Welfare Reform: Consequences for the Children

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2024

Abstract

This paper uses register-based data to analyze the consequences of a recent major Danish welfare reform for children's academic performance and well-being. In addition to work requirements, the reform brought about considerable reductions in welfare transfers. 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 decreased somewhat as a consequence of the reform, just as we observe a small increase in hours worked. At the same time, we do not detect effects on short-run child academic performance. We do find smaller negative effects on children's self-reported school well-being and document substantial uptics in reports to child protective services for children exposed to the reform.

Acknowledgements: We appreciate comments from Helena Skyt Nielsen and Marianne Page. We also received valuable inputs from participants in the 5th Family and Education Workshop, the 2020 Copenhagen Education Network Workshop, the 2021 IRP Summer Research Workshop, the 2021 IIPF Annual Congress, the 2022 CESifo Area Conference on Economics of Education 2022, the Canadian Economic Association Conference 2023, as well as seminar participants at SOFI, IFN, Stavanger University, and Aarhus University. We gratefully acknowledge funding from the Independent Research Fund Denmark. The usual disclaimer applies.

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 income loss. Moreover, it has long been recognized (e.g., Holmes and Rahe, 1967; Milligan and Stabile, 2011) that parents as well as children tend to find shocks to family financial status particularly stressful. As such, welfare reform could have broader implications and affect family members, most prominently children, too.

This paper uses population-wide register data informative about parents' welfare participation coupled with a range of parental, partner, 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 current policy: if work requirements were not fulfilled, welfare recipients could lose all or part of their benefits. At the same time, absent any behavioral responses the upper limit on welfare benefits was, in and of itself, 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).

Relative to the existing literature in the area, we utilize high-frequent panel data on a rich palette of outcomes 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 the group of children whose mothers were on welfare at the time of the reform. A key advantage 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 monthly government transfers before and after the reform for the population of mothers on welfare benefits in March 2016. The figure then compares these to the transfers paid out exactly one year earlier but to the population of mothers on welfare in March 2015 – namely those who constitute the comparison group in our empirical strategy. The benefit levels of the two populations track each other closely, long before the reform was passed into law and continuing until the reform was implemented in October 2016 for the reform population. At that point in time, in contrast, we observe a sudden decrease in government transfer receipt of about 12 percentage points.

Our analysis documents that mothers' propensity to receive welfare only decreased somewhat as a consequence of the reform, just as we observe a small increase in hours worked. As a result of the reform, monthly household discretionary income[1] fell by just above € 300. We do not detect effects on short-run child academic performance, however, but do find smaller negative effects on children's self-reported school well-being, as measured by individual-level nationally administered well-being surveys. Moreover, we document substantial uptics in reports to child protective services for children exposed to the reform. Reports were commonly due to child externalizing behaviors, insufficient care by parents, or high levels of conflict in the family. Analyses of supplementary outcomes related to school absence rates and the prevalence of injuries support our main conclusions.

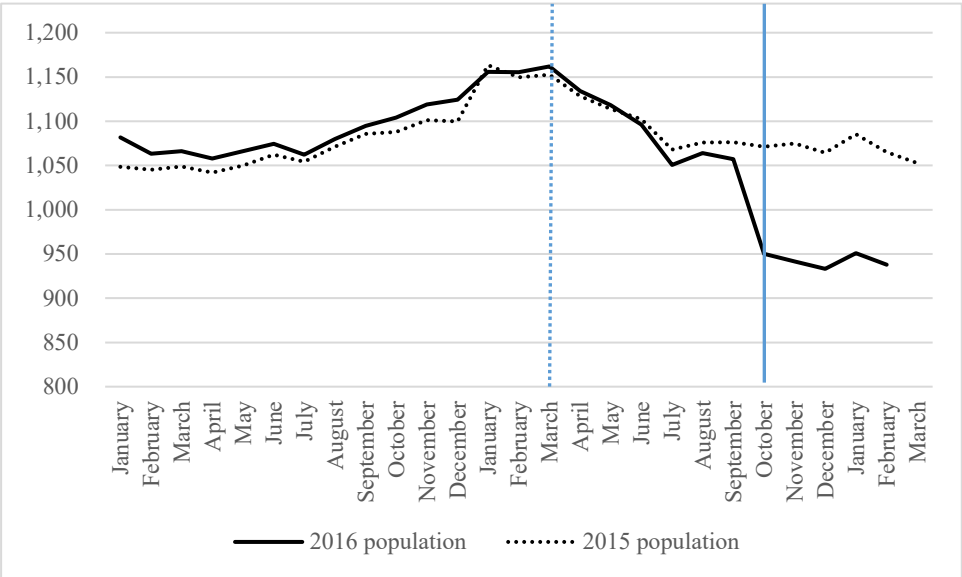
Our strategy relies, among other things, on the absence of other concurrent changes. Having first confirmed that any effects on the inflow into welfare were miniscule, we therefore investigate trends in outcomes in the non-welfare population. Importantly, our main conclusions are robust to the implementation of a triple comparative event study that accounts for overall trends in outcomes in the non-welfare population too.

We structure the remainder of the paper as follows: Section 2 describes prior knowledge on the link between welfare reform and child outcomes, while Section 3 is concerned with the institutional setting as well as the content of welfare reform in question. Section 4 explains our empirial strategy and Section 5 presents the data. Section 6 continues to show the results and Section 7 concludes.

[1] The sum of mother's and any partner's earnings and transfers minus taxes and rent.

Figure 1

Monthly sum of government transfers, household equivalent (€)



Notes: The figure shows amounts of cash benefits paid out to main population and their household in € (deflated using the consumer price index), while accounting for household size by dividing by the square root of family size. 2016 (2015) population consists of mothers of children aged 0-18 on welfare in March 2016 (2015) and corresponds to those in our estimation sample. The dashed vertical line indicates the passage of the reform (March 2016 (2015) for the 2016 (2015) population); the fully drawn vertical line indicates the timing of the reform implementation (October 2016 (2015) for the 2016 (2015) population).

2. The link between welfare reform and child outcomes

The direction of the effects of welfare reform that aims at promoting work, often through reductions in cash benefit levels and via work requirements, on child outcomes is ex ante unclear. To the extent that the tightening of welfare policies results in lower family income and more stress in the family, we expect that children will be harmed by such reforms. Several papers document that income (e.g., Dahl and Lochner, 2012; Aizer et al., 2016; Akee et al., 2018, Akee et al., forthcoming; Page, forthcoming) is important for children's emotional and behavioral health as well as later life human capital outcomes, maybe because higher income results in better home environments (e.g., Cesur et al. 2022). At the same time, early stressors (Almond and Currie, 2010) generally lead to worse child outcomes.

Still, some parents might take up work because of the reforms, which hold the potential to benefit children, for example because working parents can serve as positive role models for their children,

and because of the earned income (e.g., Heinrich, 2014). However, the empirical evidence on the causal link between parental (maternal) employment and child outcomes more generally 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).

Uncovering effects of various welfare reforms on child outcomes has been challenged by data disconnects between children and the relevant adults; 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. Our paper relates directly to a smaller literature concerned with the link between the tightening of welfare services and child human capital development. To the best of our knowledge, most other studies that include indicators of child outcomes 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). 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. Two recent studies suggest that the quality of the home environment decreased as a consequence of the tightened welfare policies from the 1990s. Reichman et al. (2020) exploit state and time variation in the US welfare reforms and document reductions in mothers' engagement in for example parent-child activities, while Kalil et al. (2022) find reductions in maternal emotional support. Miller and Zhang (2012) were the first to measure the impact of welfare reform 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.

Only a few studies employ data from outside the US. The paper that is closest to our in terms of child data availability is Hicks et al. (2022). That paper exploits a particular feature of the 2002 Canadian welfare reform that reduced the likelihood for mothers to be on welfare, namely a reduction in the age of the youngest child (from age seven to three) at which a mother on welfare was required to search for work to speak to the link between welfare receipt and child outcomes. Their empirical strategy essentially relies on a comparison of families where the youngest child was aged 4 to 6 (the

treatment families) to a group families who were required to search for work also prior to the reform; those with a youngest child aged 8 to 11. They find evidence that children's visits to general practitioners declined but find no effects on educational attainment or on contacts with the Ministry of Child and Family Development that for example arranges for foster care and offers family interventions. Our paper clearly distinguishes itself from Hicks et al. (2022) in the type of parameter of interest. Instead of considering effects of welfare enrollment per se, our paper concerns itself with the conditions for those on welfare more broadly: the reform that we study not only made it less attractive to be on welfare but also reduced the level of cash benefits to those who remained on welfare. Morris and Michalopoulos (2000) is another example. They provide experimental results from the Canadian Self-Sufficiency project that offered a temporary but generous earnings supplement with full time work and show mixed results that vary with child age. Again, in contrast to ours, that paper is concerned with the consequences of leaving welfare. Finally, Løken et al. (2018) investigate the effects of a 1998 Norwegian reform targeted single mothers. The reform was implemented over a period of three years and 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 study uses a difference-in-difference strategy that compares children of single mothers with those of married mothers. Løken et al. (2018) find no effects on school grades in the overall population but statistically significantly negative effects among children of younger mothers. Hence, the existing evidence-base is small, based on varied policy-designs and levels, and results in conflict.[2]

Our paper also speaks to a broader literature that is concerned with the consequences of providing childhood access to (near) cash welfare by way of social safety net programs[3], without a direct focus on parental employment, for child well-being and human capital accumulation (e.g., Aizer et al., 2016; Bastian and Michelmore, 2018; Bailey et al., 2020). This literature tends to find gains from access in terms of child human capital accumulation. A different vein of work is concerned with intergenerational spillovers in disability insurance; a recent paper by Dahl and Gielen (2021) based

[2] 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.

[3] For example via the introduction of the Mothers' Pension program (1911-1935) or the county-level roll-out of the Food Stamps program between 1961 and 1975.

on Dutch data finds that limiting access to disability insurance for parents reduces their children's take-up of disability insurance and improves a range of other outcomes.

3. *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 welfare benefits ("kontanthjælp") but also housing support[4] and special support ("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 welfare benefits, housing support and special support) and it required that welfare recipients had worked at least 225 hours (ordinary hours only, not including subsidized employment; six weeks full time) during the last 12 months for them to remain eligible for benefits, with the counting of hours starting in April 2016. In practice, the upper limit was set such that only individuals who received both welfare benefits, some housing support and some special support were at risk of facing the upper limit. As shown above, cash benefit level decreased substantially because of the reform, regardless of cohabitation status.

