate testing in grades 2, 4, 6, and 8)		
Effect of reform	-0.002	(0.002)
Post	0.002	(0.002)
Reform population	0.000	(0.002)
Pre-mean in comparison group	0.007	
N	23,707	
Reading		
Effect of reform	0.011	(0.029)
Post	0.013	(0.020)
Reform population	-0.014	(0.020)
Pre-mean (std. dev) in comparison group	-0.49 (1.11)	
N	24,371	
Math		
Effect of reform	0.022	(0.046)
Post	0.008	(0.030)
Reform population	-0.017	(0.030)
Pre-mean (std. dev) in comparison group	-0.6 (1.03)	
N	8,309	

Notes: The tables shows the results from comparative event study estimation using 2016-2017 data for reform cohort and 2015-2016 data for comparison cohort. Post-measurement Q1 2017 (2016); pre-measurement Q1 2016 (2015). **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

5.3 Suggestive evidence: Effects of the reform on reports to municipal social services

We finally explore the consequences for more severe outcomes related to children's wellbeing. Here, we consider reports to municipal social services because of concerns for the child in question. The left-most panel of Figure 9 reveals that children in our sample faces a quarterly risk of having at least one report made to social services of between seven and eight percent. Table A4 indicates that the most common reasons for concerns in our population are child externalizing behaviors (23% of all

reports); insufficient care from parents (18%); and high levels of conflict in the family (13%). Informants are primarily school staff (20%) or health care providers (12%), though anonomous informants are also very common (9%). The formal results are shown in the right-most panel of Figure 9. Because the gathering of this particular data source starts in 2015, we are unable to explore pretrends. For this reason, we urge caution in the interpretation of the results and think of these as providing more suggestive evidence.

We follow our approach from analyzing social wellbeing that considers the first quarter of 2016 (2015) the pre-period for the reform (comparison) population. Results show statistically significant upticks in reports to social services, coinciding with the implementation of the reform. In an absolute sense, estimates are not large (.5 percentage points in the quarter of the reform; .9 percentage points in the quarter following the reform) but they are substantial in a relative sense (7 percent of the comparison mean in the quarter of the reform; 13% in the quarter following the reform). Interestingly, our results corroborate those of Kovski et al. (2021) who document links between the presence and generosity of EITC and state-level rates of child maltreatment.

Taken at face value, these results indicate that children are more likely to come to the attention of professionals because of the reform. This can either be because of behaviors in the child or because of increased risks at home. To the extent that children worry about bringing issues at home to the attention of profesionals, these findings connect well with the decrease in the response rates on the social well-being survey.

Figure 9

Estimated effects of the reform on any reports to municipal social services
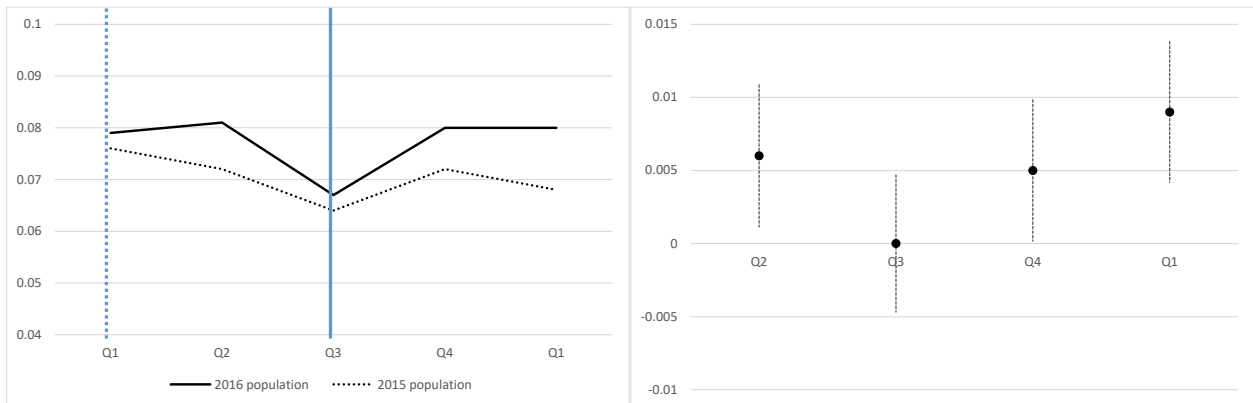
Note: The left-most figure shows any reports to social services; the right-most figure shows the estimates and 95%-confidence intervals from event study estimation anchored in Q1 2016 (2015), at the passing of the reform. The 2016 (2015) population consists of children of mothers of Danish origin on welfare benefits in March 2016 (2015). The dashed vertical line indicates the passing of the reform; a fully drawn vertical line indicates the timing of the reform implementation. The full set of estimates is shown in Table A2.

