YOUNG UNEMPLOYED, SINGLE MOTHERS AND THEIR CHILDREN

BY KATRINE HOLM REISO

To Svein Ivar

– my biggest fan

Preface

I prefer things to be finished before I even get started. Being extremely impatient, and having a father who spent nearly eleven years finishing his own thesis, a doctorate was not something I initially wanted. In many ways, I did not consider myself a very suitable doctoral student. For instance, the fact that I love introductory courses and would swap any more specific course for a broader course is quite the opposite of what I would expect of a doctoral student. To me, the ideal doctoral student is someone who prefers to dig deep into a rather limited research question and remain in that very deep hole for four years or more while still managing to be happy. Fortunately, I was wrong!

As a doctoral student, I have enjoyed teaching and travelling. I have met some incredible people working on some important research questions and have had the opportunity to undertake some interesting research myself. In all honesty, chatting to family, friends, neighbours and the like, I have not yet met a single person who finds my research uninteresting or meaningless. It turns out that what you really need to do a doctorate is determination (or stubbornness as my mother would call it), and of that I have plenty. A bit of curiosity and creativity also helps. And although I could benefit from being slightly more patient, at the end of the day it is *im*patience that makes things happen!

I am grateful to a number of people who have supported me throughout my candidature. First and foremost, I would like to thank my principal advisor and co-author Øivind Anti Nilsen for having faith in me. If he had not encouraged me to apply for the PhD programme, this thesis would not have come about. I thank him for taking the time to discuss important academic and sometimes less academic, though still important, questions with me. I would also like to thank my assistant advisor Maarten Lindeboom for his helpful input into my work and for providing me with recommendations when both applying for the PhD programme and going abroad to conduct research.

is more important than a doctorate. And thanks a million to my husband, Svein Ivar, for loving me.

Katrine Holm Reiso

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Introduction

Young unemployed and single mothers are natural target groups for labour market policy interventions. At young age, people are particularly prone to unemployment. Figure 1 depicts unemployment rates for the OECD countries and the US by age groups. As the figure shows, people aged 15-24 years are about twice as likely to be unemployed compared to people aged 25-54 years.

[Figure 1 "Unemployment rates for the OECD countries and the US, by age groups." about here]

Given that younger workers have less labour market experience compared to older (more established) workers, they may experience more difficulties becoming employed - and risk being the first to be laid off in bad times. If the young workers are unemployed for shorter periods of time while searching for jobs, this might not be a major concern though of course, for the individual, a short period without work may also imply reductions in income and reduced well-being. However, the consequences both for the individual and the society are much larger if such unemployment experience influences the rest of the individual's work-age life. Young people can potentially work for many years until retirement. Thus, it will be very costly if these people end up having low attachments to the labour market for the rest of their careers or do not participate in the labour market at all.

There are a number of theories of how unemployment or periods of non-work may influence an individual's future career: the individual may for instance forget valuable work experience and knowledge (Becker, 1993), become discouraged and used to a life without work (Clark, Georgellis and Sanfey, 2001), accept poorer quality jobs (Pissarides, 1994), change his/her preferences for work (Stutzer and Lalive, 2004) and/or become unattractive to potential

employers due to his/her unemployment history (Lockwood, 1991). Whatever the cause, empirical studies show that previously unemployed workers are more likely to suffer negative labour market experiences in the future, when compared to otherwise identical individuals who have not experienced unemployment; see for instance Arulampalam, Booth and Taylor, 2000; Arulampalam, 2001; Gregg, 2001; Gregory and Jukes, 2001; Gregg and Tominey, 2005. The main outlined challenge in this literature is to overcome the so-called unobserved selection issue. Unobserved factors such as productivity, preferences for work and work ability affect both the probability of becoming unemployed initially and the individual's future labour market experiences. Thus, if such selection is not carefully dealt with in the analysis, the correlation between previous unemployment and subsequent unemployment and low earnings may not be driven by the unemployment history of the worker, but by a selection into unemployment of particular group members that are expected to have low labour market attachment - regardless of their unemployment experiences.

Mothers work less than non-mothers. In 2011, the female employment rate for both the OECD countries on average and the US was 70 per cent. While the maternal employment rates were 65.2 per cent on average for the OECD countries (for mothers of children under the age of 15) and 62.4 for the US (for mothers of children under the age of 14).[1] Whether or not single mothers work more or less than married mothers varies across countries. For instance, in 2009, employment rates of single and partnered mothers were about the same in the US, while in the UK, single mothers worked considerably less (52 versus 69 per cent for mothers of children under the age of 15).[2] In contrast to married mothers, single mothers do not have husbands that can provide their families with income. Single mothers make up a substantial part of the

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[1] Source: Online OECD Family database, table LMF1.2.
[2] Source: Online OECD Family database, table LMF1.3.

population. In the OECD countries on average and in the US single mothers constitute about 7-8 per cent of all household types.[3]

Welfare dependence among single mothers has been a concern of policy-makers for decades. Modern welfare states offer a variety of specialized welfare programmes to secure income to residents who are unable to support themselves by working. These programmes should provide sufficient income and support for the targeted groups, without being attractive alternatives to work, and aim at making the recipients self-sufficient in the longer run. If, for instance, an individual is financially better off participating in a welfare programme than working, he/she has no financial incentives to relinquish welfare and start working. Individuals who are "trapped" in welfare may underinvest in their education, accumulate little labour market experience and become increasingly unattractive to potential employers. Thus, by being dependent on welfare, the individual may forego potential higher future labour income and associated levels of well-being.

For the society, maintaining an income security system is very costly. In Norway, for instance, almost 20 per cent of the work-age (18-66) population received some sort of health-related state benefit in 2011, and the trend is increasing.[4] Such a high figure questions the sustainability of welfare states with comprehensive and costly income security systems. Thus, there is a need to continuously minimize the disincentives to work that these programmes provide. Also, as countries experience changes in life expectancies, fertility rates, and age compositions of their residents, associated changes to the welfare programmes are necessary to keep their costs at an affordable level. Consequently, welfare programmes are constantly being renewed and reformed in all modern welfare states.

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[3] Source: Online OECD Family database, table SF1.1.
[4] Ministry of Finance, Report to the Storting No. 12 (2012–2013): *Perspectives*, p. 145.

A simple example may help to illustrate how welfare programmes can alter work incentives and make the recipient better off by not working.

[Figure 2 "Welfare benefits and work incentives." about here]

Figure 2 (Borjas, 2013) displays a scenario where an individual has 110 hours per week (assuming he/she sleeps about eight hours per day) to spend either on leisure or consumption goods. Hours of leisure are measured on the x-axis (increasing from left to right) and consumption goods are measured on the y-axis (in dollar value of the goods). For each hour of leisure the individual substitutes for an hour of work he/she will be able to consume more consumption goods. Hours of work is measured on the x-axis from right to left. The individual gets utility from consuming leisure and consumption goods, and the individual's preferences for leisure and consumption goods are given by his/her indifference curves (U_0, U_1). These curves show how much leisure the individual is willing to give up for more consumption goods, and vice versa, while keeping utility constant. The shape of the indifference curves reflects that the individual values more what is relatively scarce of leisure or consumption goods. When hours of leisure are plentiful, the individual is willing to give up a relatively high number of leisure hours for a small increase in consumption goods. When consumption is high, the individual is willing to give up a relatively high amount of consumption goods for a small increase in hours of leisure. All combinations of leisure and consumption goods on an indifference curve further away from the origin (U_1) are preferred to the combinations of leisure and consumption goods on an indifference curve closer to the origin (U_0). Indifference curves further away from the origin thus indicate higher levels of utility. The individual is restricted by his/her time and income. The total time spent on work and leisure must equal the total time the individual has available (110 hours), and total consumption must equal hours worked times the wage rate the individual faces and any

potential non-work income that the individual may receive. The budget constraint (given by *FE* for an individual with a constant wage rate and no non-work income) shows all combinations of leisure and consumption goods that are affordable to the individual. Combinations of leisure and consumption goods that lie above the budget constraint are not feasible. The individual maximizes his/her utility at the point where the indifference curve is tangent to the budget constraint. Combinations of leisure and consumption goods that yield higher utility (points on indifference curves further to the northeast of the optimal point) are not affordable, and combinations of leisure and consumption goods that yield lower utility (points on indifference curves further to the southwest of the optimal point) are not preferred. Note that different individuals may have different indifference curves depending on their relative preferences for leisure and consumption goods. Individuals that value leisure relatively more tend to have steeper indifference curves than individuals that value consumption goods relatively more. This means that individuals who face the same budget constraint may make different choices about hours to work. If the reservation wage, which gives the minimum increase in income that would make a person indifferent between not working and working one hour, exceeds the market wage, the individual will choose not to work. The reservation wage is given by the slope of the individual's indifference curve in the endowment point (the point of no work, i.e. *E* without non-work income and *G* with the welfare benefit).

In the absence of non-work income, the individual has to work to be able to consume. Given the individual's indifference curves and his/her budget constraint *FE*, the solution to this leisure-consumption decision is given in point *P* with 70 hours of leisure and 40 hours of work. At this point the individual is maximizing his/her utility given the budget constraint. Now, imagine that a welfare programme that pays $500 in cash per week as long as the individual does not work is introduced. This changes the budget constraint to *FEG*, and the individual is

maximizing his/her utility at point *G* (110 hours of leisure and $500 for consumption goods). If he/she starts working, the welfare benefit is withdrawn and the budget constraint switches back to *FE*. Thus, as drawn, this type of welfare benefit allows the individual to reach a higher utility level without working (from U_0 to U_1) and the incentives to work are therefore considerably reduced. Note that the increase in the individual's non-work income that this welfare programme represents (from zero to $500) increases the individual's reservation wage. As Borjas (2013) emphasizes, it is not that welfare programmes lower the labour market participation of certain people because they do not have decent "work ethic", rather it is (as the figure shows) that welfare programmes reduce work incentives, and provide better financial opportunities than those found in the labour market.

Mothers, and in particular single mothers, may find it hard to combine work with caring for their children. Therefor mothers are likely to have a relatively higher preference for so-called leisure time than non-mothers. The term "leisure" is misleading because it includes household tasks, child raising and the like (some textbooks use the term household production time). Thus, mothers typically need to be compensated by more in terms of wages for one hour less of leisure than non-mothers, and graphically, mothers' indifference curves will be steeper. This also implies that mothers' reservation wages are likely to be higher than those of non-mothers at a given endowment point. Thus, mothers are likely to work less compared to non-mothers, and are also more likely to find welfare programmes relatively more attractive than work.

There are many ways to reduce the disincentives to work created by the welfare programmes. For instance, instead of withdrawing the welfare benefit completely when the individual starts working, as in Figure 2, the individual could potentially keep parts of the benefit.