Clearly, a lower level of benefits lowered the attractiveness of staying on welfare but the upper limit on total transfers also created a more subtle, additional incentive to work for those affected by the

[4] Housing support 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.

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.

4. Empirical strategy

The goal of the paper is to study the consequences of the welfare reform for mothers' and ultimately children's outcomes. To learn about the full effects of the reform on household resources, we also analyze effects on partner 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. 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 saw in Figure 1 and also document below, there are clear within-calendar-year dynamics in welfare participation that are not related to the reform. These are partly due to usual seasonal variation in employment but also due to a sampling-driven version of Ashenfelter's dip: we *require* that mothers receive welfare in March 2016 for them to be part of our sample. To account for such seasonal patterns 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.

[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. We do not have information about who were exempt and would, in any case, never know about this counterfactual scenario for the comparison group.

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 well ahead of the reform, with the exact starting point depending on data availability for each outcome. We never go more than 15 months back to avoid interference from a January 2014 welfare reform that primarily targeted the youngest welfare recipients (<30 years old).[7] We continue our analysis until a year after the passage of the reform. The latter choice 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:

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

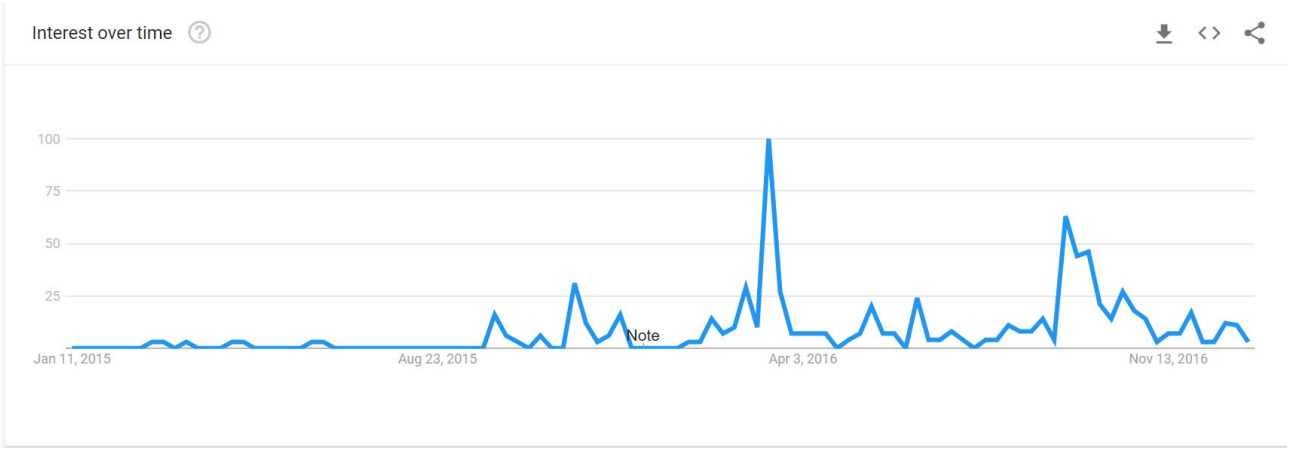
where Y is the outcome of interest, δ_j are event time dummies, and $reformpop$ indicates that individuals belong to the cohort actually exposed to the reform. ε is an error term, i indicates individuals, and t indicates time. γ are the parameters of interest; they represent the effects of the reform 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 cluster standard errors at the individual level.

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 show 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.

[7] In principle, both our reform and comparison populations are subject to the content of the previous reform but that reform relied heavily on changes to caseworker behavior that are likely to occur gradually. For that reason, we might even see some differential pre-trends very early on.

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 effects of 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 and account for concurrent changes, we also consider the development over time in outcomes for the non-welfare population in a triple comparative event study.

For some child outcomes, we deviate slightly from this ideal baseline specification that employs a balanced panel of individual level outcomes. We do this when the measurement of the outcomes (such as test scores) is closely tied to the age or grade of the child. In these cases, we instead consider the relevant cohorts at each point in time.

5. *Data, samples, and descriptive statistics*

[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.

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 to connect 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, especially when events are rare. In these cases, we aggregate to a slightly higher level. Key to our project is, of course, monthly information about welfare participation, benefit payments, and labor market outcomes such as hours worked, which we draw from the National Income Register.

In terms of distinct child outcomes, we analyze domains that reflect aspects of child cognitive and noncognitive skills (e.g. Carneiro and Heckman, 2003) as well as the quality of the home environment. We specifically focus on outcomes that we believe are malleable and indicative of the current situation but also likely to be important for future succes. We start out by considering children's academic outcomes. We base these on standardized versions of the nationally administered performance tests in Danish reading and math that have been shown to correlate positively with future academic succes (Beuchert and Nandrup, 2018).[9] In practice, 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 public 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 on the specifics of the tests.

Another outcome uses indicators of children's well-being 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 with the purpose of learning about classroom and school-level well-being. Crucially, the school management and teachers do not have access to the individual level responses. In our analyses, we focus on the social well-being scale that is collected for children enrolled in public schools in grades 4-9. The responses to all questions are always coded to range from one to five, with five being the most positive. In practice, we calculate the average

[9] Beuchert and Nandrup (2018) also document that the results from the national tests correlate with SES in the expected way.

response across all questions for each child and our well-being measure therefore ranges from 0-5.[10] As was the case for the national tests, social well-being has also been shown to correlate positively with academic performance, though estimates are not very large; see Larsen et al. (2020).

Our final main outcome regards reports to child protective services because of concerns for the child in question. Child maltreatment has consistently been shown to correlate with – or even cause – future risky behaviors (e.g., Gilbert et al., 2009a; Currie and Tekin, 2012). The data from child protective services include both the date, the reason for the concern, and the type of informant. Anyone can express concern, and the report can be anonymous. We use absence data from public schools, provided by the Danish Ministry of Education, as well as information about child injuries from hospital admissions data to explore supplementary outcomes descriptive of the behaviors associated with the reports to child protective services.

For both adults 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 33,960 mothers of children aged 0-18 who were on welfare in March 2016. We further exclude recent immigrants because of a concurrent reform of welfare benefits avaible to this particular group[11] and condition on mothers being at risk of benefit cuts, i.e., as explained above, those who received both welfare benefits, some housing support and some special support. Our final 2016 sample – our reform group – consists of 18,578 mothers and 36,820 biological children. Table A1 shows our sample loss journey in detail.

[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.

[11] Those who had not been living in the country for at least seven out of the last eight years.

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. Table 1 shows that mothers on welfare in March 2015 and 2016 resemble each other closely in terms of a wide range of observable characteristics. To grasp the potential importance of the 225 hours requirement for the population under study, note that mothers on welfare worked on average three hours in March. Accordingly, working 225 hours per year would constitute a substantial change.

Table 1

Descriptive statistics, mothers

	Welfare recipients		Standardized difference	Overall population, adult women	Standardized difference
	2015	2016		2016	
Sample size	19,716	18,578		616,310	
Age	37.57	37.79	0.03	40.50	-0.34
	(8.3)	(8.5)		(7.5)	
Number of children	2.48	2.47	-0.01	2.08	0.33
	(1.4)	(1.4)		(0.9)	
Number of children < 18 years of age	2.01	2.00	-0.01	1.76	0.25
	(1.1)	(1.1)		(0.8)	
Household size	3.10	3.09	-0.01	3.77	-0.55
	(1.4)	(1.4)		(1.1)	
Dwelling size (m2)	102.63	102.56	0.00	136.05	-0.57
	(42.6)	(42.1)		(71.9)	
Living arrangement (%)					
... Married	11	11	-0.02	61	-1.23
... Living w/ father of (at least one) child	9	9	0.01	16	-0.21
... Living w/ another adult	2	2	0.00	4	-0.11
...Single	78	78	0.01	19	1.46
Ethnicity (%)					
... Danish	68	67	-0.02	85	-0.44
... Immigrant	29	30	0.02	13	0.41
... Decendent	3	3	0.02	1	0.12
Educational attainment, %					
... primary and lower secondary	60	60	0.00	12	1.15
… upper secondary	29	29	-0.01	38	-0.20
... some tertiary	6	7	0.02	49	-1.07
... missing	5	5	0.00	1	0.22
Years of schooling	11.39	11.41	0.01	14.77	-1.32
	(2.4)	(2.5)		(2.6)	
Labor market experience, years	3.39	3.28	-0.02	12.74	-1.36
	(4.8)	(4.7)		(8.6)	
Hours worked, March					
... Total	3.34	3.00	-0.02	111.30	-2.16
	(18.7)	(16.9)		(68.9)	
... Unsubsidized	2.77	2.63	-0.01	109.61	-2.10
	(16.7)	(15.5)		(70.5)	
Household income March, household equivalence (€)					
Labor income	117	108	-0.02	3736	-0.85
	(436)	(381)		(5,989)	
Government transfers	1156	1162	0.02	256	0.71
	(292)	(436)		(1,739)	
Discretionary income	822	812	-0.02	N/A	
	(433)	(499)			

Table 1 ctd.