5.4 Sensitivity analyses: threats to the validity of the our design

Our analyses above do not reveal any issues with the parallel trends assumption, for example due to anticipatory behaviors prior to the passing of the reform. Still, other factors may pose threats to our research design.

One may raise the concern that we simultaneously explore a long list of outcomes at several points in time, which could lead to issues with multiple hypothesis testing. However, given the existing literature that primarily focuses on education-related outcomes, we think of our study as a hypothesis-generating exploratory analysis that can inform additional work in the area (Institute for Education Sciences, 2013). This is especially important since many researchers do not have easy access to a broad span of outcomes and will have to choose ex ante which to gather. Our study may guide these choices. In any case, we directly explore the sensitivity of our results to mulitiple hypotheses testing. Here, we focus on our three key child outcome domains that are all available for children in grades 4-9; school absence, self-reported social wellbeing, and national test scores. We follow Jones et al. (2019) and calulate family-wise adjusted p-values (Westfall and Young, 1993) based on 1,000 bootstraps. This procedure addresses multiple inference concerns by controlling for the family-wise

error rate (the probability of incorrectly rejecting one or more null hypotheses belonging to a family of hypotheses). Table 5 documents that our main conclusions are robust.

Table 5

Accounting for multiple hypotheses testing

Effects on outcomes one year after passing of reform, grade 4-9

	Coefficient estimate	Standard error	Conventional p-value	Wyoung p-value
Reading	0.016	(0.025)	0.51	0.80
Math	0.024	(0.032)	0.46	0.80
Any absence	0.020	(0.007)	0.004	<0.001
Days absent	0.897	(0.136)	<0.001	<0.001
Social well-being score	-0.637	(0.154)	<0.001	<0.001

Notes: The table shows sensitivity to multiple hypotheses testing (Westfall and Young, 1993). The national test score results as well as the absence results rely on comparative event study using 2016-2017 child data for reform cohort and 2015-2016 child data for comparison cohort. The social well-being results rely on comparative event study using 2015-2017 data for reform cohort and 2015-2016 data for comparison cohort. The model controls for a linear time trend.

Table 6 presents two sets of additional sensitivity analyses for the child outcomes that did show a response to the reform. We first enrich our analyses with a set of control variables and next explore the importance of clustering standard errors at the family level. At the outset, we do not expect the former to have important bearing on our results. This is because the reform and comparison groups are very similar in terms of observable characteristics. The latter, on the other hand, might easily be an issue; recall that about half of the mothers on the reform group are also present in the comparison groups and that each mother may contribute with more than one offspring in the child level analyses. In reality, our additional anlyses demonstrate that our results are completely robust to these changes to the empirical specification.

Table 6

Selected sensitivity analyses, grade 4-9

Effects on outcomes one year after passing of reform

	No control vars	Add control vars	Cluster individual level	Cluster family level
Any absence	**0.020**	**0.019**	**0.019**	**0.019**
	(0.007)	(0.007)	(0.007)	(0.007)
N			48,194	
Days absent	**0.897**	**0.787**	**0.787**	**0.787**
	(0.136)	(0.137)	(0.123)	(0.129)
N			48,194	
Social well-being score	**-0.637**	**-0.643**	**-0.643**	**-0.643**
	(0.154)	(0.153)	(0.142)	(0.142)
			49,858	

Notes: The table shows selected sensitivity analyses. The absence results rely on comparative event study using 2016-2017 child data for reform cohort and 2015-2016 child data for comparison cohort. The social well-being results rely on comparative event study using 2015-2017 data for reform cohort and 2015-2016 data for comparison cohort. The model controls for a linear time trend. Control variables include an indicator for whether the mother was married, the mother's prior hours worked, child grade, and child gender. We measure control variables at the time of or prior to sampling. **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