[Figure 3 "Welfare benefits and hours of work." about here]

In Figure 3 (Borjas, 2013), a welfare programme that pays $500 in cash per week if the individual does not work, and reduces the cash benefit by half of what the individual earns if he/she participates in the labour market is introduced. This is essentially the same as taxing the earnings of the welfare recipient by 50 per cent. In this particular example, the individuals' effective wage rate is thus reduced from $10 to $5 an hour. The budget constraint created by this welfare programme is given by *HG*. The corresponding utility maximizing allocation is found in point *R* with 10 hours of work (100 hours of leisure and $550 for consumption goods). Note that in point *R*, the individual works less than what he/she would have done in the absence of the welfare programme (point *P*). Thus, this type of welfare programme also introduces work disincentives. To understand why, it is useful to decompose the effect of the welfare programme on hours of work into two parts; an income effect and a substitution effect. The welfare programme increases the individual's income. As income increases the individual would like to consume more of both leisure and consumption goods. This income effect leads to a decrease in hours worked, and moves the individual from point *P* to *Q* in the figure. The earnings tax introduced by the welfare programme makes leisure time relatively less expensive. Previously, an hour of leisure could potentially have been spent working for a higher wage ($10 vs $5). Thus, the individual would like to substitute consumption goods for additional leisure time. This substitution effect further decreases hours worked from *Q* to *R*.

Another possibility to induce individuals to work is to reduce the tax paid by low-wage earners. This would increase their wage rate. For non-workers, an increase in the wage rate makes it more likely that the market wage will match their reservation wage so that they start to work. For workers, an increase in the wage rate may potentially lead to either a decrease or an increase in hours worked. The income effect of an increase in the wage rate moves the working individual towards more hours of leisure (and less hours of work). Leisure, however, is now

relatively more expensive. Thus, the substitution effect moves the working individual towards more hours of work. The dominating effect determines the change in hours worked. Other alternatives to reduce the disincentives to work created by welfare programmes are to limit the time that benefits may be received so that the recipient is no longer eligible after a certain time period and have to work, or to tie benefits to work so that the recipient must work to even qualify for benefits.

Reforms aimed at stimulating single mothers and low-wage earners to work, and thereby reducing their dependence on welfare have been implemented in several countries (US, UK, Australia, the Netherlands, Norway). Studies show that such reforms have been successful in increasing average earnings and labour market participation among single mothers (see for instance Blank, 2002 and Moffitt, 2007 - for an overview of the literature on a work-encouraging reform in the US, Mogstad and Pronzato, 2012 - for a work-encouraging reform in Norway, and Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Blundell, Brewer and Shephard, 2005; Francesconi and van der Klaauw, 2007 - for in-work tax benefit reforms).

Less is known about the impact of such reforms on the substitution of single mothers to other benefit programmes, on overall welfare dependence, and on child well-being and development. As modern welfare states have a variety of income security programmes for their residents, single mothers may switch to other benefit programmes (instead of start working) if access to their programme is restricted. US studies show that single mothers substitute between the welfare programme targeted at them and the Supplementary Security Income (SSI) programme, which provides benefits to disabled individuals (Garrett and Glied, 2000 and Schmidt and Sevak, 2004). Such benefit substitution implies that the reforms' aim of reducing welfare dependence among single mothers may be offset by increased participation of single mothers in other benefit programmes. For the society, such switch of participants from one

benefit programme to another may threaten the sustainability of the welfare state given that overall welfare costs may not decrease even though participation in one particular programme is reduced.

Maternal employment reduces the time the mother spends with her child. Depending both on the alternative care and the maternal care, this substitution away from home and towards work could be either positive or negative for the child (Becker, 1981; Baker, Gruber and Milligan, 2008; Havnes and Mogstad, 2011; Carneiro, Løken and Salvanes, 2014). To the extent that maternal employment increases family income, this might have positive effects on child development (Dahl and Lochner, 2012; Løken, Mogstad and Wiswall, 2012). Also, difficulties combining work with caring for children could affect the mother's health and levels of stress which in turn could influence the child (Berger, Hill and Waldfogel, 2005). Finally, a working mother could serve as a better role model for her children (Crompton and Harris, 1998; Fernández and Fogli, 2009; Fernández 2013; Dahl, Kostøl and Mogstad, 2013; Haaland, Rege, Telle and Votruba, 2013; Alesina and Giuliano, 2013). Thus, how the child may be affected by reforms that increase maternal employment depends on the underlying mechanisms. The empirical literature in this regard is scarce and inconclusive. For the US, Miller and Zhang (2009, 2012) find a positive effect of welfare reforms on children's education. Dunifon, Kalil and Danzinger (2003) find that the transition from welfare to work is associated with an increase in positive parenting, and decreases in behavior problems among children in the US. Paxson and Waldfogel (2003), on the other hand, suggest that welfare reforms in the US have increased child maltreatment.

About the thesis

In this thesis, life outcomes of three groups: young unemployed, single mothers, and the children of single mothers, are studied. The first paper asks how unemployment early in a worker's career may affect his/her future labour market opportunities. The second paper asks how restrictive reforms to welfare programmes for single mothers may affect benefit substitution and overall welfare dependence of single mothers. The third paper asks how the children of single mothers may be affected by such reforms.

All papers are empirical, and the dataset used covers the entire resident population of Norway. These data are ideal for analyzing these questions. They are register data at the individual level, and provide information on a wide range of demographic and socioeconomic variables for many decades. Families are linked through personal identifiers. Such an administrative database with the level of detailed information that the Norwegian register data provides is unique in an international context. It allows us to ask questions that can be difficult to answer with other data sources. The fact that these data provide very long time-series makes it possible to condition on work experience before workers potentially experience unemployment for the first time, and investigate the long-term individual effects for several subsequent years. Thus, unlike most studies that analyse long-term consequences of unemployment, we are able to capture the potential effect on future labour market experiences resulting from an initial period of unemployment as opposed to potential effects associated with accumulated unemployment by individuals with unknown employment histories. Since this individual-level dataset is drawn from administrative registers, it does not suffer from the under-reporting of welfare participation, which is a major concern with survey data used elsewhere. With this data, it is possible to measure benefit substitution by single mothers to an exhaustive list of all available alternative benefit programmes, and thus to measure total benefit substitution and overall changes in welfare

dependence in response to restrictive reforms to single mothers welfare programmes. Also, the fact that mothers and children are linked through personal identifiers, and that the data contain information on educational performance, provides a unique possibility to analyze reform effects on single mothers' children. With this data, the exact mechanisms through which welfare reforms may affect children can be studied.

In comparison to other countries, unemployment rates in Norway are low, especially in recent years after the financial crises that started in 2007 (see Figure 4). Also in Norway, however, younger workers have at least twice the likelihood of being unemployed compared to older workers.

[Figure 4 "Unemployment rates for Norway, by age groups." about here]

A work-encouraging reform to the welfare programme for single mothers in Norway was implemented in the late 1990s. This reform has many similarities to a major US reform in 1996. Both reforms introduced time limits on the receipt of benefits and implemented work requirements. In line with the studies of the US reform, Mogstad and Pronzato (2012) find that the Norwegian reform increased average labour market participation and the earnings of single mothers. However, the reform also led to a reduction in disposable income and increased poverty among a sizable subgroup of single mothers. Notably, Mogstad and Pronzato do not evaluate the associated benefit substitution and child effects. Figure 5 shows employment rates in Norway, calculated by Mogstad and Pronzato, of single mothers, married mothers (includes also cohabiting mothers) and women without children for the years 1993 to 2001.

[Figure 5 "Employment rates for women in Norway, by marital status and children (ages 18-55)." about here]

From this figure it is apparent that also in Norway mothers work less than non-mothers, and especially single mothers. The figure shows that in 1998, the year the work-encouraging welfare reform was introduced in Norway, the employment rates of single mothers increased. A comparable figure for the US for the years 1989 to 2000 is found in Blank (2002) and displayed in Figure 6.

[Figure 6 "Employment rates for women in the US, by marital status and children (ages 20-65)." about here]

Single mothers, married mothers and married women without children work considerably less than single women without children in the US. However, single mothers do not work less than married mothers. Following the US reform in 1996, there is an increase in single mothers' employment. Another similar finding across the US and Norway, is that the case-loads (the number of persons receiving benefits) of the welfare programmes for single mothers fell dramatically during the time of the reforms (Blank, 2002, and Mogstad and Pronzato, 2012).

Given the similarities of the descriptive statistics, of the work-encouraging reforms in Norway and in the US, and the similar responses in the labour market to these reforms across the two countries, the papers of this thesis may contribute valuable insight to decision makers also outside of Norway. However, one has to carefully consider the specific cultural and institutional contexts in which countries operate to successfully perform such a generalization. In the following is a short introduction to each paper.

Summary of the papers

The first paper of this thesis, entitled "Scarring effects of early-career unemployment" co-authored with Øivind Anti Nilsen (published in Nordic Economic Policy Review 2014, 1, 13-46),

asks how unemployment early in a worker's career may affect his/her future labour market opportunities. In the paper, young Norwegian workers are followed for 10 years after they potentially experienced their first unemployment spell. The results indicate that early career unemployment is decisive for subsequent labour market success. Young unemployed have a much higher risk of repeated unemployment compared to young workers not initially unemployed. They are also more likely to be out of the labour force and to return to school. This finding may be used as support for public expenditures and active labour market programmes targeted toward young workers in particular.

The second paper, entitled "The effect of welfare reforms on benefit substitution", analyzes what impact restrictive reforms of welfare programmes for single mothers may have on benefit substitution and welfare dependence. In the paper, two reforms that reduced access to the welfare programme for single mothers in Norway are exploited. Comparing single mothers, the target group of the welfare programme, to married mothers, who are ineligible, in the periods before and after the reforms, we find evidence of considerable benefit substitution by single mothers. While the reforms decreased the participation rate in the welfare programme for single mothers by 32 percentage points, this was offset by an estimated total benefit substitution effect of about 10 percentage points. The results imply that single mothers who left the welfare programme targeted at them did not necessarily become self-sufficient, and many continued to depend on welfare. From a policy perspective, this second paper stresses the importance of taking into account the entire income security system when designing welfare reforms aimed at getting individuals off welfare and into work.

The third paper, entitled "Single mothers and their children: Evaluating a work-encouraging welfare reform" co-authored with Kjell Erik Lommerud and Katrine Vellesen Løken, investigates how children of single mothers may be affected by reforms aimed at getting

their mothers to work. In the paper, we estimate the effect of reforms of the welfare programme for single mothers in Norway on the children's educational performance as adolescents. For the whole sample of children of single mothers, we find the effect on school grades at completion of junior high school at age 16 to be near zero and insignificant. If one concentrates on younger single mothers, those most likely to be affected by the reform, the grade point average of their children drops significantly by 7 % of a standard deviation. The likely mechanisms are less control of children's after-school time and reductions in family income. Despite the fact that the reforms were successful in getting single mothers to work, they had unintended adverse effects on the educational performance of children of younger single mothers. Encouraging single mothers to work may be a positive policy. However, policy makers then need to ensure that there are good substitutes for maternal time at home.