Descriptive statistics, mothers

	Welfare recipients		Standardized difference	Overall population, adult women	Standardized difference
	2015	2016		2016	
Assets and liabilities, household equivalence (€)					
owner of property (%)	2.95	2.49	-0.03	71.91	-2.06
... Value of property	2198	1953	-0.02	136077	-0.64
	(16,568)	(15,156)		(297,354)	
… value of mortgage	1050	825	-0.03	87078	-0.67
	(9,376)	(8,141)		(181,272)	
Owner of vehicle(s) (%)	26	23	-0.06	79	-1.34
... Market value of car	886	360	-0.29	4083	-1.08
	(2,232)	(1,193)		(4,725)	
Financial assets (excl. pensions) (€)	25	16	-0.01	8120	-0.04
	(859)	(571)		(270,343)	
Pensions (€)	7245	6980	-0.03	56769	-1.07
	(11,779)	(8,733)		(65,209)	
Bank deposits (€)	916	901	-0.01	12238	-0.43
	(2,639)	(1,926)		(36,848)	
Unsecured loans (€)	10782	10851	0.00	25648	-0.20
	(21,525)	(19,752)		(104,682)	
Medical utilization previous 12 months, any (%)					
Inpatient hospital care:					
... Psychiatric	2.0	1.8	-0.01	0.3	0.15
... Related to reproduction	11	11	0.00	9	0.06
...(other) Somatic	16	15	-0.02	8	0.24
Outpatient hospital Care:					
... Psychiatric	11	11	-0.01	2	0.39
... Related to reproduction	15	15	0.00	11	0.11
...(other) Somatic	53	53	0.00	40	0.27
primary care provider visit	97	97	-0.01	93	0.20
Specialist visit	41	41	0.01	35	0.13
Urgent Care visit	22	23	0.02	13	0.26
Dentist visit	36	37	0.01	63	-0.54
Crime previous 12 months, any (%)					
Victim of					
... Sexual assault	0.1	0.2	0.02	< 0.1	0.05
... Violent assault	1.6	1.6	0.00	0.30	0.14
... property	1.5	1.6	0.00	0.78	0.07
charged with					
... Sexual assault	< 0.1	< 0.1	0.00	< 0.1	0.02
... Violent assault	0.55	0.56	0.00	0.06	0.09
... Property	2.57	2.27	-0.02	0.20	0.19
... DUI	0.32	0.27	-0.01	< 0.1	0.06
... Possesion w/ intent to distribute	0.43	0.36	-0.01	< 0.1	0.08

Notes: The table shows descriptive statistics for mothers on welfare in March 2015 and March 2016 and compares these to the overall population of women aged 18 or above. Family size adjusted discretionary income is defined as the sum of mother's and any partner's earnings and transfers minus taxes and rent, divided by the square root of family size

Table 1 also shows how this population compares to the overall population of Danish mothers not on welfare and clearly documents that the former group is severely disadvantaged in terms of background characteristics and attachment to the labor market. The mothers in are our data are less likely to be married and to cohabit, have lower educational attainment, and have much less work experience. Except for dental care, they also interact much more with health care professionals and are much more likely to be victims of or to commit crime. Appendix table A2-A3 show similar patterns for the biological fathers and for the current partners of the mothers.

Table 2 instead describes the children of the mothers on welfare in March 2015 and 2016 and also compares them to the overall population. As was the case for the mothers, the observable characteristics of children are balanced across the reform and comparison groups. Children of mothers on welfare are clearly disadvantaged compared to other children and this is obvious already from birth: their mothers were more likely to be overweight and to smoke, and their parents were more likely to be teenagers at the time of birth. They are also more likely to be enrolled in special schools and have vastly lower test performance. Moreoever, their risk of having at least one report to child protection services is six times higher than for a child in the overall population.

Table 2

Descriptive statistics, children

	Children of welfare recipients		Standardized difference	Overall population, children	Standardized difference
	2015	2016		2016	
Sample size	39,228	36,859		1,072,809	
Age	9.56	9.59	0.01	9.34	0.05
	(5.0)	(5.0)		(5.1)	
Female, %	49	49	-0.01	49	0.00
Age group, %					
... 0 - 5 years old	27	27	0.00	30	-0.06
... 6 - 12 years old	43	43	0.00	41	0.04
... 13 - 17 years old	30	30	0.01	29	0.02
Ethnicity, %					
... At least one parent is Danish citizen	65	65	-0.01	90	-0.64
... Born abroad to non-Danish parents	2	2	0.00	3	-0.04
... Born in DK to non-Danish parents	32	33	0.01	7	0.69
Living arrangement, %					
... Living w/ both parents	22	22	0.00	75	-1.26
... Living w/ mother and her new partner	4	4	-0.01	6	-0.13
... Living w/ single mother	62	63	0.01	15	1.12
... Living w/ father and his new partner	2	2	0.00	1	0.11
... Living w/ single father	5	4	-0.01	2	0.13
... Living w/ neither parents	6	6	0.01	1	0.29
Information from medical birth records					
% with records	96	96		94	
Mother's weight and height recorded, %*	54	59		60	
Birth weight, gram	3,352	3,346	-0.01	3,487	-0.23
	(615)	(611)		(610)	
Birth weight < 2,500 gram, %	7	7	0.00	5	0.08
Gestation age, days	276	276	0.00	278	-0.12
	(14)	(14)		(14)	
Gestation age < 224 days (32 weeks), %	1.19	1.13	-0.01	0.90	0.02
APGAR score	9.86	9.86	0.00	9.87	-0.01
	0.66	0.65		0.61	
Number of prenatal visits to midwife	4.70	4.58	-0.05	4.93	-0.16
	(2.2)	(2.3)		(2.1)	
Mother's BMI prior to pregnancy	25.51	25.49	0.00	24.40	0.13
	(9.1)	(9.2)		(7.9)	
Mother's BMI > 30, %	21	20	0.00	12	0.23
Mother smoking during pregnancy, %	43	43	-0.01	17	0.59
Father's age at time of birth	31.01	31.11	0.01	32.75	-0.25
	(7.2)	(7.3)		(5.7)	
Father teenager at time of birth, %	2	2	0.00	0.31	0.18
Mother's age at time of birth	27.42	27.59	0.03	30.23	-0.49
	(5.9)	(5.9)		(4.8)	
Mother teenager at time of birth, %	7	7	-0.01	1	0.30

Table 2 ctd.

Descriptive statistics, children

	Children of welfare recipients		Standardized difference	Overall population, children	Standardized difference
	2015	2016		2016	
Number of children of school age	25,016	23,360		635,890	
School form, %					
... Public School	80	79	-0.01	78	0.04
... Private school	10	10	0.02	18	-0.22
... Special ed school	5	5	0.01	2	0.19
... Home schooled	6	5	-0.02	3	0.12
Old for grade cond on privat or public scho	27	25	-0.04	14	0.30
New school current school year, %**	9	11	0.06	6	0.18
Test scores taken in grade 2, 4, 6, and 8					
Reading					
...N in sampling year	7,012	7,510		192,403	
... Standardized score	-0.53	-0.57	0.03	0.06	-0.43
	(0.97)	(0.97)		(0.96)	
Math					
...N in sampling year	3,709	3,879		99,273	
… Standardized score	-0.60	-0.61	0.01	0.06	-0.48
	(0.97)	(0.97)		(0.97)	
Wellbeing grade 4-9					
...N in sampling year	9,524	8,872		264,839	
Wellbeing average (1-5)	4.00	3.94	0.06	4.14	-0.15
	(0.68)	(0.68)		(0.61)	
Reports to child protective services 0-18				1,068,477	
Any report in Q1	0.073	0.080	-0.02	0.012	0.22

Note: The table shows descriptive statistics for children of mothers on welfare in March 2015 and March 2016 and compares these to the overall population of Danish children. Test scores, self-reported social wellbeing, and reports to child protective services are measured in the spring of 2015 (2016) for the 2015 (2016) population.

*Mothers' height and weight are only recorded from 2004 and onwards

** Conditional on private or public school attendance

6. Consequences of welfare reform

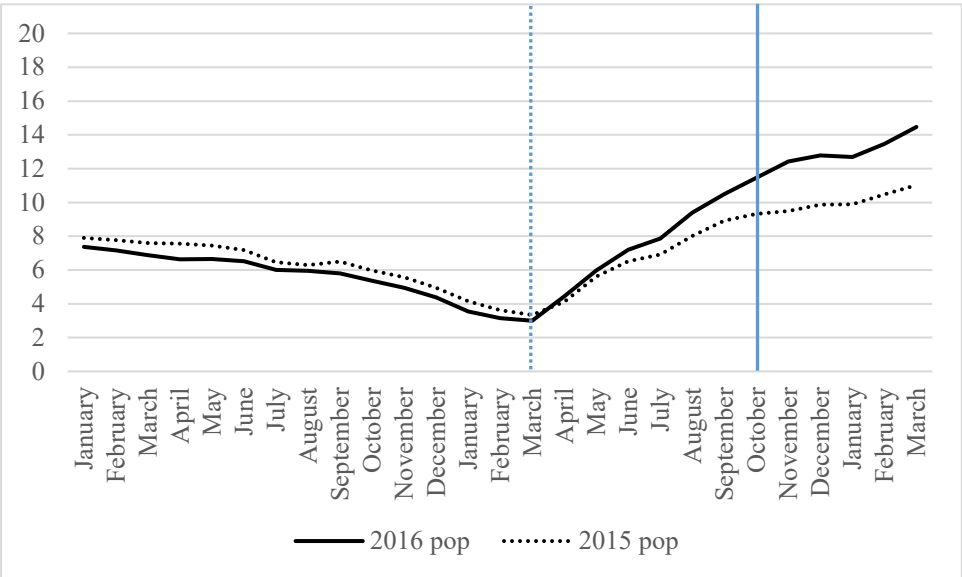
This section shows our main empirical findings. Since the overall purpose of the reform was to incentivize labor market participation, and ultimately lower levels of welfare participation, we start by exploring these margins for the mothers. Equipped with those insights, we then move to explore the consequences for child academic outcomes and wellbeing. We finally perform a range of robustness analyses and investigate heterogeneity across subsamples.

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

Results on mothers' hours worked appear in Figure 3. The upper panel shows the differences in hours worked over time for our reform and comparison groups. The lower panel shows the estimated effects of the reform from the comparative event study estimation anchored in February just prior to the passage of the reform. Note first that the estimated effects of the reform are essentially zero in the months prior to its passage 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. In an absolute sense, we estimate small, positive effects on hours worked as a consequence of the reform. In March 2017, for example, the estimated effect is 4 hours. In a relative sense, effects are large: four hours correspond to as much as 35% of the comparison mean at that point in time. Still, to put this into perspective, only 13% of mothers on welfare benefits in March 2016 managed to accumulate at least 225 hours in the 12 months from April 2016. Mothers typically worked in low-skilled jobs, for examples as aides in elderly care (17% of mothers with some employment in 2016); as cleaning assistants (12%); and as pedagogical assistants (9%); see Table A5.[12]

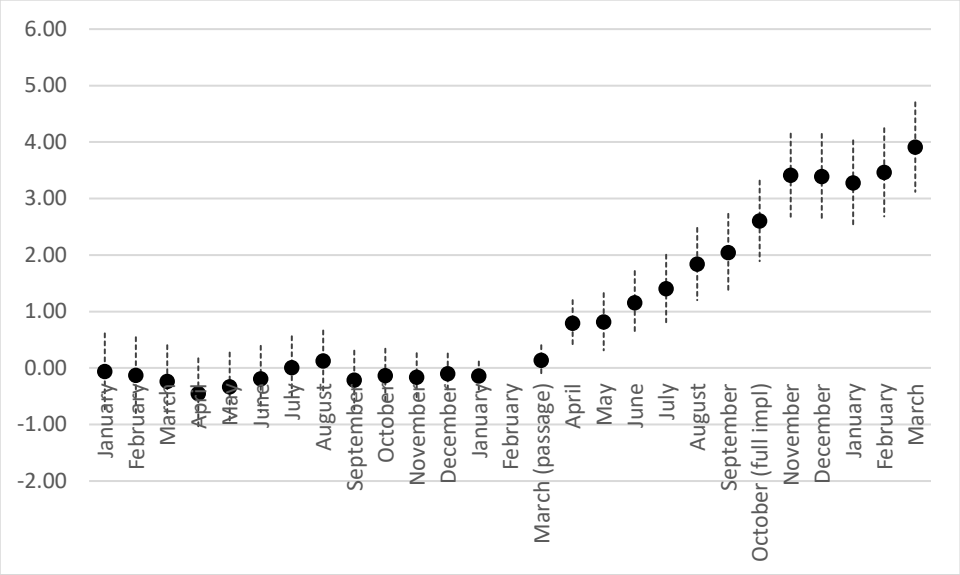
Figure 3

Effects of the reform on mothers' hours worked



[12] We have also explored the extent to which the reform affected the propensity to work at all (any hours in a given month); to work part time (at least 80 hours per month); and to work full time (at least 160 hours per month). We find that the reform mostly affect the tendency to work at all (about 5 percentage points more in March 2017 compared to a mean in the comparison group of 10%) but also had some effect on part time work (an increase of 2 percentage points compared to a mean in the comparison group of 7%). The reform had no impact on full time work, however. The full set of results is available upon request.