A separate important concern relates to changes in context across the various outcome domains that coincide with (or postdate) the reform and thus may conflate our findings. Such changes could, for example, include shocks to labor demand; epidemics; or even revisions of the institutional setting. To explore the sensitivity of our findings to the role of such factors, we implement a triple comparative event study that exploits a population that was unaffected by the reform. We track the outcomes for this population during the exact same period as the reform cohort, allowing us to estimate and account for overall trends. Specifically, we propose to use the population of mothers who were *not* on welfare at any point in time during 2015-2016 (as well as their children). To verify that this population was indeed not affected by the reform, we first explore whether the reformed affected inflows into welfare for this group. Figure A2 shows the share of all mothers on welfare benefits during 2015 and 2016 as well as the difference between these shares. There is some within-calendar year variation yet the difference between the shares across the two years is constant throughout and thus does reveal any differential inflow in connection with the reform; at least not in the period that is relevant for our study.
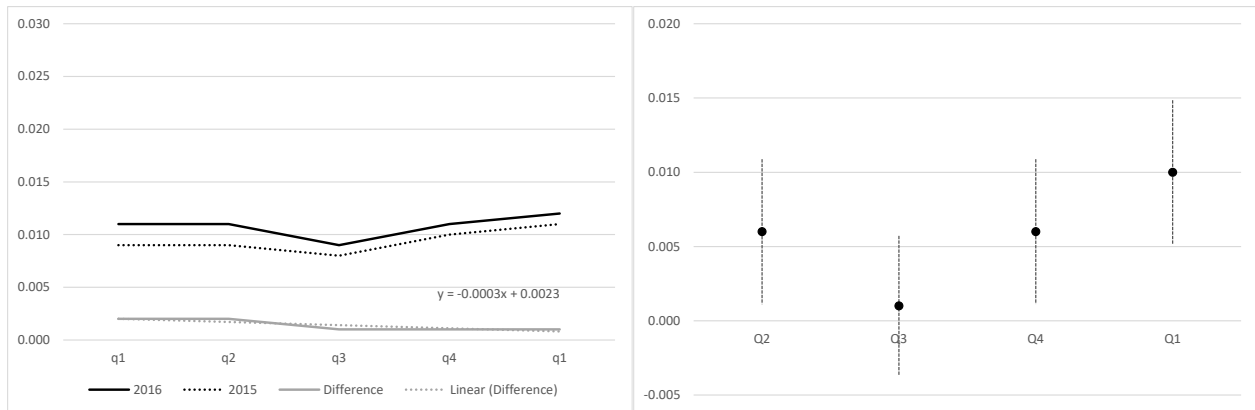
Figure 10