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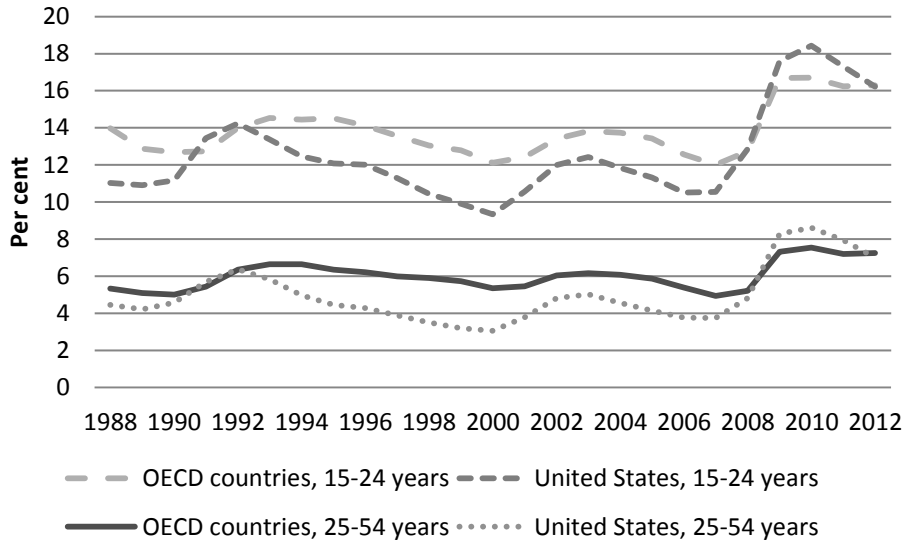
Figure 1: Unemployment rates for the OECD countries and the US, by age groups.

Source: Online OECD Employment database.
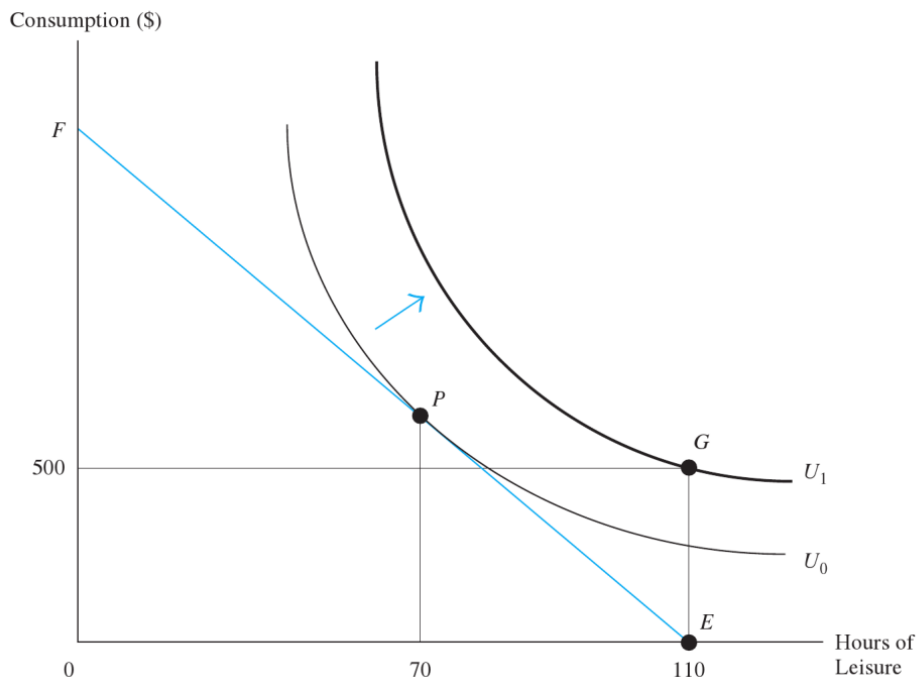
Figure 2: Welfare benefits and work incentives.

Source: Borjas (2013), Figure 2.14.
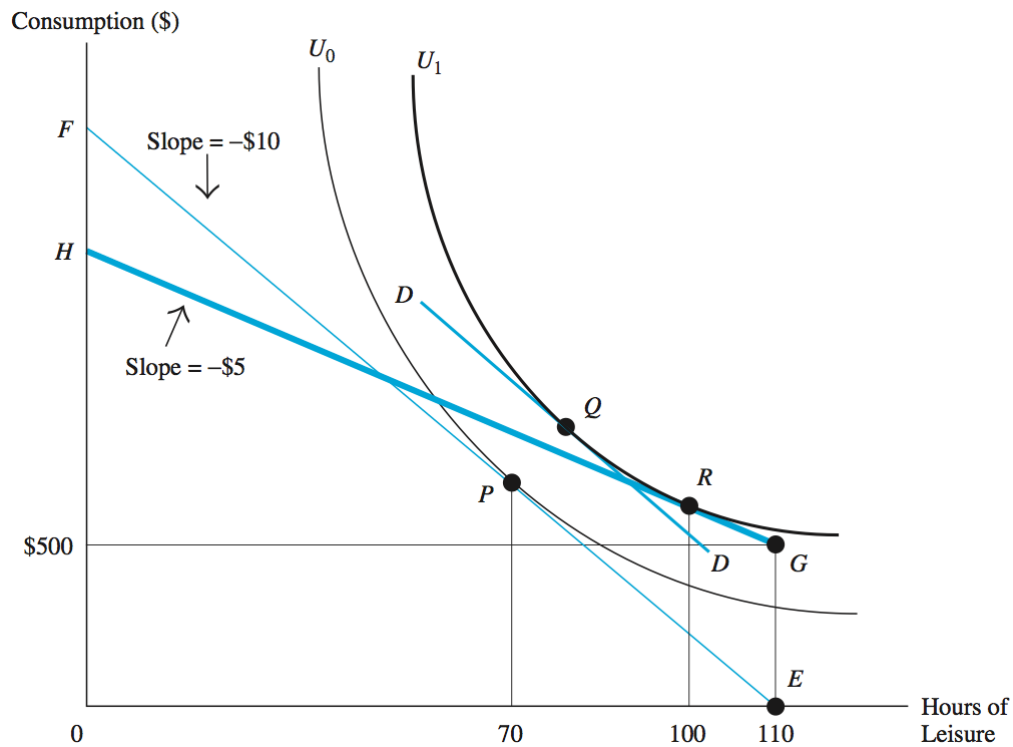
Figure 3: Welfare benefits and hours of work.

Source: Borjas (2013), Figure 2.15.
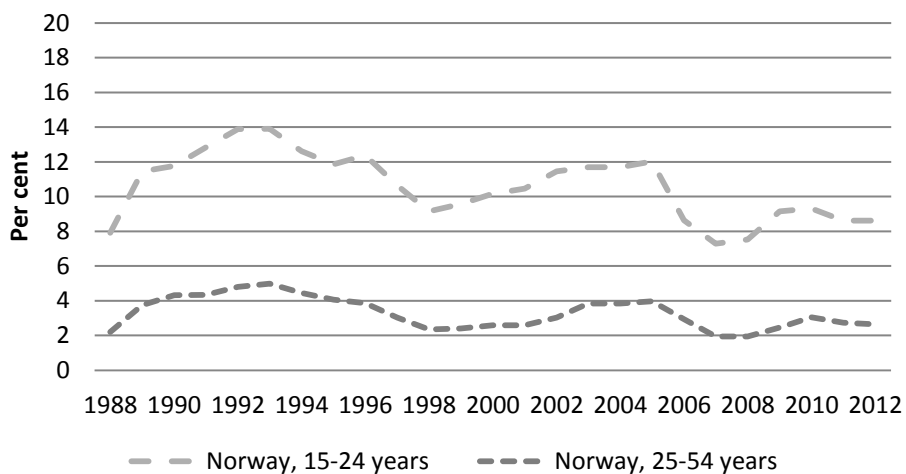
Figure 4: Unemployment rates for Norway, by age groups.

Source: Online OECD Employment database.
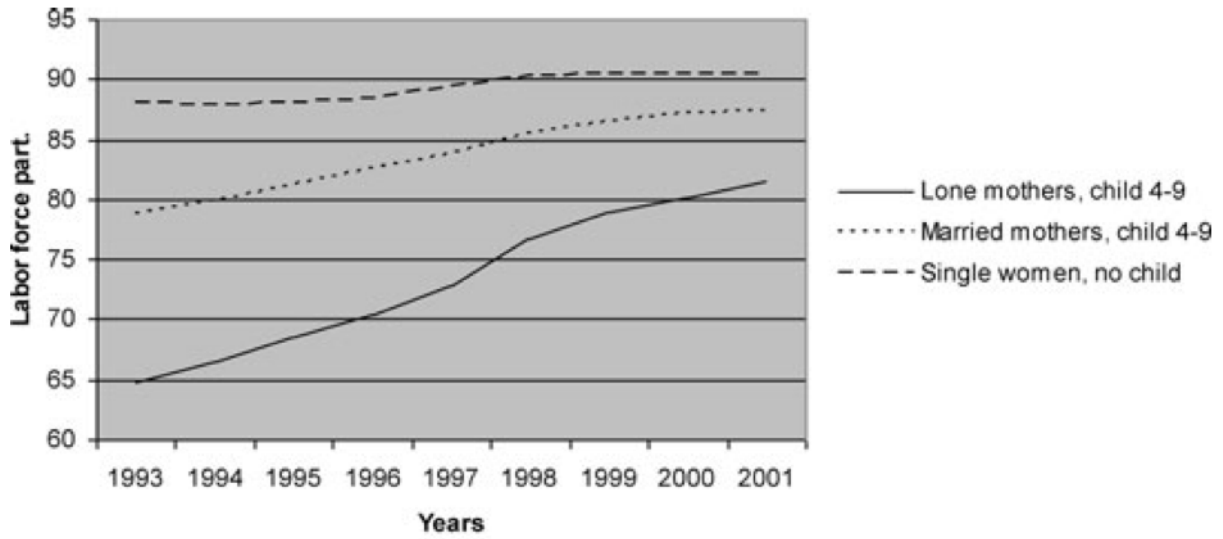
Figure 5: Employment rates for women in Norway, by marital status and children (ages 18-55).

Source: Mogstad and Pronzato (2012), Figure 3.
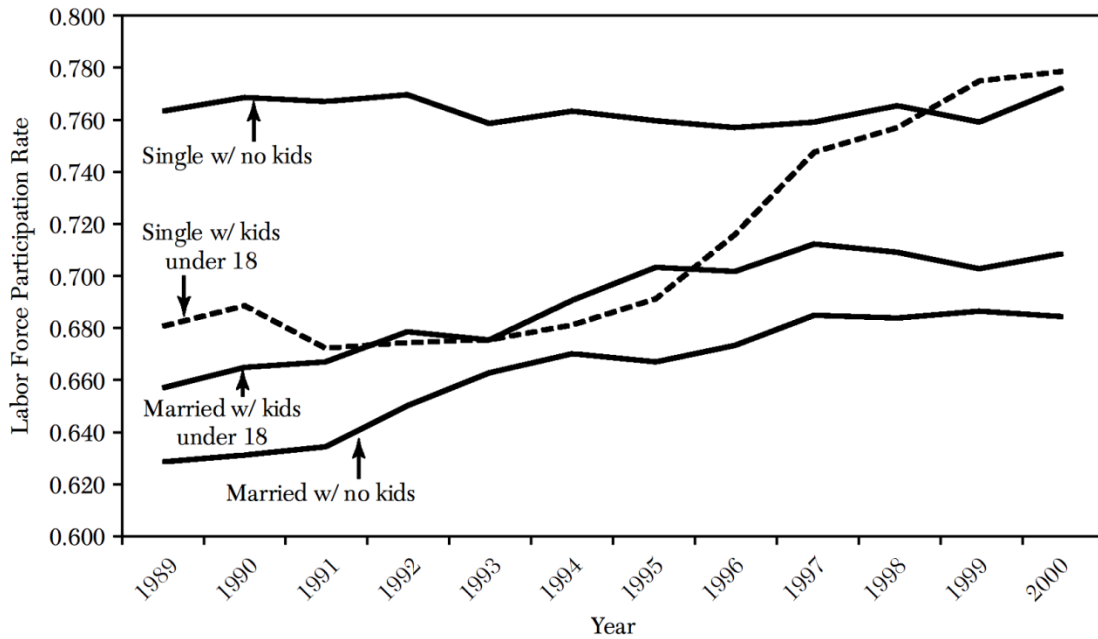
Figure 6: Employment rates for women in the US, by marital status and children (ages 20-65).