Panel A: Mean hours worked



Panel B: Estimated effects of the reform

Note: Panel A shows mean hours worked; Panel B shows the estimates and 95%-confidence intervals from a comparative event study estimation anchored in February, just prior to the passage of the reform. 2016 (2015) population consists of 18,578 (19,716) mothers on welfare benefits in March 2016 (2015). The dashed vertical line in Panel A indicates the passage of the reform (March 2016 (2015) for the 2016 (2015) population); the fully drawn vertical line indicates the timing of the reform implementation (October 2016 (2015) for the 2016 (2015) population). The full set of estimates is shown in Table A4.

Figure 4 is instead concerned with mothers' welfare participation. Pretrends are well-aligned for the nine months (August and onwards) prior to the reform. We suspect that the small negative estimates of roughly 1 percentage point in the months long before the passage of the reform may be driven by the implementation of the January 2014 reform discussed above. After the reform – from July and onwards, and particularly from October when the reform was fully implemented – we detect significantly negative effects on welfare participation. Again, effects are small in an absolute sense: the estimate in March 2017 is -0.082, corresponding to 10% of the comparison group mean.

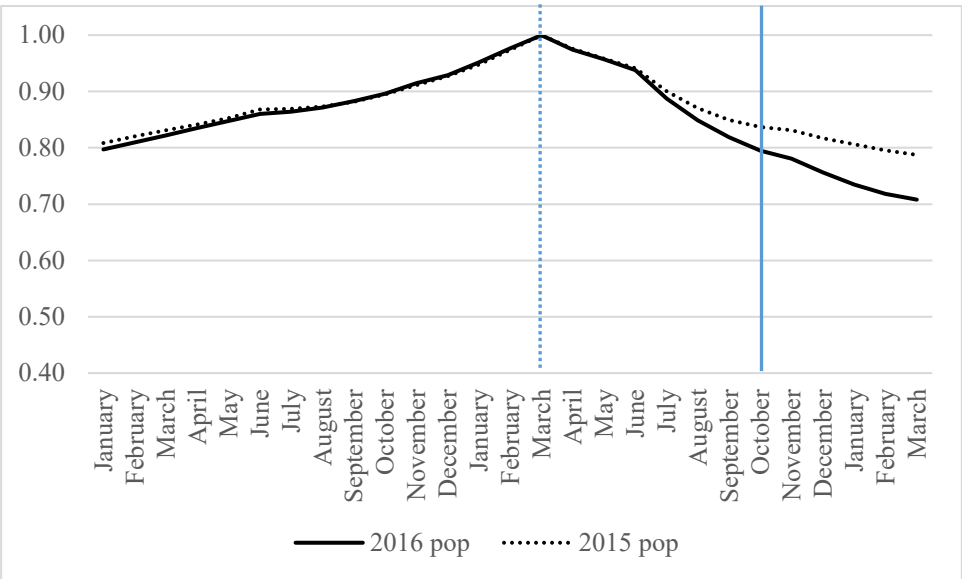
In short, some but not many mothers in this population managed to leave welfare entirely and only a smaller share increased their labor supply to an exent sufficient to avoid potential monetary sanctions associated with the 225 hours requirement. The timing of the responses suggest to us that mothers in this population are rational and forward looking but, since there were no anticipatory responses, either

derive substantial disutility from work or are just not well-informed about upcoming changes to current policies.

Figure A2 in Appendix A shows that the reform also affected the mothers' partners (of whom about half were also on welfare in March, just prior to the reform). As for the mothers, we estimate small, positive effects on hours worked of around six and small, negative effects on welfare participation of roughly six percentage points one year after the passage of the reform. 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. One year after the passage of the reform, we estimate that monthly discretionary household income[13] was on average lower by just above €300 because of the reform; see Table A6.

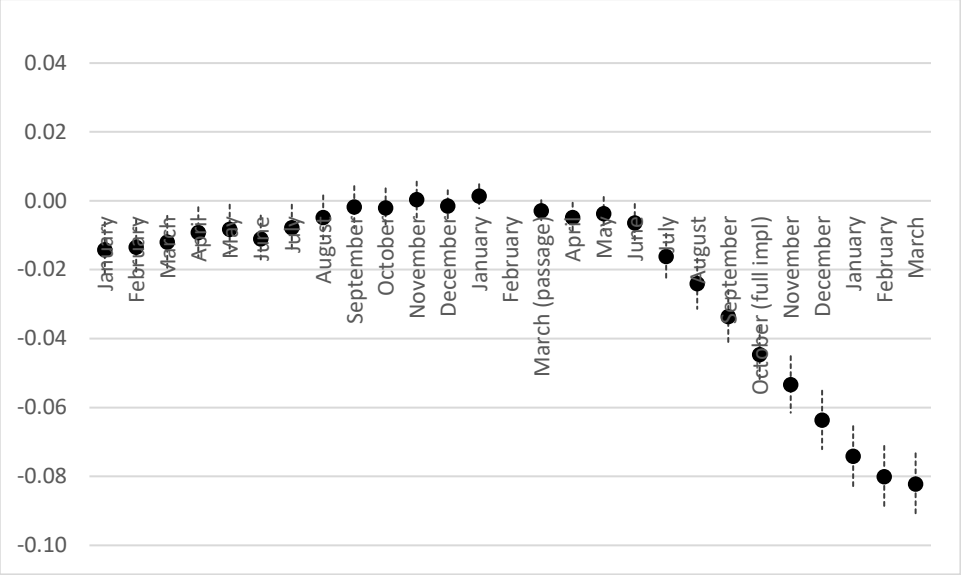
Figure 4

Estimated effects of the reform on mothers' welfare participation



Panel A: Share on welfare

[13] As noted above, discretionary income is defined as the sum of mother's and any partner's earnings and transfers minus taxes and rent.



Panel B: Estimated effects of the reform

Notes: Panel A shows the share receiving any welfare benefits; Panel B shows the estimates and 95%-confidence intervals from a comparative event study estimation anchored in February, just prior to the reform. 2016 (2015) population consists of 18,578 (19,716) mothers on welfare benefits in March 2016 (2015). The dashed vertical line indicates the passage of the reform (March 2016 (2015) for the 2016 (2015) population); the fully drawn vertical line indicates the timing of the reform implementation (October 2016 (2015) for the 2016 (2015) population). The full set of estimates is shown in Table A4.

6.2 Effects of the reform on child outcomes

Given insights into how the reform affected mothers' labor market attachment and family resources, specifically resulting in lower discretionary income but slightly more hours worked, we move on to investigate effects on child outcomes measuring academic performance as well as indicators of wellbeing.

Table 3 first shows the results for academic performance as measured by the national test scores in Danish reading and math.[14] In parallel to the adults, we consider children born to mothers who were on welfare in March of 2016 (2015) to be the treated (comparison) cohort. Importantly, we detect no negative effects on test scores as a consequence of the reform; if anything, our results actually indicate

[14] We have tested whether the reform affected test-taking for the national tests and survey-taking for the social wellbeing measure. We did not find evidence of this. 91.8% (91.3%) of children in the reform (comparison) population take the national test in Danish reading in 2016. 87% of children in both the reform and comparison population answer the social wellbeing survey.

small but statistically insignificant upticks in academic performance corresponding to 1.5% of a standard deviation in Danish reading and 3.1% in math. It is possible, of course, that we fail to estimate strong results since test scores to some extent reflect skills that accumulate over time and that immediate learning – and well-being – may still be worse because of the reform.[15]

Table 3

Estimated effects of the reform on test scores

	Danish reading		Math	
Variables	Coefficient	Standard error	Coefficient	Standard error
Reform population indicator	0.031	0.018	0.019	0.022
Time indicators:				
One year prior	0.000	0.017	0.034	0.022
One year after	**0.034**	0.017	-0.021	0.022
Effects of the reform:				
One year prior	-0.023	0.031	-0.056	0.037
One year after	0.015	0.030	0.031	0.037
Constant	**-0.569**	0.012	**-0.614**	0.016
# children, reform population	17,240		11,422	
# children, comparison population	18,136		12,249	

Notes: The table shows the results from comparative event study estimation using 2015-2017 data for the reform cohort and 2014-2016 data for the comparison cohort. The reform (comparison) cohort consists of children of mothers on welfare in March 2016 (2015), enrolled in public schools and at grade-levels where testing is carried out. The analysis is anchored in Q1, just prior to the passage of the reform. Post-measurement is Q1 2017 (2016 for the comparison cohort). **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

To probe into contemporaneous consequences for children's state of mind, we next explore self-reported social well-being, constructed from the nationally administered well-being surveys developed by the Danish Ministry of Education. The survey is gathered each spring, which implies that we consider the spring of 2016 (2015) as the pre-period for the reform (comparison) population. Social well-being is our only measure that relies entirely on children's own reports and informs us about how they perceive to be thriving but one should keep in mind that the questions asked primarily concern children's well-being at the school domain, not at home. As shown in Table 4, we detect a

[15] Landersø et al. (2020) 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.

negative effect on well-being of 0.06 points, corresponding to 8% of a standard deviation or just below 50% of the difference between the population of children on welfare and other children; see Table 2.[16]

Table 4

Estimated effects of the reform on self-reported social well-being

Variables	Coefficient	Standard error
Reform population indicator	0.009	0.005
Time indicator:		
One year after	0.010	0.005
Effect of the reform:		
One year after	**-0.055**	0.013
Constant	**3.99**	0.008
# children, reform population	14,729	
# children, comparison population	13,170	

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. The reform (comparison) cohort consists of children of mothers on welfare in March 2016 (2015) who were enrolled in public schools. The analysis is anchored in Q1, just prior to the passage of the reform. Post-measurement is Q1 2017 (2016) for the reform (comparison) cohort. Model controls for linear time trend but this does not impact the estimated effect of the reform. **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

As described above, the school management and teachers do not have access to the individual level responses, yet children may still be concerned that their reports will reflect negatively on their parents and this loyalty conflict may impact their responses.[17] In the US, for example, fewer than one percent of the reports to child protection services are made by the victims themselves, indicating that children are reluctant to reach out to authorities about serious family issues; see Gilbert et al., (2009b). If children worry about bringing issues at home to the attention of professionals, analyses of children's wellbeing that relate to changes in their parents' circumstances cannot only rely on children's self-reports. Partly for this reason but also to explore effects on a more severe outcome indicative of children's wellbeing and the quality of the home environment, we analyze an alternative measure.