Estimated effects of the reform on selected outcomes, triple comparative event study
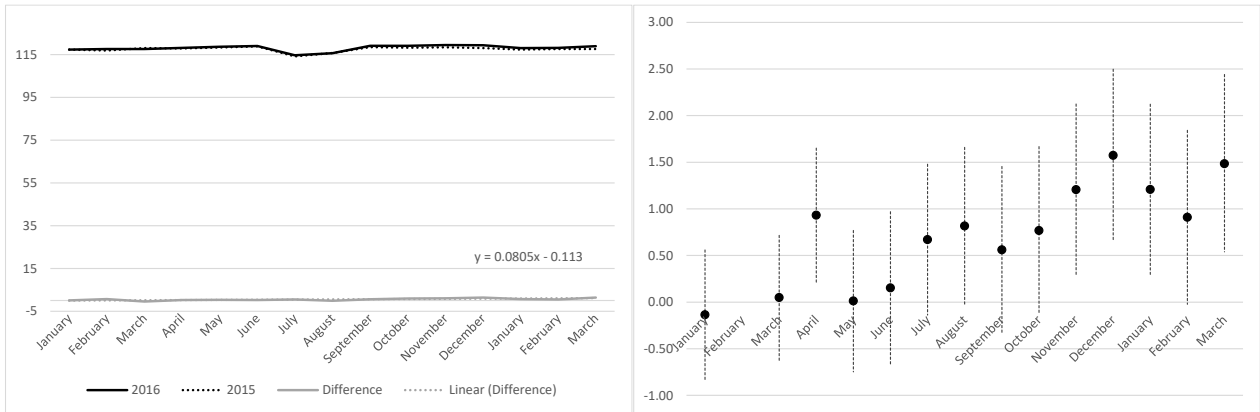
Panel A: Mothers' hours worked
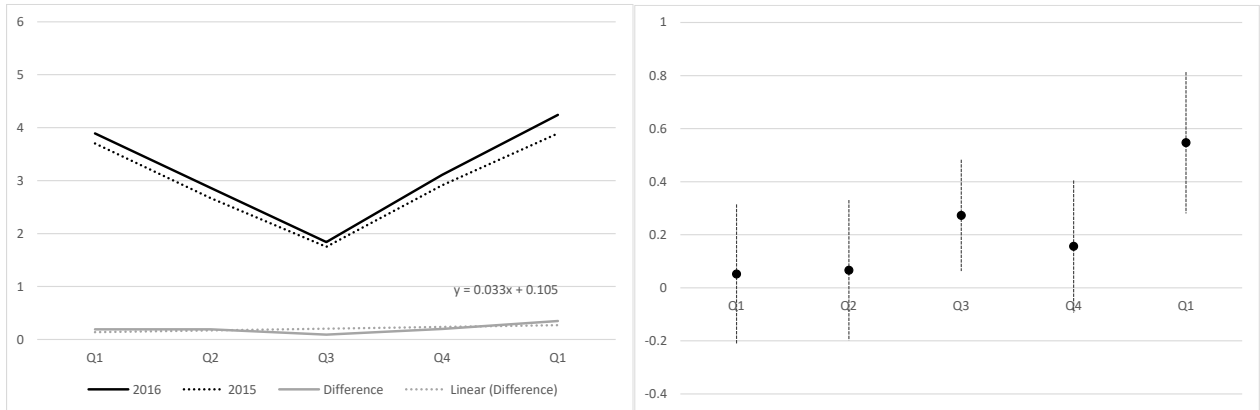
Panel B: Days absent from school, children in grades 4-9

Panel C: Any reports to municipal social services

Note: The left-most figures show means for mothers not welfare benefits (Panel A) and their children (Panels B-C) during 2015 and 2016 as well as the difference between these means. The right-most figure in Panel A shows the estimates and 95%-confidence intervals from a triple comparative event study estimation anchored in February, just prior to the reform. 2016 (2015) population consists of mothers of Danish origin on welfare benefits in March 2016 (2015). The second comparison is based on a balanced panel of mothers not on welfare benefits, observed Jan 2015-March 2017. The right-most figure in Panel B shows the estimates and 95%-confidence intervals from a triple comparative event study estimation anchored in Q4, just prior to the reform. 2016 (2015) population consists of children of mothers of Danish origin on welfare benefits in March 2016 (2015). The second comparison is based on a balanced panel of children of mothers not on welfare benefits, observed Jan 2015-March 2017. The rigth-most figure in Panel C shows the estimates and 95%-confidence intervals from a triple comparative event study estimation anchored in Q1. 2016 (2015) population consists of children of mothers of Danish origin on welfare benefits in March 2016 (2015). The second comparison is based on a balanced panel of children of mothers not on welfare benefits, observed Jan 2015-March 2017.

We present the results for selected outcomes based on the triple comparative event study in Figure 10. The left-most figures show the means for the non-affected population as well as the difference in means between 2015 and 2016, while the right-most figures shows the estimated effects of the reform. Fortunately, there is no evidence that our findings were driven by other concurrent events.

5.5 Channels at play: some insights from heterogeneity analyses

Given the estimated effects on maternal outcomes, the effects on children's outcomes probably arise both from lower access to resources useful for child development as well as from worse emotional well-being. Also, much evidence suggests that particularly disadvantaged children are more susceptible to negative shocks. To explore the role of prior vulnerability in terms of the family environment, we estimate effects by measures of family stability (marital status, single parent). To

shed light on the importance of access to resources, we consider effects by mothers' distance to the labor market. We also separately investigate effects by child gender, although the extant literature is less clear about whether boys or girls are more likely to be affected.