Source: Blank (2002), Figure 4.

Chapter 1

Scarring effects of early-career unemployment

Scarring effects of early-career unemployment[*]

Øivind A. Nilsen[**] and Katrine Holm Reiso[***]

Summary

The dramatically high levels of unemployment among younger worers, especially in southern Europe, emphasise an important question, how does unemployment early in a career affect future labour market opportunities? In this paper, young Norwegian residents are followed over a 15 years period. The findings show that early-career unemployment is generally associated with weaker labour market attachment. The risk of repeated unemployment decreases over time, whereas the risks of being out of the labour force and going back to school remain fairly constant. Finally, it is unlikely that the increased probability of unemployment is caused solely by selection on unobservable factors i.e. early-career unemployment leaves individuals with long-term unemployment scars.

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It is well known that younger workers are at greater risk of becoming unemployed than their older and more established counterparts. This has become particularly evident during the most recent financial crisis and recessionary conditions affecting several countries, especially those in Southern Europe, where most countries have experienced a significant increase in youth unemployment. For some countries, such as Spain and Greece, unemployment rates among the youngest cohorts often exceed 50 per cent. With this in mind, it is of great interest to know how unemployment at an early stage in a worker's career affects future labour market opportunities. If a period of unemployment results in a permanent exit from the labour market, this may be particularly severe for the young who have their entire working career ahead of them, as opposed to older workers closer to the retirement age. This is of serious policy relevance given the concern that young people may become detached from the labour market with the increased risk of a subsequently lower aggregate labour supply. Thus, unemployment may not only induce individual costs, but may have important implications for the economy as a whole, sometimes for many years (OECD, 2011). This is the same reasoning used by policymakers when they construct specific active labour market programmes targeting young workers.

There is already ample evidence of "scarring" effects in the literature, where scarring is defined as the negative long-term effects an incidence of unemployment in *itself* has on future labour market opportunities. Thus, an individual who has been unemployed will be more likely to suffer from negative labour market experiences in the future, when compared to an otherwise identical individual previously not unemployed. For instance, using UK data, Arulampalam (2001), Gregory and Jukes (2001) and Gregg and Tominey (2005) suggest that unemployment leads to subsequent losses in the range of 4 to 14 per cent of the wages. Further, again in the UK, Arulampalam et al. (2000) and Gregg (2001) provide evidence of recurring unemployment, so-called state dependence or scarring effects, in individual unemployment histories.[1] A number of studies provide comparable Scandinavian evidence. For example, Skans (2004) finds a three percentage point increase in the probability of unemployment and a 17 per cent reduction in annual earnings five years after any initial unem-

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[1] State dependence (scarring) effects have also been found in Germany. See Biewen and Steffes (2010).

ployment experience. Similarly, Eliason and Storrie (2006) also find strong evidence of earnings losses and a deteriorated employment record following job displacements using Swedish data, while Verho (2008) finds significant effects on employment, particularly significant earnings loss effects, several years after workers experience job displacement using Finnish data. Norwegian papers of particular interest include observations by Raaum and Røed (2006) of patterns of youth unemployment persistence and studies of downsizing (Huttunen et al., 2011; Bratsberg et al., 2013) indicating the increased probability of displaced workers leaving the labour force.[2]

Given this background, the aim of this paper is to analyse the magnitude of any possible scarring effects of unemployment on future labour market status, namely, being *unemployed* or *out of the labour force*, among workers at an early stage in their careers. At the same time, we analyse the probability of going *back to school*. We regard the return to school as a separate outcome because undertaking additional education potentially represents a commitment to return to work, and may thus be of rather less concern to policymakers than being unemployed or exiting the labour market. In our analysis, we focus on relatively young individuals who already have some work experience prior to potentially experiencing their first spell of unemployment. Restricting the sample in this manner makes the individuals in our sample more homogeneous in terms of labour market experience, and may reduce potential concerns regarding the initial state condition. In addition, work experience provides the unemployed with an incentive to register as such given they are likely to be entitled to unemployment benefits and hence are observable to researchers. Note also that as unemployment is more wide-spread among the youth, it is likely that unemployment is more randomly distributed within this group than among older workers. Thus, focusing on relatively young workers reduces any potential selection problems arising from unob-

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[2] While there is evidence of actual scarring effects in the literature, rather less is known about the cause. Several theories attempt to explain scarring, including the depreciation of human capital (Becker, 1993), psychological discouragement or habituation effects (Clark et al., 2001), theories of job matching where the unemployed accept poorer quality employment (Pissarides, 1994), social work norms that influence individuals' preferences for work (Stutzer and Lalive, 2004) and employers using an individual's unemployment as a signal of low productivity (Lockwood, 1991).

served heterogeneity.[3] Following standard practice in labour market studies, we analyse the potential scarring effects separately for males and females. The reasons for any potential gender differences include, for instance, differences in education, choice of occupation, family structures and individual preferences.

The data we use comprise young workers in Norway. Norway has a relatively low youth unemployment compared to many other European countries, with specific active labour market programmes targeted at younger workers.[4] This suggests that young workers in Norway generally have a higher likelihood of (re)employment, and that the scarring effects in such an economy, if any, are small. Conversely, being one of few unemployed in the Norwegian economy could send a potentially stronger negative signal to employers about the motivation and skills of the applicant as just one of many unemployed in economics where unemployment is more widespread.

Our Norwegian data have several advantages in this type of analysis. First, they provide us with a very long time series. This makes it possible for us to condition on work experience before workers potentially experience unemployment for the first time and investigate the long-term individual effects for several subsequent years. Thus, unlike most studies in this field, we are able to capture the potential scarring effects resulting from an initial period of unemployment as opposed to those associated with accumulated unemployment by individuals with unknown employment histories. Second, the data sources comprise administrative registers, e.g. the public tax register, thereby reducing problems with self-reporting errors, sample attrition, etc. Third, our data are census data, and therefore highly representative, and provide a large number of observations. Finally, unlike most other studies in this field, our data include information on female workers.

Our focus is on workers who registered as unemployed for the first time during the period 1992-1998, a period of both boom and recession in Norway. We form a comparison group, constituted of young individuals

[3] There is, of course, an extensive literature on school-to-work transition. However, as we focus on young workers with at least two years of work experience, we do not discuss this literature. We are aware that this restriction could make us underestimate the possible scarring effect given that unemployment could be considered a stronger signal about the qualifications and skills of individuals with less or no work experience.

[4] For details about Norwegian labour market programmes for youths, see NOU (2011:14, p. 143).

who are employed, and record the labour market status of the two groups over the next 10 years.[5] To ensure that the two groups are as comparable as possible, we employ a nearest-neighbour propensity score matching method. Our main finding is that there is a significant and persistent positive relationship between early-career unemployment and the future labour market status of being *unemployed.* There also appears to be rather constant but smaller long-run relationships between early-career unemployment and being *out of the labour force* and going *back to school.* This indicates that there may be a considerable scarring effect of unemployment early in a worker's career. We find that the estimated relationships are similar for males and females.

The remainder of the paper is structured as follows. Section 1 presents information about the institutional setting in Norway. Section 2 details the data and Section 3 describes the matching procedure. Section 4 provides the main results and those of several sensitivity analyses. Finally, we offer some concluding remarks in Section 5.

1. Institutional setting

The unemployment rate in Norway has traditionally been very low. In comparison, the average unemployment rate in the 27 member countries of the European Union in 2005 was 8.9 per cent, but only 4.6 per cent in Norway (OECD.Stat). However, like most countries, unemployment in Norway among younger cohorts is much higher than for older individuals. This is clearly depicted in Figure 1, where we plot the youth and overall unemployment rates for males and females in Norway.

For instance, in 1993, during a recession in Norway, the unemployment rate among males aged 15-24 years was 14.4 per cent, but only 5.7 per cent among males aged 25-54. The corresponding figures for females were 12.9 and 4.2 per cent. In 1998, a period of boom in the Norwegian economy, the corresponding figures for males and females were 9.1 and 2.2 per cent and 9.5 and 2.3 per cent, respectively. The gender difference

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[5] We do not focus on wage scarring for those returning to employment. While there is evidence of wage scarring in the literature, this appears to be of less concern in the Norwegian context. For example, Huttunen et al. (2011) find only modest effects of displacement on earnings for those remaining in the labour force, unlike the significant effects of displacement on the probability of leaving the labour force.

in unemployment rates found among younger individuals could result from the fact that males are traditionally employed in sectors that are more exposed to fluctuations in the business cycle (for instance, manufacturing and construction), while females are more typically employed in the public sector. We should also note that females to a much larger extent than males are employed part time (46.7 per cent vs. 9.4 per cent in 1995) and that gender segregation in the Norwegian labour market is quite high (see OECD, 2002). However, females generally have more education than males, at least among the youngest cohorts. For instance, based on the figures available for individuals aged 25-29 years in 1999, 30.6 per cent of the males had a university education compared to 39.2 per cent of the females (Statistics Norway).

Figure 1. Unemployment rates for Norway, by age and gender
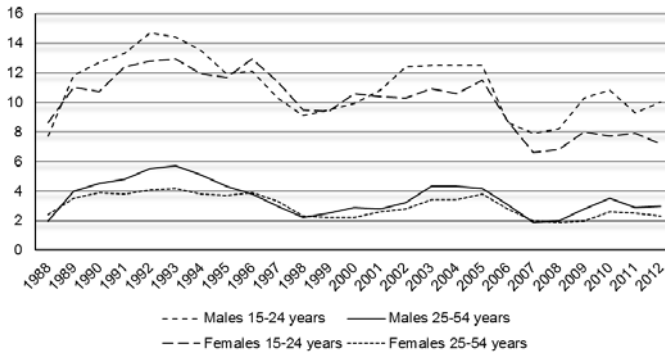
Source: Statistics Norway.

Individuals who are either residents or work as employees in Norway are automatically insured under the National Insurance Scheme. The conditions for receiving unemployment benefits are that the worker has previously earned income, has lost a job for reasons beyond the individual's control and is actively seeking employment and is capable of work.[6] To receive state benefits during the review period of this study (1992-1998), a beneficiary needed to earn a minimum of approximately NOK 50 000 (in 2009 terms) the year prior to becoming unemployed, or twice this

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[6] However, individuals who resign voluntarily, or are dismissed for reasons within their control, may also receive benefits after a waiting period of at least eight weeks.

amount during the three years prior to unemployment (NAV, 2010).[7] The benefit received is 62.4 per cent of previous earnings up to some maximum amount.[8] The unemployment benefit period varies depending on previous earnings, where benefits could in practice be received for about three years during the period 1992-1998.[9]

The two main laws regulating hires and fires in Norway are the laws of employment (*Sysselsettingsloven*) and labour relations (*Arbeidsmiljøloven*). However, there is no legal ruling on the selection of workers to be dismissed in the case of a mass lay-off. In the main collective agreement (*Hovedavtalen*) between the labour unions and the employers' association (*Næringslivets Hovedorganisasjon*), it is stated that employers should emphasize seniority when restructuring and during mass lay-offs. However, it is possible for employers to ignore the seniority rule if there are good reasons for this.