[16] Appendix Table A7 shows the results for each of the sub-questions included in the social wellbeing score. We observe small effects across all question.

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

Specifically, we consider reports to child protective services because of concerns for the child in question. The upper panel of Figure 5 reveals that children in our sample faces a quarterly risk of having at least one report made to child protective services of between seven and eight percent.[18] Table A8 indicates that the most common reasons for concerns in our population are child externalizing behaviors (22% of all reports); insufficient care from parents (16%)[19]; and high levels of conflict in the family (13%). Informants are primarily school staff (21%) or health care providers (13%), though anonomous informants are also very common (8%).

The formal results are shown in the lower panel of Figure 5. We follow our approach from above and consider the first quarter of 2016 (2015) as the pre-period for the reform (comparison) population. Note that because the gathering of this particular data source starts in 2015, we are unable to explore pretrends. Results show statistically significant increases in reports to child protective services, coinciding with the implementation of the reform. In an absolute sense, estimates are not very large (.8 percentage points in the quarter of the reform; 1.9 percentage points in the quarter following the reform) but they are substantial in a relative sense (10 percent of the comparison mean in the quarter of the reform; 26% in the quarter following the reform).

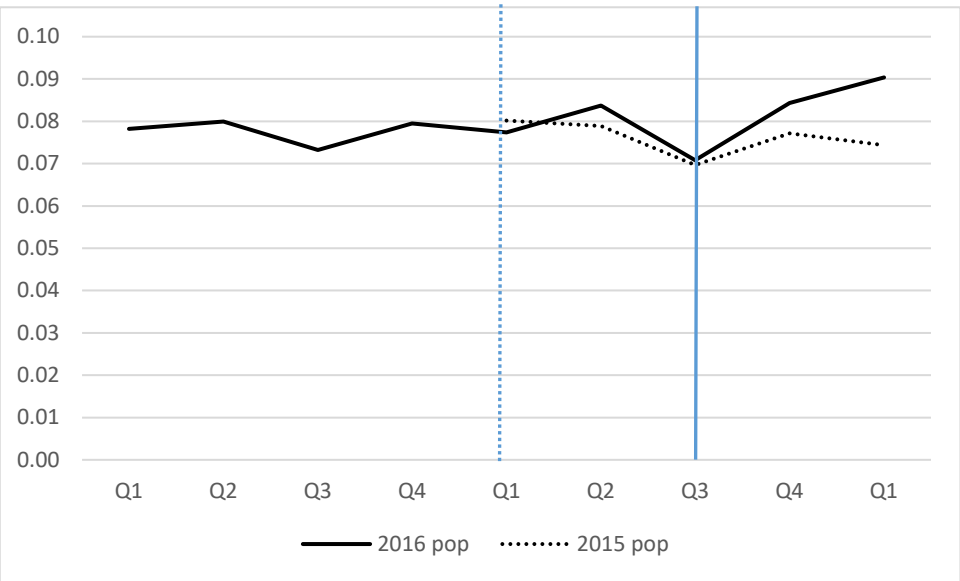
Taken at face value, these results indicate that children are much more likely to come to the attention of professionals because of the reform. This can either be because of behaviors in the child that lead professionals, for example teachers at schools, to react or because of increased risks at home that are directly detected by individuals who engage with the family. Interestingly, moreoever, 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.

[18] The number of reports to child protection services have generally increased over the period that we study. This is most likely because of early reforms ("The Child's Reform" (Barnets Reform) from 2011 and "The Abuse Reform" (Overgrebspakken) from 2013). Own calculations show that the increase is driven by the number of reports for a given child but not by an increase in the number of children with any report.

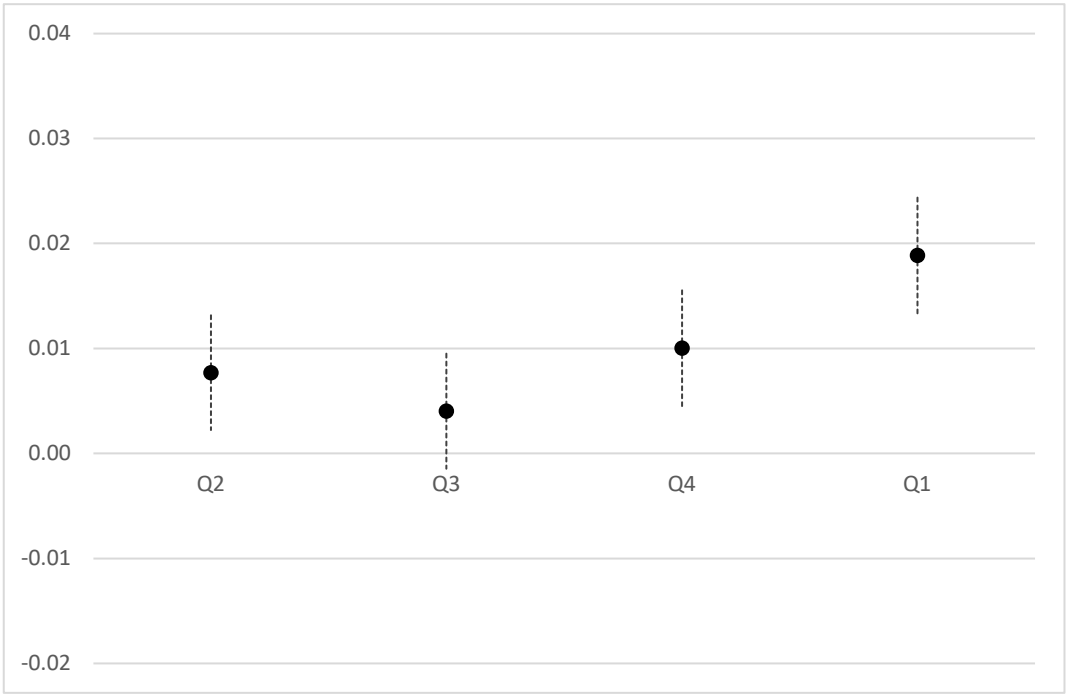
[19] Insufficient care relates to factors such as physical care (lack of food, clothes, and protection), emotional care (love and affection), developmental care (basic educational needs), and access to appropriate medical care. See Gilbert et al. (2009) and the Danish national guidelines for medical staff here: https://www.sundhed.dk/borger/patienthaandbogen/boern/sygdomme/socialpaediatri/omsorgssvigt-og-overgreb-mod-boern-og-unge-diagnostik/.

Figure 5

Estimated effects of the reform on reports to child protective services



Panel A: Share with at least one report made



Panel B: Estimated effects of the reform

Note: Panel A shows the share of children with at least one report to child protictive services; Panel B shows the estimates and 95%-confidence intervals from a comparative event study estimation anchored in Q1, the quarter just prior to the passage of the reform. 2016 (2015) population consists of 36,859 (39,228) children of mothers on welfare benefits in March 2016 (2015). The dashed vertical line indicates the passage of the reform (Q1 2016 (2015) for the 2016 (2015) population); the fully drawn vertical line indicates the timing of the reform implementation (Q3 2016 (2015) for the 2016 (2015) population).

The full set of estimates is shown in Table A9. Sample size: 577,256.

6.3 The role of concurrent changes: a potential threat to the validity of the design

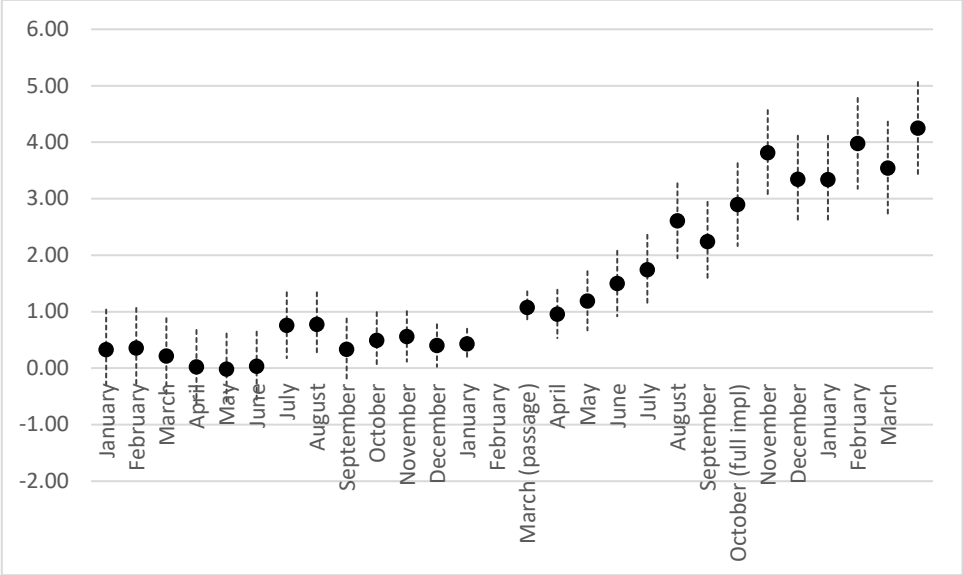
A possible important concern relates to changes in context across the various outcome domains that coincide with (or postdate) the reform and thus may conflate our findings. Such changes could, for example, include shocks to labor demand; epidemics; or even revisions of the institutional setting. To 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 populations of mothers who were *not* on welfare in March 2016 (and their children) to capture concurrent events for the reform sample and the population not on welfare in March 2015 to capture trends for the comparison sample. To investigate whether this population was affected by the reform, we first explore whether the reform affected inflows into welfare for mothers. Figure A1 shows the share of all mothers on welfare benefits during 2015 and 2016 as well as the difference between these shares. The share on welfare is essentially flat throughout 2016 and difference between the shares across the two years is also close to constant.