There are good reasons to think that effects on the children may vary with the quality of the family environment. To the extent that a stable partner can help alleviate any stress incurred by the reform and assist with means of income, we expect that children in married families are more resilient to reform exposure. This is exactly what we see in Table 7; the decrease in social well-being and the increase in absence are much stronger in the group of children from non-married families. Of course, since more mothers are non-married in the overall population, we have greater statistical power in these specifications. We do not detect effects on test scores, regardless of the mother's marital status.

We next delve into the importance of mothers' prior labor market participation. In practice, we estimate reform effects by whether the mother had accumulated fewer than or at least ten hours on the labor market during the 12 months prior to the passing of the reform. Effects are clearly driven by children of mothers with very low prior labor market participation. This is not only a question of statistical significance; estimated effects are much larger in this group too.

Table 7

Heterogeneity in the estimated effects of the reform one year after its passing, selected outcomes

	Mother married	Mother not married	Boys	Girls	Mother <10 acc hours	Mother >=10 acc hours
Any absence	0.010	**0.022**	0.014	**0.025**	**0.018**	0.013
	(0.016)	(0.008)	(0.010)	(0.010)	(0.008)	(0.015)
Pre-mean in comparison group	0.796	0.818	0.807	0.821	0.817	0.799
Estimate/pre-mean	0.013	0.027	0.017	0.030	0.022	0.016
N	9,707	38,487	24,549	23,645	36,775	11,419
Days absent	**0.86**	**0.88**	**0.83**	**0.97**	**0.82**	0.35
	(0.29)	(0.15)	(0.19)	(0.20)	(0.16)	(0.28)
Pre-mean in comparison group	5.06	5.69	5.47	5.68	5.76	4.74
Estimate/pre-mean	0.17	0.15	0.15	0.17	0.14	0.07
N	9,707	38,487	24,549	23,645	36,775	11,419
Social well-being score	-0.106	**-0.416**	*-0.324*	**-0.432**	**-0.372**	-0.324
	(0.313)	(0.164)	(0.194)	(0.212)	(0.164)	(0.313)
Pre-mean in comparison group	38.35	37.83	39.07	36.83	37.22	38.40
Estimate/pre-mean	-0.003	-0.011	-0.008	-0.012	-0.010	-0.008
N	10,168	39,690	25,045	24,813	39,745	10,113

Notes: The table shows selected heterogeneity analyses. The absence results rely on comparative event study using 2016-2017 child data for reform cohort and 2015-2016 child data for comparison cohort. The social well-being results rely on comparative event study using 2015-2017 data for reform cohort and 2015-2016 data for comparison cohort. The model controls for a linear time trend. **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

5.6 Differential help-seeking or other adaptive behaviors

One might argue that the reform could have lead to differential help-seeking as well as other adaptive behaviors that might indirectly contribute to our findings. In particular, we investigate effects on health care use such as visits to primary care physicians, private-practicing psychologists or psychiatrists, and emergency department care. We also explore whether the reform induced school-switching through moves away from more expensive housing. We find no evidence, however, that the reform affected any these margins. Results are available upon request.

6. Conclusion

This paper uses register-based data to analyze the consequences of a recent major Danish welfare reform on children's human capital and well-being. In addition to work requirements, the reform introduced an upper limit on welfare benefits. We implement a comparative event study that contrasts individuals on welfare at the time of reform announcement before and after the reform with the development in outcomes for the group of individuals on welfare exactly one year prior. Our results reveal that benefit payments did indeed decrease because of the reform but that mothers' propensity to receive welfare was largely unaffected by the reform, as were their labor market participation, at least in the short run. We then show small but negative effects on children's school well-being, as measured by individual-level nationally adminstered well-being surveys and small increases in absence from school because of the reform. Short-run child academic performance, in contrast, was not measurably affected by the reform. We also document increases in reports to social services for children exposed to the reform but suggest that these results are interpreted with care because the data source is relatively new and does not allow for analyses of differences in pre-trends.

Given that the reform did not appear to have large effects on the likelihood of receiving welfare for the adult population, one could reasonably scale our intention-to-treat estimates with the share still receiving welfare arrive at an average treatment effect for those still exposed to the policy. Since about 70% of mothers on welfare at the time the reform was passed into law continue to be on welfare at the time at which the reform was implemented, this would increase our findings around the time of implementation with around 40%.