2. Data

2.1 Construction of sample

The data are from Statistics Norway and include information on all Norwegian residents aged between 16 and 74. This information includes details of employment relationships, labour market status, earnings, education, age, experience, marital status and municipality of residence, collected from different administrative registers over the period 1986 to 2008. There is also information about the number of months an individual has been registered as unemployed during a particular year.[10] Unfortunately, the registered unemployment variable is only available after 1988. Individuals entitled to unemployment benefits and those who are not may register as unemployed. However, they may only be considered for unemployment benefits if registered.

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[7] 1 NOK ≈ 1/8 EUR.

[8] The maximum benefit in 1998 was approximately NOK 340 000 (in 2009 terms).

[9] Within a period of 52 weeks, an individual may cease to receive unemployment benefits, for instance, due to employment, and then return to receiving unemployment benefits without having to meet the minimum earnings threshold.

[10] In the data, an initiated month of registered unemployment is recorded as a full month even if the unemployment spell is shorter.

The sample is constructed by pooling all individuals in the period 1992 to 1998, which constitutes what we denote as the base years. These base years are chosen to ensure that one could observe the registered unemployment histories for individuals at least four years prior to any base year and to follow individuals up to ten years after any base year. Given that we are interested in early-career scarring, we limit our sample to those who quit school within 3-4 years prior to a base year.[11] We condition on the number of years since school and not age *per se*, so that the more and less educated have a similar amount of labour market experience. Furthermore, we exclude individuals who delayed their schooling and are more than five years off-track as compared to their peers who engaged in education non-stop from when they commenced primary school. Individuals who completed their education two years faster than normal and those with less than nine years of education are also excluded. Further, only individuals who have been working for at least two years prior to the base year are included. This includes all individuals who in the two years prior to a base year satisfy the following criteria: working in Norway for at least twenty hours a week, registered with a plant identification number, classified as receiving a wage in the tax records, and did not complete any education.[12,13] In addition, we exclude individuals who registered for unemployment benefits in any of the four years prior to a base year. That is, from when they quit school until the base year, none of the individuals in the sample experienced unemployment.[14] By requiring no unemployment and at least two years of work experience, we have made the sample more homogeneous and we believe that this reduces any potential concerns regarding the initial state condition. Consequently, if we identify any scarring effects in the analysis, these are likely due to the initial period of unemployment and do not result from a history of multiple unemployment spells and work instability found among a subgroup of workers with poor employment records. In addition, these criteria make it likely that the individuals in the sample are entitled to unemployment

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[11] Note that quitting school is not necessarily the same as graduating. Individuals may have completed a degree, finished only some courses, or simply dropped out.

[12] Being registered with a plant identification number indicates having an employer in the register month, being May for 1990 to 1995 and November for 1996 to 1998.

[13] Note that the criterion of being classified as receiving a wage excludes self-employment.

[14] We do not restrict the individuals in the sample to those who have worked non-stop since they quit school 3-4 years before. Thus, individuals who served in the military, travelled, etc., the year after quitting school are not excluded.

benefits in a base year so that it is economically beneficial for those who lose their job to register as unemployed.

On the basis of an individual's employment status in a base year, we divide the sample into two groups: the employed and the unemployed. The group of employed individuals is those registered with a plant identification number. In addition, individuals with a missing plant identification number, but registered with an identical plant identification number the year prior and subsequent to the year the plant identification number is missing, are also categorized as employed. A further requirement is that the individuals in the employed group should not be registered for any months of unemployment and not be a full-time student (i.e. not registered for ongoing education and working fewer than 20 hours a week) in a given base year.[15] All individuals with registered unemployment in a base year, regardless of whether they are full-time students or have a plant identification number, constitute the unemployed group. Individuals in a base year who are neither part of the employed group nor part of the unemployed group are excluded.[16]

For each year over a period of ten years following a base year, we compare the employment statuses of the two groups, i.e. those who were unemployed in a base year versus those who were employed. We refer to these as the follow-up years. For each of the follow-up years, we divide the individual employment statuses into four categories: *employed*, *unemployed*, not participating in the labour force, i.e. *out of the labour force* and going *back to school*. To be classified as *employed* or *unemployed*, the same criteria apply as for the classification of these two groups in a base year. We classify individuals with missing information for multiple accessible employment relationship variables and who are not already classified as *employed* or *unemployed* as *out of the labour force*.[17] Individuals who are full-time students, i.e. registered for ongoing education

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[15] Note that this definition of employment includes part-time workers.

[16] Note that even though there are seven base years in total, there is only one base year observation per individual. For individuals satisfying the criteria of being in the sample in multiple base years, we use the earliest base year observation.

[17] The employment relationship variables include the plant identification number, the firm identification number, the municipality of work and the start and termination dates of the employment relationships.

and working fewer than twenty hours a week, are classified as going *back to school*.[18]

We specify age, age squared, years of education, earnings (fixed NOK at 2000 prices), marital status and whether the individual is born outside Scandinavia as control variables. We also include information about the type of education, industry, and the size and centrality of residence. Both educational type and industry type are divided into nine categories.[19] The types of residence areas are divided into seven categories based on the size and centrality characteristics defined by Statistics Norway (Hartvedt et al., 1999), ranging from the urban capital region to relatively rural micro regions. In addition, we calculate separate unemployment rates for males and females across 46 regional labour markets.[20,21]

2.2 Descriptive analysis

Table 1 reports the characteristics of the two groups (unemployed and employed) in a given base year by gender. All characteristics are for the year prior to the base year. We can see that even though the unemployed and employed groups are similar, they are not identical. For instance, individuals in the unemployed group are on average younger, less likely to be married (especially males), and have lower levels of education and lower wages compared to those in the employed group. Among other factors, they are also less likely to work in the public sector and more likely to work in the construction industry, and less likely to live in the capital region. Moreover, individuals in the unemployed group typically live in local labour market areas with higher unemployment rates.

Figures A1 (males) and A2 (females) in Appendix A depict the shares of individuals classified as being *unemployed*, *out of the labour force* and going *back to school* in the follow-up years, where we split the sample according to the individual's employment status in the base year, i.e.

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[18] We excluded 7.8 per cent of the individuals in the sample because of inconsistencies in their employment relationship variables over time.

[19] See Statistics Norway (1989) for the education type classification and Statistics Norway (1983) for the industry classification.

[20] The 46 regional labour markets are categorized by Statistics Norway and classified according to commuting statistics (Bhuller, 2009).

[21] We employ data from the Norwegian Social Science Data Services (NSD) to construct these unemployment rates. NSD is not responsible for the analysis of the data nor the interpretations drawn in this study.

unemployed or employed. As shown, the unemployed group has a higher probability of being in any of the above-mentioned employment statuses in all follow-up years when compared to the employed group. However, these differences may result from differences in the observed characteristics and not the initial unemployment experience. Accordingly, to construct a valid control group for the unemployed group, we employ matching.

Table 1. Descriptive statistics before matching . Mean values and shares. All characteristics measured the year prior to the base year

	Males				Females			
	Un empl.	Empl.	|bias| (%)[a]	p-value	Un-empl.	Empl.	|bias| (%)[a]	p-value
Age	22.22	24.46	75.7	0.00	22.59	24.48	66.4	0.00
Yrs. of educ.	11.58	13.14	76.9	0.00	12.08	13.48	68.5	0.00
Earnings in 1 000[b]	173	237	74.0	0.00	146	196	73.9	0.00
Married	.06	.17	33.5	0.00	.13	.20	20.4	0.00
Non-Scand.	.02	.02	3.1	0.07	.02	.02	0.1	0.98
Education type[c]								
General	.14	.08	18.7	0.00	.21	.12	25.6	0.00
Teaching	.01	.03	13.8	0.00	.06	.12	21.7	0.00
Humanities/art	.03	.03	0.2	0.92	.07	.06	6.1	0.03
Business adm.	.10	.19	25.8	0.00	.31	.29	4.3	0.02
Sciences/techn.	.62	.54	17.4	0.00	.10	.10	0.8	0.66
Transport	.02	.02	0.3	0.88	.03	.02	4.5	0.01
Health services	.00	.03	20.4	0.00	.05	.19	44.3	0.00
Agriculture	.03	.03	0.5	0.77	.02	.02	4.3	0.01
Service/defence	.05	.06	3.9	0.04	.16	.09	19.9	0.00
Industry[c]								
Agriculture	.04	.03	8.2	0.00	.02	.01	7.5	0.00
Petroleum	.01	.01	5.7	0.00	.00	.01	2.7	0.18
Manufacturing	.28	.23	10.3	0.00	.10	.07	9.7	0.00
Electricity	.05	.03	10.4	0.00	.01	.00	7.5	0.00
Construction	.25	.13	29.9	0.00	.02	.01	7.8	0.00
Wholesale	.18	.17	1.9	0.28	.36	.22	31.4	0.00
Transport	.04	.05	5.3	0.01	.02	.03	5.9	0.00
Finance	.04	.10	25.9	0.00	.07	.08	5.9	0.00
Public	.14	.26	31.2	0.00	.41	.57	33.2	0.00

Table 1. Continued....

	Males				Females			
	Un empl.	Empl.	|bias| (%)[a)]	p-value	Un-empl.	Empl.	|bias| (%)[a)]	p-value
Residence Char.[c)]								
Capital region	.18	.26	20.5	0.00	.25	.33	17.6	0.00
Metropolis region	.16	.18	3.7	0.05	.18	.17	3.6	0.05
University region	.02	.02	2.2	0.20	.02	.02	1.0	0.61
Centre region	.29	.26	6.6	0.00	.24	.23	2.3	0.21
Med.-size region	.10	.09	4.7	0.01	.09	.08	4.4	0.02
Small-size region	.08	.07	4.6	0.01	.06	.05	2.8	0.12
Micro-size region	.17	.13	11.5	0.00	.16	.12	11.2	0.00
Base years[c)]								
1992	.33	.22	24.7	0.00	.27	.26	2.1	0.24
1993	.18	.12	17.3	0.00	.17	.14	8.4	0.00
1994	.12	.12	1.6	0.37	.15	.14	3.9	0.03
1995	.10	.11	3.0	0.10	.14	.12	5.3	0.00
1996	.10	.11	4.5	0.01	.12	.11	1.0	0.58
1997	.07	.14	23.2	0.00	.09	.11	8.6	0.00
1998	.09	.17	24.7	0.00	.07	.11	17.3	0.00
Unempl. rates	6.49	5.94	30.8	0.00	4.74	4.56	16.5	0.00
No. of individuals	3 294	45 139			3 128	45 041		

Source: Own calculations.

Note: a) Absolute standardized bias. For each covariate X, the absolute standardized bias is defined as $\left| 100 * \bar{X}_U - \bar{X}_E \middle/ \sqrt{0.5*\left(V_U\left(X\right)+V_E\left(X\right)\right)} \right|$ where $\bar{X}_U \left(V_U\right)$ is the mean (variance) in the unemployed group and $\bar{X}_E \left(V_E\right)$ is the mean (variance) in the employed group. b) Fixed NOK in 2000 prices. c) Shares in each category within each group (unemployed and employed). Sums vertically to one.