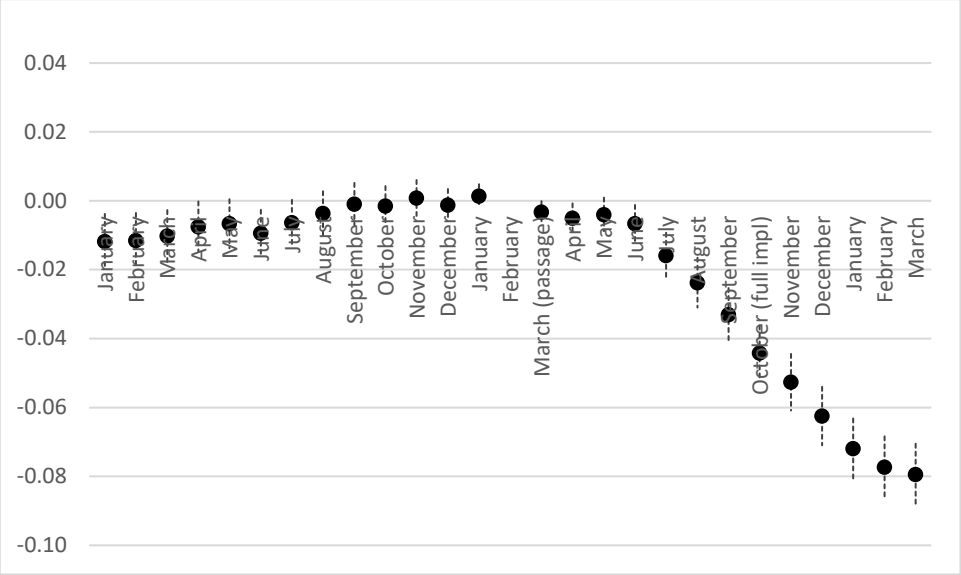
We present the results for the outcomes from above based on the triple comparative event study for mothers' outcomes in Figure 6. Panel A shows the results for hours worked, while Panel B depicts those for welfare participation. Fortunately, there is no evidence that our findings were driven by other concurrent events.

Figure 6

Estimated effects of the reform on mothers' outcomes, triple comparative event study



Panel A: Mothers' hours worked



Panel B: Mothers' welfare participation

Note: This figure shows the estimates and 95%-confidence intervals from a triple comparative event study anchored in February, just prior to the reform. The approach incorporates to the original sample the population of mothers not on welfare benefits in March 2015 and March 2016. Panel A shows the results for mothers' hours worked and Panel B those for welfare participation. The full set of estimates is shown in Table A4. 18,578 mothers on welfare make up the 2016 population exposed to the reform, while 19,716 mothers on welfare make up the 2015 comparison population. 616,310 mothers are in the non-welfare 2016 population and 616,789 mothers are in the non-welfare 2015 population.

Table 5 continues to show estimates from the triple comparative event study for children's outcomes. The estimated effect on Danish reading actually increases from 1.5 standard deviations in Table 3 to 4.8 standard deviations. Moreover, the coefficient associated with social wellbeing is essentially zero in this specification. The conclusions for math and reports to child protective services are similar to those based on our analysis from above; the estimated effect on math is still small and statistically insignificant; and the estimates associated with reports to child protective service only change at the third decimal point.

Table 5

Estimated effects of the reform on childrens' outcomes, triple comparative event study

	Coef.	Std. error
Danish reading		
One year after	**0.048**	0.018
# children, reform population	17,240	
# children, comparison population	18,136	
# non-welfare 2016 children	453,159	
# non-welfare 2015 children	454,386	
Math		
One year after	0.009	0.023
# children, reform population	11,422	
# children, comparison population	12,249	
# non-welfare 2016 children	301,225	
# non-welfare 2015 children	301,523	
Social wellbeing	0.004	0.009
# children, reform population	14,729	
# children, comparison population	13,170	
# non-welfare 2016 children	388,754	
# non-welfare 2015 children	330,797	
Child protective services		
1st quarter after	**0.006**	0.003
2nd quarter after	0.004	0.003
3rd quarter after	**0.009**	0.003
4th quarter after	**0.017**	0.003
# children, reform population	36,859	
# children, comparison population	39,228	
# non-welfare 2016 children	1,073,923	
# non-welfare 2015 children	1,069,701	

Note: This table shows the estimates and 95%-confidence intervals from a triple comparative event study that incorporates to the original sample the population of children of mothers not on welfare benefits in March 2015 and March 2016. The analyses are all anchored in the quarter just prior to the passage of the reform. Post-measurements for Danish reading and social wellbeing are taken in Q1 2017 (2016 for the comparison cohort). Post-measurements for child protective services are taken in the four quarters following the passage of the reform. Social wellbeing model controls for linear time trend but this does not impact the estimated effect of the reform. **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

6.4 Refined insights: heterogeneity analyses and alternative mother and child responses

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 (single parent versus married or cohabiting parent). 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. Finally, for reports to child protective services that exist across the entire child age range, we explore the role of child age.

There are good reasons to think that effects on the children may vary with the mother's civil status. To the extent that a partner can help alleviate any stress incurred by the reform and assist with means of income, we expect that children in families are more resilient to reform exposure.[20] And importantly, as seen in the first two columns of Table 6: the reform increases reports to child protective services more (and earlier on) in single headed households. We also detect non-negligible positive effects on test scores for children in households with more than one adult, with the estimate associated with math being significant at the 10%-level.

We do not find differences across gender in terms of academic outcomes or social wellbeing, but it is clear from Table 6 that the increase in reports to child protective services is driven by the boys: the estimates are much larger and only statistically significant for boys, too.[21] With regards to age, effects on child protective services are also generally larger for children aged 6-14 both compared to the preschool children, who are generally less visible to outsiders, and to the older children in the sample (15-18), who are more independent and can more easily escape from high levels of conflict.

[20] Interestingly, supplementary analyses show that the effects of the reform on family adjusted discretionary income varied by whether a partner was present or not. Yet with mean family size adjusted discretionary income (in the comparison group one year after the passage of the reform) of €1,545 for single headed households and €1,845 for other households, the reform did affect the former group relatively more.

[21] The share of reports to child protective services as well as the types of concerns expressed by informants are similar across boys and girls.

Table 6

Heterogeneity in the estimated effects of the reform

	Mother single		Mother married or cohab.		Girls		Boys		Age < 6		Age 7-14		Age 15-18	
	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error
Danish reading														
One year after	-0.005	0.035	0.072	0.058	0.037	0.041	-0.012	0.045						
Math														
One year after	0.004	0.043	*0.117*	0.070	0.023	0.051	0.037	0.054						
Social wellbeing														
One year after	**-0.051**	0.015	**-0.067**	0.024	**-0.050**	0.018	**-0.060**	0.017						
Child protective services														
1st quarter after	**0.010**	0.003	0.001	0.004	0.004	0.004	**0.011**	0.004	0.000	0.005	**0.013**	0.003	0.003	0.006
2nd quarter after	*0.005*	0.003	0.001	0.004	0.001	0.004	**0.006**	0.004	-0.001	0.005	**0.007**	0.003	0.003	0.005
3rd quarter after	**0.012**	0.003	0.005	0.004	0.008	0.004	**0.012**	0.004	0.006	0.005	**0.013**	0.004	0.005	0.006
4th quarter after	**0.019**	0.004	**0.019**	0.005	0.009	0.004	**0.028**	0.004	**0.012**	0.006	**0.021**	0.004	**0.020**	0.007

Notes: The table shows heterogeneity by children's and mothers' characteristics. The specifications parallel those from Tables 3-4 and Figure 5. **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

Our heterogeneity analyses suggest that the reform had differential impacts depending on family and child characteristics. Another way to unpack our results is by studying alternative behaviors. One implication of the reform, for example, might be that mother would move their families to smaller and cheaper housing, which could, in itself, cause disturbances to child and family wellbeing. To shed light on the prevalence of this type of behavior we use monthly information about place of residence to explore the effects of the reform on the propensity to move. Table A6 shows that a smaller share of families, namely 1 percentage point one year after the passage, reacted to the reform by moving to another address. Despite the apparent fixed costs associated with changes to one's residence, moves are not uncommon in this population; in the comparison group, as many as 17% of mothers had moved one year after the passage of the reform. I.e., though the reform did induce some families to move, we think it is unlikely to fully account for the effects we saw above.

A second implication could be that children will exhibit more absence behaviors because of their lower (family) wellbeing. While potentially interesting in its own right, the outcome is also important because 6% of the reports to child protective services come about because of high levels of school absence. To analyze the effects of the reform on absence rates, we use data provided by the Ministry of Education, informative about children enrolled in public schools. We detect statistically significant upticks in absence rates because of the reform in the non-summer quarters; in the fourth quarter after the passing of the reform, we estimate an increase in children's absence rate of 0.007 that should be seen relative to mean monthly absence rates of around 0.09; see again Table A6. We think of this result as corroborating our main findings.

A third implication could be that children will experience more injuries, maybe because of behaviors in the child or because the parents' attention is directed elsewhere due to the reform. As discussed above, the reports to child protective services were primarily driven by externalizing behaviors in the child; insufficient care from parents, and high levels of conflict in the family. We explore the effects of the reform on the incidence of injuries by means of diagnosis data from hospital admissions (in- and outpatient).[22] Table A6 shows an increase in the prevalence of injuries of 0.6 percent in the second quarter of the reform, where the overall prevalence of injuries is 5.1% in the comparison population. Note that this is precisely the quarter that contains the summer months where school is out. Table A10 shows that common injuries among children are contusions to the wrist or hand (9%), head

[22] We include the ICD-10 diagnosis codes S00-T88 that cover injury, poisoning and certain other consequences of external causes.

wounds (9%), dislocated joints at the ankle or foot level (8%), fractures of the forearm (6%), and dislocated joints at the wrist or hand level (6%). Again, we believe the finding supports our main conclusions from above.