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Appendix A

Figure A1

Share of population that has worked at least 225 hours, with counting starting in April
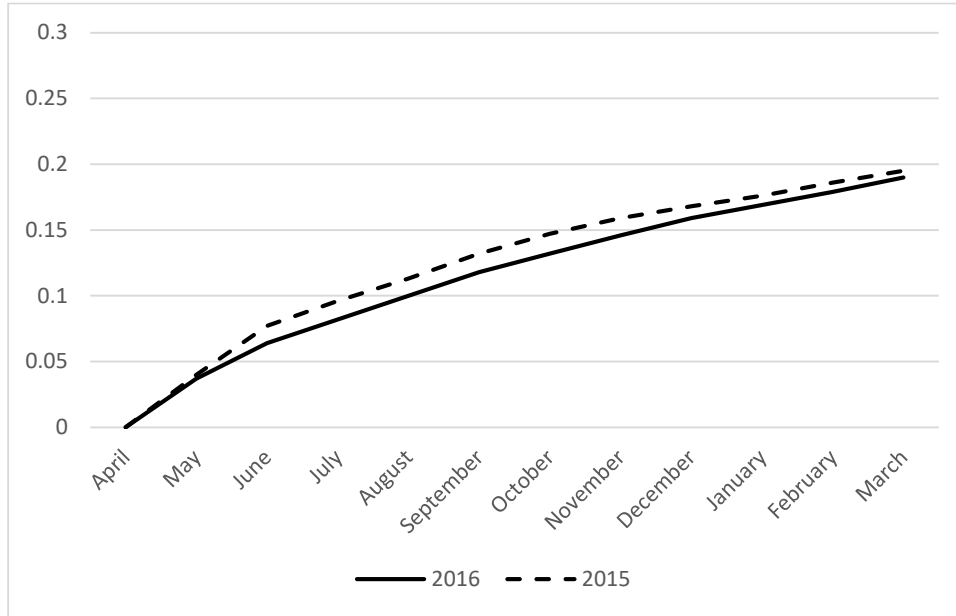
Notes: Figure shows share who has worked at least 225 hours, with counting starting in April; 2016 (2015) population consists of mothers of Danish origin on welfare benefits in March 2016 (2015).

Figure A2

Inflow into welfare
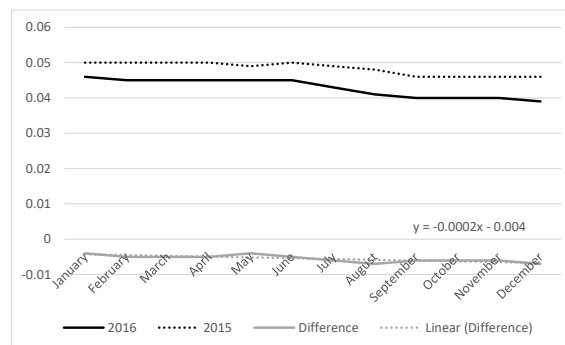
Note: The figure shows the share of mothers of Danish origin on welfare benefits during 2015 and 2016 as well as the difference between the shares.

Table A1

Estimated effects of the reform

	Mothers' hours worked		Mothers' welfare participation		Children aged 15-18, hours worked	
	Estimate	Standard Error	Estimate	Standard Error	Estimate	Standard Error
September	-0.01	(0.39)	0.002	(0.004)	-0.21	(0.41)
October	0.01	(0.39)	0.000	(0.004)	0.26	(0.43)
November	-0.23	(0.38)	0.004	(0.004)	0.44	(0.43)
December	0.06	(0.37)	-0.001	(0.003)	0.65	(0.43)
January	-0.14	(0.36)	0.001	(0.003)	0.33	(0.44)
February						
March	0.04	(0.34)	**-0.010**	(0.002)	0.35	(0.45)
April	**0.93**	(0.37)	**-0.009**	(0.003)	0.39	(0.47)
May	0.01	(0.39)	-0.002	(0.003)	0.51	(0.47)
June	0.15	(0.42)	-0.003	(0.003)	0.66	(0.48)
July	0.67	(0.41)	**-0.013**	(0.004)	**1.41**	(0.52)
August	*0.81*	(0.43)	**-0.018**	(0.004)	**1.64**	(0.53)
September	0.56	(0.46)	**-0.009**	(0.004)	**1.98**	(0.52)
October	*0.77*	(0.46)	**-0.014**	(0.004)	**1.57**	(0.53)
November	**1.21**	(0.47)	**-0.016**	(0.004)	**1.62**	(0.54)
December	**1.58**	(0.47)	**-0.023**	(0.004)	**1.69**	(0.54)
January	**1.21**	(0.47)	**-0.025**	(0.004)	**1.38**	(0.54)
February	*0.91*	(0.48)	**-0.027**	(0.004)	**1.52**	(0.54)
March	**1.49**	(0.48)	**(0.027)**	(0.005)	**1.360**	(0.548)