3. Empirical method

3.1 Matching estimator

It would be desirable to compare the two potential outcomes Y_i^1 (labour market status if experienced initial unemployment) and Y_i^0 (labour market status if did *not* experience initial unemployment) in the follow-up years for individuals in the unemployed group. However, we can only observe a single outcome for each individual in the unemployed group, Y_i^1, and not the potential outcome for these same individuals had they not been unemployed, Y_i^0.

Instead, we could compare the mean differences in outcomes for all individuals in the unemployed group, the group "treated" with an initial

unemployment period in a base year, and obtain the average treatment effects. We formally define this average treatment effect on the treated (ATT) as:

$$\tau^{ATT} = E\left(Y^1 \mid D=1\right) - E\left(Y^0 \mid D=1\right), \tag{1}$$

where D indicates treatment, i.e. initial unemployment in a base year, and takes a value of $D=1$ if the group experiences initial unemployment, and $D=0$ otherwise. $E\left(Y^1 \mid D=1\right)$ is the mean outcome for individuals in the treated group who experience initial unemployment given that they *are* actually experiencing (read: are treated with) initial unemployment. This means that outcome $E\left(Y^1 \mid D=1\right)$ is observable. On the other hand, the second term in equation (1), $E\left(Y^0 \mid D=1\right)$, is never observed. This hypothetical term denotes the mean outcome for those in the treated group, $D=1$, who do *not* experience initial unemployment. Using the mean outcome of the employed group $E\left(Y^0 \mid D=0\right)$ may not be an appropriate alternative for $E\left(Y^0 \mid D=1\right)$. This non-interchangeability of $E\left(Y^0 \mid D=0\right)$ and $E\left(Y^0 \mid D=1\right)$ is due to the fact that characteristics that determine whether an individual experiences unemployment in a base year are also likely to determine the individual's future labour market status.

One way of dealing with this effect, often referred to as the selection effect, when estimating the ATT is by using a matching method. In essence, this method ensures that a control group, consisting of individuals from the employed group, $D=0$, is equal to the treated group, $D=1$, in terms of observed characteristics (see Caliendo and Kopeinig, 2008 for an overview). For instance, every unemployed 26-year-old man with 13 years of education, five years of work experience, working in the wholesale industry, living in the university region, etc. (…) in a base year, is matched with an employed man with the exact same characteristics. With such matching, we could anticipate that the mean outcome of the employed group $E\left(Y^0 \mid D=0\right)$ could be used as proxy for the hypothetical term $E\left(Y^0 \mid D=1\right)$. However, with many often continuous variables, there will be many groups. To diminish this dimensionality problem, we match the individuals using propensity scores.[22]

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[22] We have considered methods that explicitly control for unobserved characteristics. However, we do not find binary fixed effects panel data methods to be satisfactory in this context

The propensity score, defined as $p_i(x_i) = \Pr(D_i = 1 \,|\, x_i)$, assigns each individual i a probability of experiencing unemployment in a base year, given its characteristics x_i. The propensity scores are estimated separately for males and females using logistic regressions. All controls from the summary statistics reported in Table 1, in addition to the square root of age, are included in the estimations. To reduce potential problems caused by the endogeneity of the explanatory variables, all measures are for the year prior to the base year.[23] The estimated propensity score of each individual in the unemployed group is then matched with the nearest estimated propensity score of an individual in the employed group. This form of matching is referred to as the one-to-one nearest-neighbour propensity score matching method. After the matching, we have one employed individual for each unemployed individual in a base year.

3.2 Assessing the matching quality

Figures 2 and 3 depict the distributions of the estimated propensity scores before and after matching for males and females, respectively. While the distributions for the unemployed and the employed groups differ, the distributions of the employed groups cover the ranges of the unemployed groups. The extreme values (minimum and maximum) of the propensity score for the unemployed group are within the extreme values of the em-

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given that they only utilize information on individuals who experience changes in their employment status over time, thus making the definition of the control group unclear. In addition, if the scarring effect is permanent, it is removed when using fixed effects methods, while not having any spell data available prevents us from applying duration models. In addition, we considered a variety of plausible instruments for unemployment without success. For instance, using downsizing or plant closures to instrument unemployment will not satisfy the exogeneity condition, given that these will have an effect on the subsequent employment status through work-to-work transitions, and not solely through unemployment experience. Given migration decisions and differences in job match qualities, local or business cycle unemployment rates are also invalid as exogenous instruments.

[23] The variables measured prior to treatment are usually considered as exogenous, i.e. they are not influenced by the treatment itself. This is not always the case. For instance, absence because of sickness in the pretreatment period may in itself be the result of working in a firm experiencing downsizings or an increased risk of bankruptcy, which in turn may lead to an initial period of unemployment in a base year. In this sense, absence because of sickness is not exogenous to the experience of initial unemployment and thus, we do not include this in the matching. Nevertheless, we should note that there is information in the data on long-term spells of sickness (lasting 15 days or more) for 1992 onwards. We found that individuals in the treated group have a somewhat higher incidence of sickness in the pretreatment period (i.e. prior to the base year) compared to those in the matched control group.

ployed group (not shown). These patterns are important as they ensure the existence of a counterpart from the employed group for every individual in the treated group, i.e. the unemployed group. This is referred to as the common support condition. The results in Figures 2 and 3 indicate that this condition is satisfied in that after matching, the distributions of the treated and the control groups are visually identical for both genders.[24]

Figure 2. Propensity scores before and after matching – males
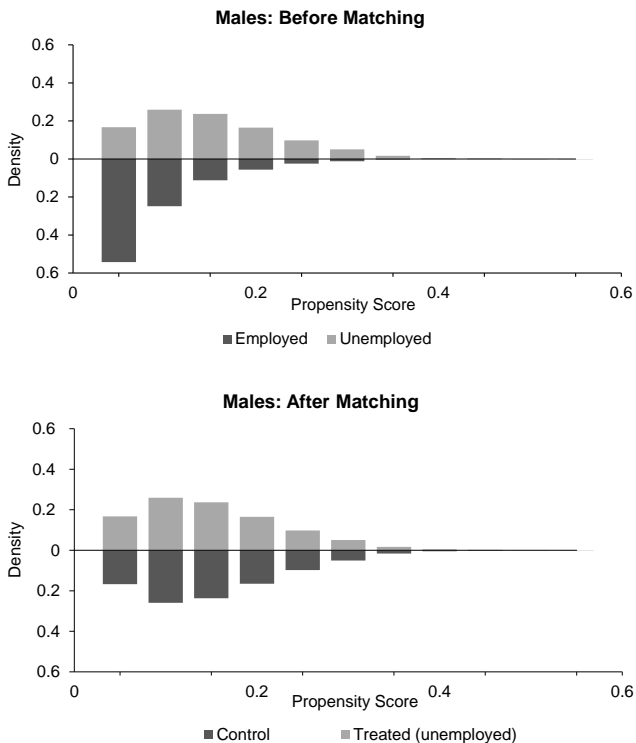
Males: Before Matching

Males: After Matching

Source: Own calculations.

Note: The employed group is not equal to the control group. The control group consists of a limited sample of the employed group after matching.

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[24] Another condition, the conditional independence assumption (CIA), also needs to hold when we condition on $p(x)$ instead of X (Rosenbaum and Rubin, 1983). The CIA states that given the observed characteristics, X, the potential outcomes are independent of treatment. Put differently, when the observed characteristics are taken into account, the probability of experiencing unemployment in a base year should be uncorrelated to whether an individual, in fact, experiences unemployment or not in the given base year.

Figure 3. Propensity scores before and after matching – females

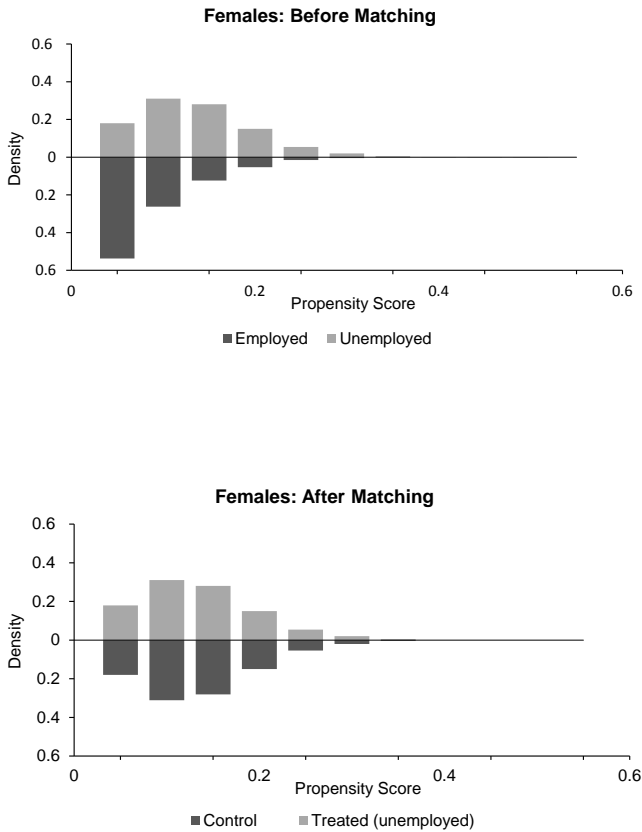Females: Before Matching

Females: After Matching

Source: Own calculations.

Note: The employed group is not equal to the control group. The control group consists of a limited sample of the employed group after matching.

Table 2. Descriptive statistics after matching. Mean values and shares. All characteristics measured the year prior to the base year

	Males				Females			
	Treated (Un-empl.)	Control	|bias| (%)[a)]	p-value	Treated (Un-empl.)	Control	|bias| (%)[a)]	p-value
Age	22.22	22.14	2.6	0.23	22.59	22.59	0.2	0.95
Yrs. of educ.	11.58	11.55	1.7	0.40	12.08	12.09	0.5	0.84
Earnings in 1 000[b)]	173	172	1.1	0.64	146	147	0.9	0.71
Married	.06	.06	1.3	0.50	.13	.13	0.3	0.91
Non-Scand.	.02	.02	2.8	0.26	.02	.02	0.7	0.78
Education type[c)]								
General	.14	.15	3.3	0.24	.21	.21	0.4	0.90
Teaching	.01	.01	0.8	0.64	.06	.06	1.1	0.59
Humanities/art	.03	.03	0.4	0.88	.07	.08	3.7	0.17
Business adm.	.10	.10	1.1	0.59	.31	.31	1.6	0.53
Sciences/techn.	.62	.62	1.2	0.61	.10	.09	0.2	0.93
Transport	.02	.02	0.0	1.00	.03	.03	1.4	0.61
Health services	.00	.00	0.0	1.00	.05	.04	2.1	0.21
Agriculture	.03	.03	0.4	0.89	.02	.02	1.0	0.72
Service/defence	.05	.05	2.8	0.22	.16	.15	2.5	0.36
Industry[c)]								
Agriculture	.04	.04	0.3	0.90	.02	.02	0.3	0.92
Petroleum	.01	.01	0.4	0.86	.00	.00	0.0	1.00
Manufacturing	.28	.28	1.3	0.60	.10	.10	0.7	0.80
Electricity	.05	.05	1.8	0.52	.01	.01	2.5	0.39
Construction	.25	.24	1.9	0.49	.02	.02	0.9	0.76
Wholesale	.18	.18	0.9	0.73	.36	.37	0.7	0.79
Transport	.04	.03	1.5	0.50	.02	.02	1.0	0.67
Finance	.04	.04	0.5	0.79	.07	.07	0.0	1.00
Public	.14	.13	0.6	0.77	.41	.41	0.1	0.98
Residence Char.[c)]								
Capital region	.18	.18	0.3	0.90	.25	.25	0.0	1.00
Metropolis region	.16	.17	1.1	0.67	.18	.18	1.8	0.47
University region	.02	.02	1.5	0.56	.02	.02	0.9	0.72
Centre region	.29	.28	1.4	0.59	.24	.25	1.1	0.68
Med.-size region	.10	.10	2.1	0.41	.09	.09	2.5	0.36
Small-size region	.08	.08	0.3	0.89	.06	.06	0.8	0.74
Micro-size region	.17	.18	2.7	0.30	.16	.16	1.0	0.71
Base years[c)]								
1992	.33	.33	0.3	0.92	.27	.27	1.3	0.61
1993	.18	.17	3.3	0.21	.17	.18	0.7	0.79
1994	.12	.13	0.9	0.71	.15	.15	0.7	0.78
1995	.10	.11	0.9	0.72	.14	.14	0.4	0.88
1996	.10	.10	0.3	0.90	.12	.12	0.9	0.72
1997	.07	.07	0.4	0.85	.09	.09	1.4	0.56
1998	.09	.09	1.4	0.52	.07	.06	0.9	0.68
Unempl. rates	6.49	6.44	2.4	0.32	4.74	4.73	1.5	0.56
No. of individuals	3 294	3 294			3 128	3 128		

Source: Own calculations.