7. 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 that resulted in considerable reductions in welfare. 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 analysis documents that mothers' propensity to receive welfare only decreased somewhat as a consequence of the reform, just as we observe a small increase in hours worked. Together, this led to a considerable decrease in families' discretionary income per household member, and the relative effect was particularly large in single households. We do not detect effects on short-run children's academic performance but do, however, document smaller negative effects on children's self-reported school well-being along with substantial uptics in reports to child protective services for children exposed to the reform. Notably, reports to social services were primarily due to child externalizing behaviors, insufficient care by parents, or high levels of conflict in the family. Interestingly, the increases in reports to child protective services were especially large in single headed households.

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 one year after its passage, this would increase our findings around that time with around 40%. With regards to social wellbeing, our main estimate would increase to .08 points or 12% of a standard deviation, while the estimated effect on reports to child protective services would increase to 2.7 percentage points.

One may raise the concern that we simultaneously explore various outcomes and mechanisms at several points in time, which could lead to issues with multiple hypothesis testing. However, given

the existing literature that primarily focuses on a limited set of often academically 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 serve as a guide for these choices.

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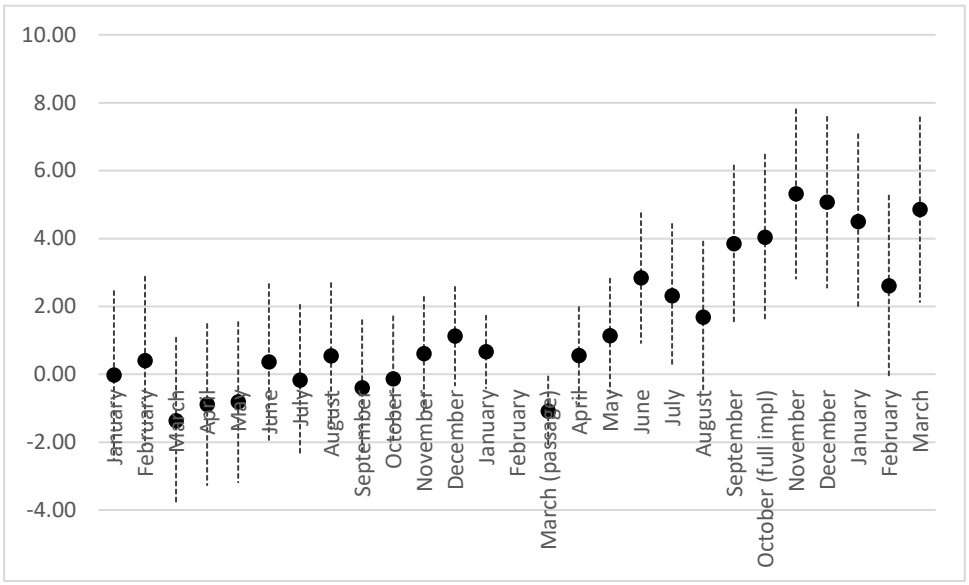
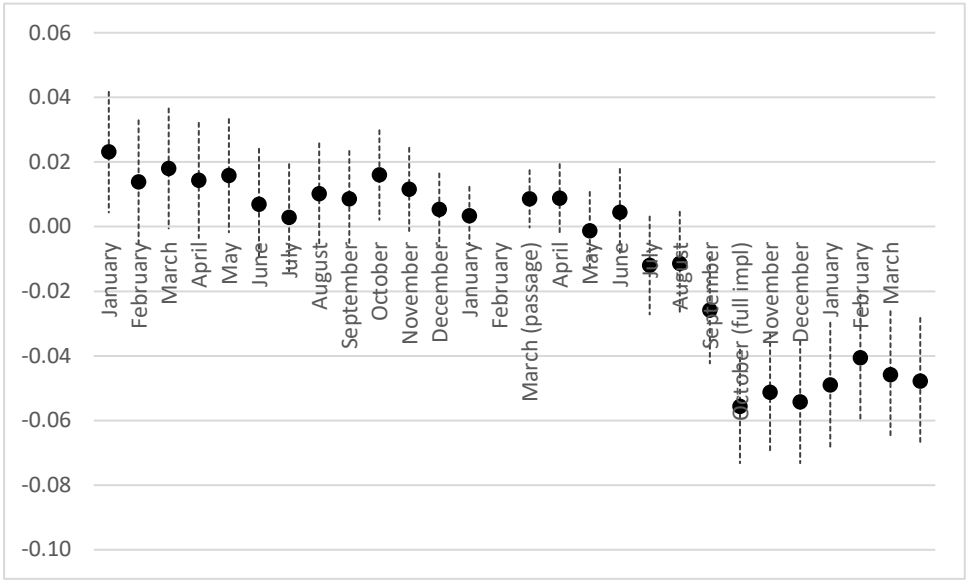
Appendix A

Figure A1

Estimated effects of the reform by month, partners' outcomes



Panel A: Hours worked

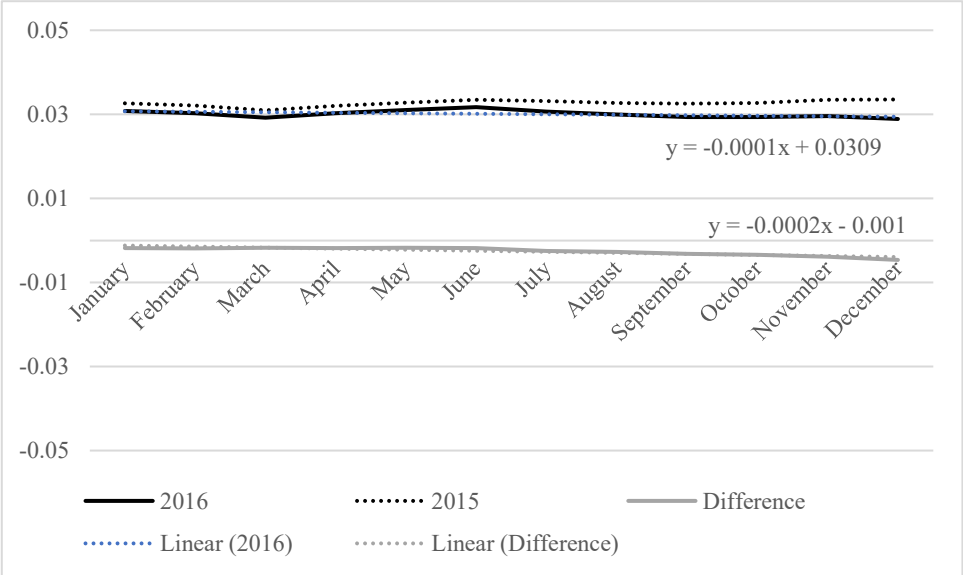


Panel B: Welfare

Notes: This figure shows the estimates and 95%-confidence intervals from a comparative event study estimation anchored in February, just prior to the reform. 2016 (2015) population consists of partners to mothers on welfare benefits in March 2016 (2015). Sample size: 303,264.

Figure A2

Inflow into welfare



Note: The figure shows the share of mothers on welfare benefits during 2015 and 2016 as well as the difference between the shares. Estimated trend lines relate to the share of welfare in 2016 (upper) and the difference between the 2016 and 2015 shares.

Table A1

Sample loss journey

	Individuals		Percent	
	2015	2016	2015	2016
Total number of welfare recipients	140,019	139,187	100%	100%
With no other types of income support	137,166	134,876	98%	97%
Living in country ultimo previous year	137,050	134,835	98%	97%
Women	65,535	64,771	47%	47%
... With children	44,370	43,667	32%	31%
… … Below the age of 18	34,827	33,960	25%	24%
Living in country at least 7 out of 8 previous years	31,098	30,175	22%	22%
With benefit levels at risk of cuts	19,716	18,578	14%	13%
Number of children	48,846	45,882		
… below 18	39,388	36,820		

Note: The table shows how the selection criteria affect the number of observations in the sample

Table A2

Descriptive statistics, fathers

	Welfare recipients		Standardized difference	Overall population, fathers	Standardized difference
	2015	2016		2016	
Sample size	22,427	20,979		603,338	
Age	40.54	40.68	0.02	43.03	-0.27
	(9.0)	(9.2)		(8.0)	
Ethnicity (%)					
... Danish	71	70	-0.02	87	-0.43
... Immigrant	27	27	0.02	12	0.40
... Decendent	2	3	0.02	1	0.12
Educational attainment, %					
... primary and lower secondary	46	46	0.02	16	0.71
… upper secondary	36	35	-0.02	45	-0.21
... some tertiary	8	8	0.01	37	-0.72
... missing	11	11	0.01	3	0.30
Years of schooling	12.2	12.2	-0.01	14.5	-0.91
	(2.5)	(2.6)		(2.6)	
Labor market experience, years	9.4	9.2	-0.02	16.6	-0.80
	(8.7)	(8.7)		(9.7)	
Hours worked, March					
... Total	63.3	61.0	-0.03	124.4	-0.86
	(78.3)	(78.3)		(69.5)	
... Unsubsidized	62.8	60.8	-0.02	123.4	-0.85
	(77.2)	(77.3)		(70.6)	
Medical utilization previous 12 months, any (%)					
Inpatient hospital care:					
... Psychiatric	2	2	0.01	0.3	0.00
...(other) Somatic	11	11	0.00	6	0.16
Outpatient hospital Care:					
... Psychiatric	5	5	0.01	1	0.22
...(other) Somatic	39	39	0.01	30	0.19
Primary care provider visit	77	77	0.00	76	0.04
Specialist visit	22	22	0.00	21	0.03
Urgent Care visit	15	16	0.03	10	0.18
Dentist visit	30	30	0.01	55	-0.53
Crime previous 12 months, any (%)					
Victim of					
... Violent assault	1.1	1.1	0.00	0.3	0.09
... property	0.7	0.8	0.01	0.5	0.04
charged with					
... Sexual assault	0.2	0.2	0.01	< 0.1%	.
... Violent assault	2.5	2.5	0.00	0.3	0.18
... Property	4.9	4.8	-0.01	0.6	0.26
... DUI	2.2	2.1	-0.01	0.3	0.16
... Possesion w/ intent to distribute	3.3	2.6	-0.04	0.3	0.20

Notes: The table shows descriptive statistics for the fathers of the children whose mothers are on welfare and compares these to the overall population of fathers aged 18 or above.