Notes: This table shows estimates corresponding to those in Figures 4-6.

Table A2

Estimated effects of the reform

	Any school absence, grades 0-3		Any school absence, grades 4-9		Days absent grades 0-3		Days absent grades 4-9		Any reports to municipal social services	
	Estimate	Standard Error	Estimate	Standard Error	Estimate	Standard Error	Estimate	Standard Error	Estimate	Standard Error
Q3	0.001	(0.010)	-0.005	(0.008)	0.126	(0.085)	-0.018	(0.111)		
Q4		0.000								
Q1	**0.017**	(0.008)	0.003	(0.007)	**0.519**	(0.103)	*0.242*	(0.134)		
Q2	0.009	(0.009)	**0.016**	(0.008)	**0.267**	(0.101)	*0.256*	(0.135)	**0.006**	(0.002)
Q3	**0.024**	(0.009)	0.012	(0.008)	**0.291**	(0.083)	**0.363**	(0.107)	0.000	(0.002)
Q4	*0.016*	(0.009)	**0.017**	(0.007)	**0.448**	(0.100)	**0.356**	(0.127)	**0.005**	(0.002)
Q1	-0.004	(0.008)	**0.020**	(0.007)	**0.321**	(0.104)	**0.897**	(0.136)	**0.009**	(0.002)

Notes: This table shows estimates corresponding to those in Figures 7-9.

Table A3

Estimated effects on subquestions that enter into the social wellbeing score

	Coefficient estimate	Standard error
How well do you like your school?	**-0.092**	(0.019)
How well do you like the other children in your classroom?	**-0.060**	(0.019)
Do you feel lonely? (reverse coded)[*]	0.022	(0.021)
Are you afraid of being ridiculed at school? (reverse coded)	**-0.146**	(0.025)
Do you feel safe at school? [*]	**-0.064**	(0.021)
Since the start of the school year, did anyone bully you? (reverse coded)	**-0.136**	(0.020)
I feel I belong at my school.	-0.014	(0.022)
I like the breaks at school.	**-0.039**	(0.018)
Most of the pupils in my classroom are kind and helpful.	**-0.082**	(0.020)
Other pupils accept me as I am.[*]	**-0.092**	(0.021)
N	49,858	

Notes: The table shows the results from comparative event study estimation using 2015-2017 data for reform cohort and 2015-2016 data for comparison cohort. Model controls for linear time trend. [*] indicates that measure reflects aspects of emotional stability; see Andersen et al. (forthcoming). **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

Table A4

Reports to municipal social services: reasons for concern and types of informants

	Reason for concern | concern
Drug abuse, child	0.020
Crime, child	0.031
Problems at school, e.g., absence	0.080
Other child problem behaviors; e.g, externalizing behaviors	0.232
Disability, child	0.029
Abuse (sexual, violence) towards child	0.060
Other type of abuse or neglect	0.094
Drug abuse, parents	0.114
Crime, parents	0.010
Disability, parents	0.062
High level of conflict or violence between adults at home	0.126
Insufficient care from parents	0.178
Residential tenant eviction, homelessness	0.053
Other reason	0.207
Type of informant | reason for concern:	
School	0.203
Health care provider	0.124
Anonymous	0.093
Relative, child in question, or acquaintance	0.087
Municipal transfer in connection with moves	0.077
Police or court	0.068
Day care institution	0.064
Other	0.284
# observations	18,322

Notes: This table shows distribution of reasons for concerns given that a concern was raised as well as the distribution of types of informants. Population is children of mothers on welfare in March 2016.