Note: See notes to Table 1.

The results in Table 2 show that the means of the observed characteristics for the treatment and the control groups are very similar after matching. The *p*-values of the *t*-tests show that none of the means are significantly different between the two groups. Furthermore, there is no absolute standardized bias (Rosenbaum and Rubin, 1985) greater than 4 per cent for any of the observed characteristics for either males or fe-

males. Hence, the matching procedure has generated a control group for the treated group that is, on average, identical in terms of the observed characteristics.

4. Results

4.1 Main results

Figures 4, 5 and 6, respectively, depict (for males) the average treatment effects on the treated, ATTs, i.e. the differences in the probability of being *unemployed*, *out of the labour force* and going *back to school* in the follow-up years. The ATTs are the mean differences in outcomes between the group consisting of those who experience initial unemployment in a base year (the treated group) relative to the control group.

Figure 4. Average treatment effect on the treated (ATT) on the probability of being unemployed in the follow-up years – males
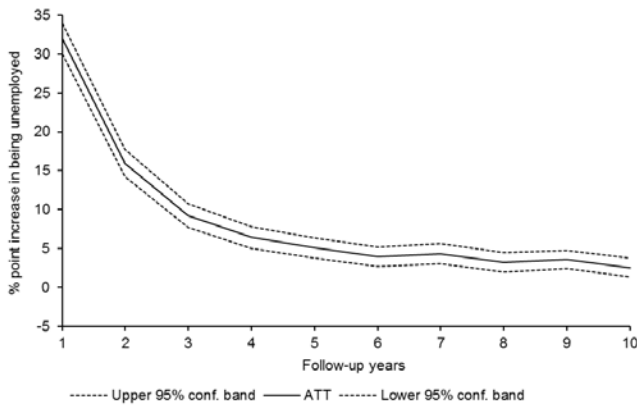
Source: Own calculations.

Note: The confidence band is calculated assuming the ATT to follow a normal distribution (reported standard errors from STATA routine *psmatch2*).

Starting with the average treatment effects on the treated of being *unemployed* in Figure 4, we can see that this is somewhat higher than 30 percentage points in the first follow-up year. Thus, individuals who experienced unemployment in a base year are on average 30 percentage points more likely to be unemployed this year relative to similar individuals who

did not experience unemployment in a base year. Note, however, that in the first follow-up year, it is likely that individuals in the treated group are in the same continuous unemployment spell that started in a base year.[25] The estimated effect drops to about 5 percentage points in the fifth year. Looking at the probabilities behind this figure in follow-up year five (not shown), we find that those in the control group have a probability of 5.3 per cent of being *unemployed*, while the corresponding number for the individuals in the treated group is much higher at 10.4 per cent. Turning to the evolution over time, we see that the average treatment effects appear to stabilize at 4 percentage points from follow-up year six onwards.

Figure 5. Average treatment effect on the treated (ATT) on the probability of being out of the labour force in the follow-up years – males
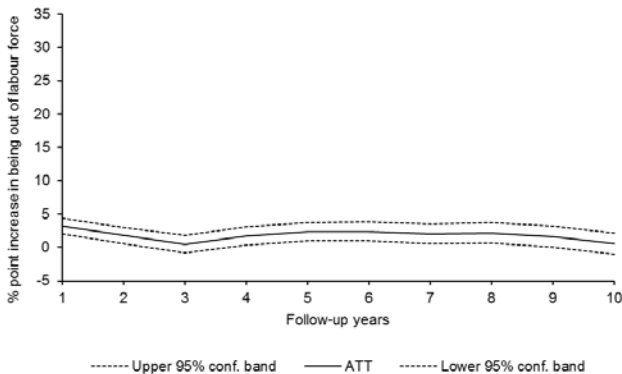
Source: Own calculations.

Note: See note to Figure 4.

Moving now to the treatment effects on *out of the labour force*, this appears quite stable over time, fluctuating around 2 percentage points. This appears consistent with the findings in Huttunen et al. (2011) (see

[25] The data do not allow us to investigate how many individuals are in the one continuous unemployment spell. We only observe the number of months an individual is registered as unemployed each year, so the individual may have been repeatedly unemployed both within the same year, and from one year to the next. Therefore, we are prevented from performing more detailed analyses of the duration of unemployment spells. However, most of the unemployed experience relatively short unemployment spells. For instance, recent figures show that 57.3 per cent of the registered unemployed aged 25-29 years had a spell duration of less than three months and only 5.6 per cent had a spell duration of more than one year (Norwegian Labour and Welfare Service). Hence, we are inclined to believe that most of the long-term effects are driven by repeated unemployment.

their Figure 3), where they analyse the effects of job displacement in Norway. Admittedly, the *out of the labour force* effects found in this study are smaller. One reason could be that in addition to *unemployment* and *out of the labour force*, we are analysing going *back to school* separately. The *back to school* effect is also stable over time and relatively small.[26]

Figure 6. Average treatment effect on the treated (ATT) on the probability of back to school in the follow-up years – males
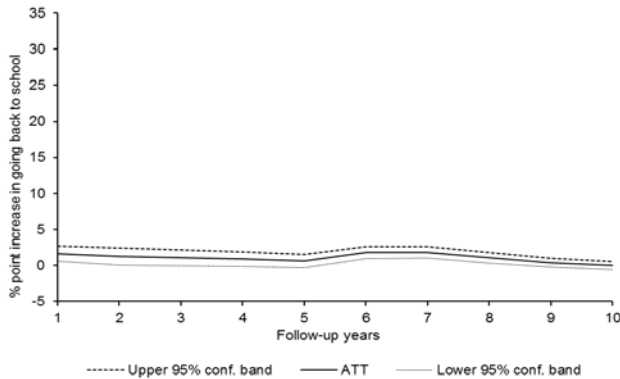

Source: Own calculation.

Note: See note to Figure 4.

Our unemployment scarring effects align with related studies analysing relatively young individuals. Interestingly, Arulampalam (2002) finds that the scarring effects are smaller for younger individuals (those less than 25 years old). She states: "This is consistent with the view that although the incidence of unemployment is generally higher among the younger men relative to older men, the younger men are less scarred by their experience in terms of relative probabilities." In an earlier version of the present paper (Nilsen and Reiso, 2011), the average age of the individuals was two years older, in which case the scarring effects were found to be somewhat larger. We also note that we consider our current sample to be positively selected, given that the included individuals have at least

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[26] In an earlier version of this work (Nilsen and Reiso, 2011), we grouped *out of the labour force* and *back to school* together. We found, not very surprisingly, that the pattern over time was the same, but that the probability of being out of the labour force (which included back to school) was larger. This could indicate that the merging of the two subgroups causes some problems when analysing the effects of unemployment for relatively young individuals.

two years of work experience and no unemployment experience since they quit school. As mentioned, these requirements are induced to reduce potential concerns regarding the initial state condition and to make the sample more homogeneous in terms of labour market experience. In addition, we only include individuals who quit school within a time frame of two years prior to, and five years after, what is expected had they undertaken their education non-stop from when they commenced primary school. Thus, when we find unemployment scarring for this somewhat selected sample, we could interpret the effects as a lower bound. It is also important to keep in mind that recurring unemployment is and should be of concern, whether it is due to state dependency or unobserved heterogeneity, even though the policy implications of the two differ.

Figures 7, 8 and 9 depict the comparable findings for females, corresponding to the differences in the incidences of *unemployment*, *out of the labour force* and *back to school*, respectively. Somewhat surprisingly, we find the pattern for females to be very similar to that for males. As discussed, in Norway, females appear to undertake more education, typically work in different industries and tend to be more family oriented earlier in the life cycle when compared to males.

Figure 7. Average treatment effect on the treated (ATT) on the probability of being unemployed in the follow-up years – females

Source: Own calculations.

Note: See note to Figure 4.

Figure 8. Average treatment effect on the treated (ATT) on the probability of being out of the labour force in the follow-up years – females
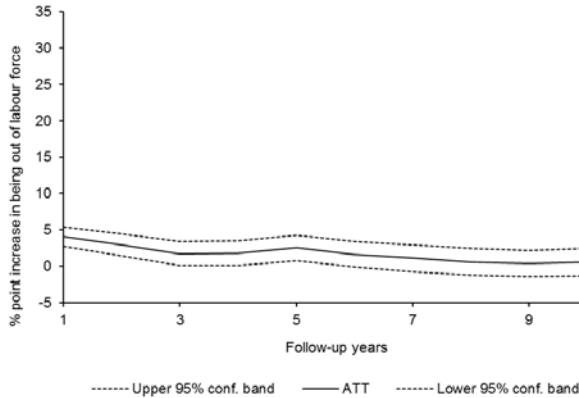
Source: Own calculations.

Note: See note to Figure 4.

Figure 9. Average treatment effect on the treated (ATT) on the probability of back to school in the follow-up years – females

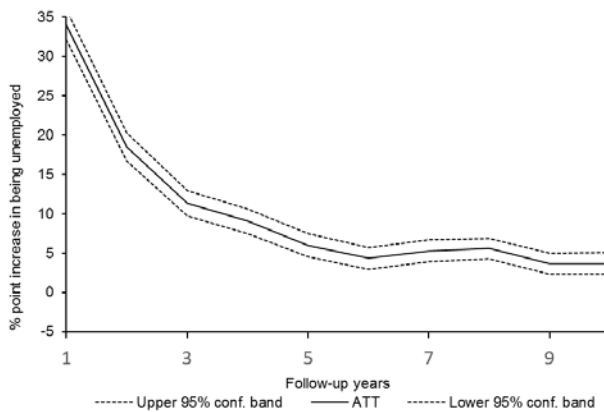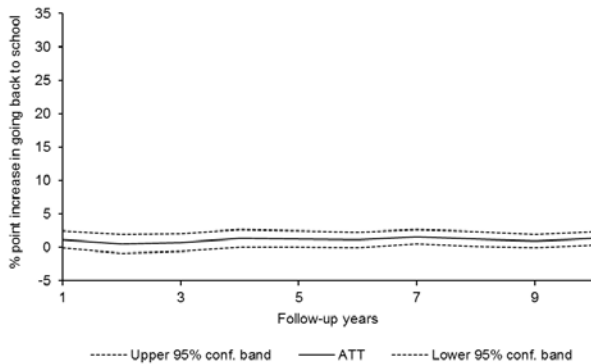Source: Own calculations.