Table A3

Descriptive statistics, partners

	Welfare recipients		Standardized difference	Overall population, partners	Standardized difference
	2015	2016		2016	
Sample size	4,362	4,063		498,578	
Age	38.2	38.0	-0.02	42.7	-0.53
	(9.6)	(9.7)		(8.0)	
Male, %	99.9	99.8	0.00	99.7	0.03
Ethnicity (%)					
... Danish	52	51	-0.01	87	-0.83
... Immigrant	44	45	0.01	12	0.77
... Decendent	4	4.06	0.01	1	0.19
Educational attainment, %					
... primary and lower secondary	55	58	0.05	14	1.04
… upper secondary	31	28	-0.06	45	-0.36
... some tertiary	8	8	0.01	40	-0.81
... missing	6.1	5.9	-0.01	1.2	0.26
Years of schooling	11.7	11.6	-0.03	14.7	-1.15
	(2.7)	(2.7)		(2.6)	
Labor market experience, years	5.4	4.9	-0.08	16.6	-1.48
	(6.0)	(5.7)		(9.6)	
Hours worked, March					
... Total	35.6	33.9	-0.03	131.3	-1.54
	(62.8)	(61.4)		(64.9)	
... Unsubsidized	32.2	31.7	-0.01	130.4	-1.56
	(61.1)	(60.4)		(66.0)	
Medical utilization previous 12 months, any (%)					
Inpatient hospital care:					
... Psychiatric	0.8	0.7	-0.01	0.1	0.09
...(other) Somatic	11	10	-0.02	6	0.17
Outpatient hospital Care:					
... Psychiatric	5	5	0.00	0.7	0.28
...(other) Somatic	44	43	-0.01	30	0.28
Primary care provider visit	88	86	-0.04	76	0.26
Specialist visit	30	29	0.00	21	0.20
Urgent Care visit	18	20	0.05	10	0.28
Dentist visit	27	28	0.03	58	-0.64
Crime previous 12 months, any (%)					
Victim of					
... Violent assault	0.7	0.8	0.01	0.3	0.07
... property	0.4	0.6	0.03	0.4	0.03
charged with	0	0		0	
... Sexual assault	<0.1%	0.3	0.06	< 0.1%	0.07
... Violent assault	2	2	0.01	0.2	0.17
... Property	4	4	0.01	0.3	0.25
... DUI	2	2	0.00	0.2	0.16
... Possesion w/ intent to distribute	2	2	-0.01	0.1	0.18

Notes: The table shows descriptive statistics for the partners to mothers who are on welfare and compares these to the overall population of male partners aged 18 or above.

Table A4

Estimated effects of the reform by month, mothers' outcomes

	Outcome: hours worked				Outcome: on welfare			
	Comparative event study		Triple comparative event study		Comparative event study		Triple comparative event study	
	Coefficient	Standard error	Coefficient	Standard error	Coefficient	Standard error	Coefficient	Standard error
January	-0.061	0.344	0.332	0.359	**-0.014**	0.004	**-0.012**	0.004
February	-0.127	0.344	0.358	0.359	**-0.013**	0.004	**-0.012**	0.004
March	-0.239	0.327	0.215	0.342	**-0.012**	0.004	**-0.010**	0.004
April	-0.452	0.317	0.024	0.331	**-0.009**	0.004	**-0.008**	0.004
May	-0.330	0.307	-0.015	0.321	**-0.008**	0.004	-0.007	0.004
June	-0.193	0.298	0.033	0.313	**-0.011**	0.003	**-0.009**	0.003
July	0.005	0.283	0.760	0.298	**-0.008**	0.003	-0.006	0.003
August	0.125	0.275	0.778	0.288	-0.005	0.003	-0.004	0.003
September	-0.214	0.266	0.335	0.278	-0.002	0.003	-0.001	0.003
October	-0.132	0.242	0.495	0.253	-0.002	0.003	-0.001	0.003
November	-0.160	0.216	**0.560**	0.228	0.000	0.003	0.001	0.003
December	-0.100	0.182	**0.401**	0.192	-0.001	0.002	-0.001	0.002
January	-0.138	0.129	**0.429**	0.138	0.001	0.002	0.001	0.002
February								
March (passage)	0.138	0.136	**1.076**	0.145	-0.003	0.002	**-0.003**	0.002
April	**0.791**	0.209	**0.960**	0.217	-0.005	0.002	**-0.005**	0.002
May	**0.818**	0.258	**1.192**	0.267	-0.004	0.002	-0.004	0.002
June	**1.156**	0.287	**1.500**	0.296	**-0.006**	0.003	**-0.007**	0.003
July	**1.408**	0.304	**1.743**	0.315	**-0.016**	0.003	**-0.016**	0.003
August	**1.840**	0.326	**2.612**	0.337	**-0.024**	0.004	**-0.024**	0.004
September	**2.047**	0.349	**2.240**	0.359	**-0.034**	0.004	**-0.033**	0.004
October (full impl)	**2.605**	0.363	**2.896**	0.373	**-0.045**	0.004	**-0.044**	0.004
November	**3.415**	0.375	**3.815**	0.386	**-0.053**	0.004	**-0.053**	0.004
December	**3.390**	0.383	**3.345**	0.394	**-0.064**	0.004	**-0.062**	0.004
January	**3.279**	0.383	**3.342**	0.394	**-0.074**	0.004	**-0.072**	0.004
February	**3.465**	0.399	**3.979**	0.411	**-0.080**	0.005	**-0.077**	0.005
March	**3.911**	0.405	**3.546**	0.416	**-0.082**	0.005	**-0.079**	0.005

Notes: This table shows estimates corresponding to those in Figures 3,4 and 6.

Table A5

Ten most common jobs held

	Share
Aide in elderly care	0.169
Cleaning assistant, offices or residential homes	0.116
Pedagical assistant	0.092
Sales agent	0.066
Clerk	0.042
Kitchen assistant	0.036
Cash register operator	0.036
Nursing aide, hospitals or institutions	0.026
Warehouse assistant	0.018
Teacher, primary or lower secondary schools	0.013

Notes: This table shows the ten most common jobs held in 2016 for the group of mothers exposed to the reform.

Table A6

Estimated effects of the reform: Channels and potential mechanisms

	Coef.	Std. error
Family adjusted discretionary income (€)		
One year after	**-316.06**	5.57
# mothers in comparison group	18,578	
# mothers in treatment group	19,716	
Change of address		
One year after	**0.011**	0.004
# mothers in comparison group	18,578	
# mothers in treatment group	19,716	
Absence rate		
1st quarter after (Q2)	**0.012**	0.001
2nd quarter after (Q3)	0.001	0.001
3rd quarter after (Q4)	0.002	0.001
4th quarter after (Q1)	**0.007**	0.002
# children in comparison group	27,529	
# children in treatment group	25,666	
Any accidents		
1st quarter after (Q2)	-0.001	0.002
2nd quarter after (Q3)	**0.006**	0.002
3rd quarter after (Q4)	0.001	0.002
4th quarter after (Q1)	-0.002	0.002
# children in comparison group	36,859	
# children in treatment group	39,415	

Notes: The table shows the results from comparative event study estimation. The reform (comparison) cohort consists of families (family adjusted discretionary income), mothers (change of address), children enrolled in public shools (absence rate), and all children of mothers on welfare (accidents) in March 2016 (2015). The analysis is anchored just prior to the reform, i.e., in March 2016 (2015 for the comparison cohort) for family adjusted discretionary income and change of address and in Q1 for the absence rate. **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

Table A7

Estimated effects of the reform on each subquestion on the social wellbeing survey

	Coefficient estimate	Standard error	Sample size
How well do you like your school?	**-0.075**	0.020	45,782
How well do you like the other children in your classroom?	**-0.067**	0.020	45,703
Do you feel lonely? (reverse coded)	**0.053**	0.020	45,338
Are you afraid of being ridiculed at school? (reverse coded)	-0.017	0.020	45,886
Do you feel safe at school?	**-0.070**	0.020	45,372
Since the start of the school year, did anyone bully you? (reverse coded)	**-0.068**	0.021	44,726
I feel I belong at my school.	**-0.113**	0.020	44,725
I like the breaks at school.	**-0.118**	0.025	45,208
Most of the pupils in my classroom are kind and helpful.	-0.009	0.023	44,949
Other pupils accept me as I am.	**-0.077**	0.022	45,278

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. The reform (comparison) cohort consists of children of mothers on welfare in March 2016 (2015) who were enrolled in public schools. The analysis is anchored in Q1, just prior to the passage of the reform. Post-measurement is Q1 2017 (2016) for the reform (comparison) cohort. Model controls for linear time trend but this does not impact the estimated effect of the reform. **Bold** indicates significance at a 5% level; *italic* indicates significance at a 10% level.

Table A8

Reports to child protective services:

Reasons for concerns and types of informants

	Share
Reason for concern | concern	
Drug abuse, child	0.016
Crime, child	0.051
Problems at school, e.g., absence	0.078
Other child problem behaviors; e.g, externalizing behaviors	0.216
Disability, child	0.027
Abuse (sexual, violence) towards child	0.071
Other type of abuse or neglect	0.092
Drug abuse, parents	0.095
Crime, parents	0.010
Disability, parents	0.072
High level of conflict or violence between adults at home	0.126
Insufficient care from parents	0.157
Residential tenant eviction, homelessness	0.051
Other reason	0.225
Type of informant | reason for concern:	
School	0.206
Health care provider	0.129
Anonymous	0.081
Relative, child in question, or acquaintance	0.076
Municipal transfer in connection with moves	0.081
Police or court	0.080
Day care institution	0.068
Other	0.279
# reports	15,299

Notes: This table shows reasons and types of informants for report to child protective services for 2016 for the group of children exposed to the reform.

Table A9

Estimated effects of the reform by quarter, reports to child protective services

	Coefficient	Standard error
1st quarter after	**0.008**	0.003
2nd quarter after	0.004	0.003
3rd quarter after	**0.010**	0.003
4th quarter after	**0.019**	0.003

Notes: This table shows estimates corresponding to those in Figure 5.

Table A10

Ten most common injuries

ICD-10 code		Share
S60	Superficial injury of wrist and hand (like contusion of a finger)	0.091
S01	Open wound of head	0.090
S93	Dislocation, sprain and strain of joints and ligaments at ankle and foot level	0.078
S52	Fracture of forearm	0.059
S63	Dislocation, sprain and strain of joints and ligaments at wrist and hand level	0.059
S90	Superficial injury of ankle and foot (like contusion of an ankle)	0.053
S62	Fracture at wrist and hand level	0.051
S00	Superficial injury of head	0.046
S50	Superficial injury of forearm (like contusion of elbow)	0.041
S61	Open wound of wrist and hand	0.039

Notes: This table shows the ten most common injuries that led to hospital visits in 2016 among children of the group of mothers exposed to the reform.