Note: See note to Figure 4.

As a robustness check, we estimate a model with months of unemployment per year as an outcome (where the number of months is zero for those with no registered unemployment in the given follow-up year).[27] We find that those in the treatment group who experience unemployment in a base year are on average unemployed for an additional 1.2 months in the first follow-up year as compared to those in the control group. How-

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[27] These results, together with those described in the subsequent paragraph, are not shown, but are available from the authors upon request.

ever, this difference contracts rather rapidly and remains between 0.1 and 0.2 months (i.e. less than a week) from follow-up year five and onwards. However, it is unclear what this model really captures, for instance, whether these results are driven by the fact that more individuals in the treated group are unemployed in the follow-up years as compared to the control group, or perhaps whether those in the treated group who experience unemployment are unemployed for a larger fraction of the follow-up years. To investigate the latter, we examine the number of months of unemployment among those actually unemployed in the follow-up years, i.e. the number of months *conditional* on experiencing unemployment. In doing so, we find that among those who experience unemployment in the first follow-up year, the individuals in the treated group are on average unemployed for an additional month compared to those in the control group. This difference contracts to about zero in the subsequent follow-up years. Thus, in the long run, it appears that even though individuals in the treated group are more likely to become unemployed, they do not necessarily have longer unemployment spells than those in the control group.

With the current recessionary conditions in southern Europe in mind, an interesting and relevant question is whether the potential scarring effects vary with the business cycle at the time of initial unemployment. If one believes that the scarring effect stems from signalling, i.e. that employers use individual unemployment histories as a signal of low productivity and favour those with less unemployment, one could hold the prior belief that individuals experiencing initial unemployment in recessionary years could be less scarred. The reason for this is that being unemployed in such a situation is the norm rather than the exception and does not send a strong signal to the employers. We split the two samples, males and females respectively, such that two subsamples include those who experience unemployment in the base years of a recession (1992 and 1993), and two subsamples include those who experience unemployment in the remaining base years (1994-1998). The relationship between initial unemployment and future *unemployment* is found to be smaller in the long run (follow-up years 4-10 for males and 6-10 for females) for the subsamples experiencing unemployment in the base years of a recession, compared to the subsamples experiencing unemployment in the remaining base years. This pattern is consistent with the signalling theory. However, the findings are also consistent with a selection story where the unobserved char-

acteristics of individuals experiencing initial unemployment may vary with the general level of unemployment. That is, when more individuals are affected by unemployment during recessions, the unemployed may be more productive, on average, compared to those who are unemployed during periods of expansion. If our controls (including years of education and previous earnings) are unable to fully capture productivity, this could also explain the observed pattern. Thus, to conclude from a single sample split which theory or theories explain the scarring effects and/or which unobserved characteristics account for the revealed pattern is rather speculative. Note also that for both males and females, the patterns of the ATTs for these subsamples do not differ to any considerable extent from the results for the full sample already reported.

4.2 Sensitivity analysis

Even though we control for a variety of observed characteristics, there could be unobserved factors, such as productivity, preferences for work and ability, which affect both the probability of becoming unemployed in a base year and the outcome variables in the follow-up years. To address this so-called unobserved selection issue, we apply a procedure proposed by Rosenbaum (2002). This procedure tests how much these unobserved factors must influence the selection process into being treated, i.e. experiencing unemployment in a base year, before the estimated effects are no longer significant.[28]

Appendix B includes details of the Rosenbaum bounding approach. Based on the results in Table B1, we state the following. The estimated effect of being *unemployed* for males is not especially sensitive to unobserved selection bias (all but a small minority of the *p*-values in the follow-up years are zero when changing the individual relative differences of receiving treatment by a factor of 1.5, i.e. 50 per cent). However, the estimated effects for *out of the labour force* and *back to school* are more sensitive. Turning to females, the overall finding is consistent with the reported results for males. However, note that this does not infer that selection biases are present. What we may conclude is that given that most of the estimated effects of initial unemployment on being *unem-*

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[28] In addition to Rosenbaum (2002), Aakvik (2001) and Caliendo and Kopeinig (2008) also provide useful overviews of this approach.

ployed are robust to a relatively high level of unobserved selection bias, these effects are unlikely to solely be caused by selection on unobservable factors.

5. Concluding remarks

This paper contributes to the existing literature by investigating a possible scarring effect of initial unemployment on future labour market status for early-career workers with some years of work experience. We conduct separate analyses for males and females. Taking advantage of rich register data from Norway, we use a matching estimator to construct a control group that is as similar as possible with regard to observables as the individuals experiencing an incidence of unemployment. This is done in an attempt to disentangle the effects of observables and the potential scarring effects.

The main finding is that there is a persistent negative relationship between early-career unemployment and future labour market status for both genders. For males, the average treatment effects on *unemployment* start at about 30 percentage points in the first follow-up year and decline to 5 percentage points by the fifth year. In contrast, the treatment effects on being *out of the labour force* and *back to school* are about 2 percentage points and rather stable over time. Comparing males and females, we find the patterns to be very similar. When we analyse the sensitivity of the results using the Rosenbaum (2002) bounding approach, we find the majority of the estimated effects of initial unemployment on *unemployment* to be robust to a relatively high level of unobserved selection bias. Thus, it appears as if unemployment leaves early-career workers with long-term employment scars. The existence of these scars is consistent with the findings of other Scandinavian studies of labour displacement, even though most of these are based on older and more established workers. Furthermore, individuals who experience unemployment at an early stage in their career face a longer time horizon until retirement, thereby making the long-term scarring effects particularly severe.

The results of our analysis are for individuals with at least two years of labour market experience prior to the incidence of unemployment. The unemployed with no prior work experience may be even more scarred.

Thus, when there is a strong indication that early labour market history is decisive for subsequent labour market success, these findings may be used as support for significant public expenditures targeting young workers. Such policies may also be justified knowing that there is an intergenerational correlation in unemployment (Ekhaugen, 2009). However, to obtain more specific policy recommendations, research is needed regarding the exact cause(s) of unemployment scarring. Given the incidence of extremely high unemployment among youth, especially in southern Europe, this is an important and urgent topic for future research.

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Appendix A

Figure A.1 Shares of males in the two groups (employed and unemployed) being unemployed, out of the labour force or back to school in the follow-up years

Figure A.1 Continued….

Percentage back to school- males
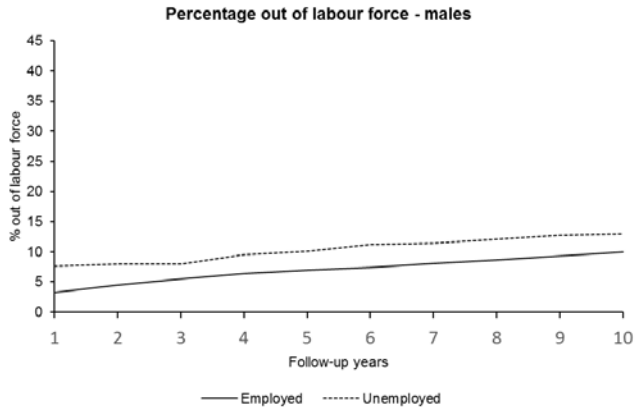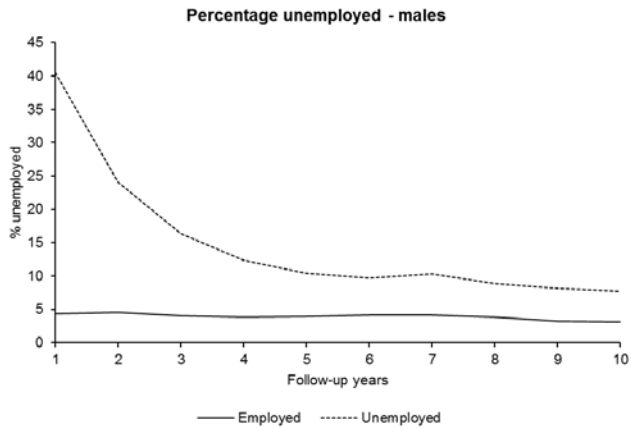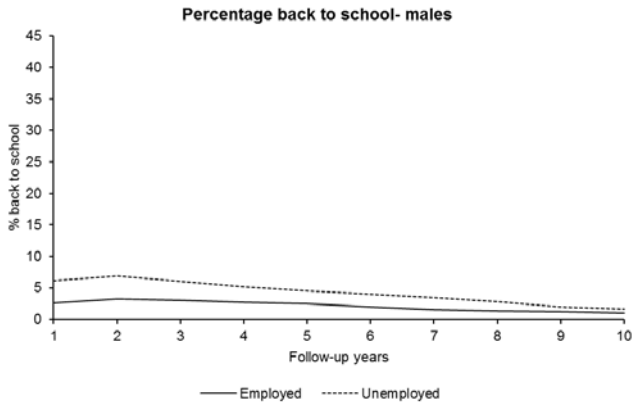
Source: Own calculations.

Figure A.2 Shares of females in the two groups (employed and unemployed) being unemployed, out of the labour force or back to school in the follow-up years

Percentage unemployed - females

Figure A.2 Continued….

Percentage out of labour force - females

Percentage back to school- females
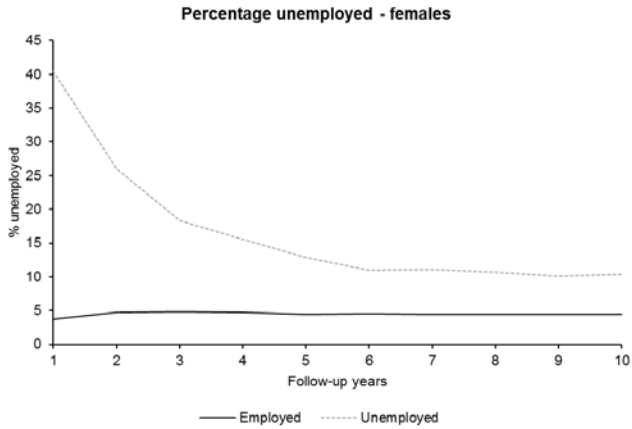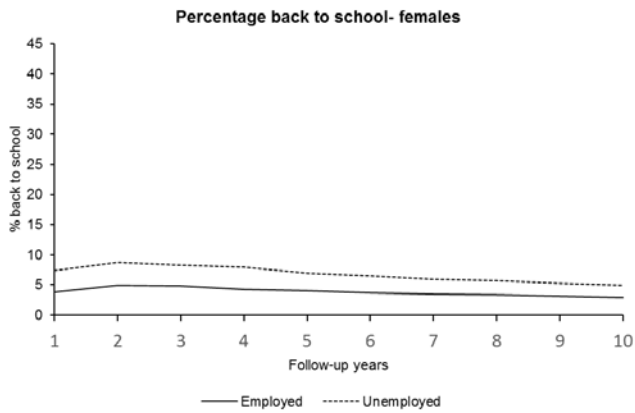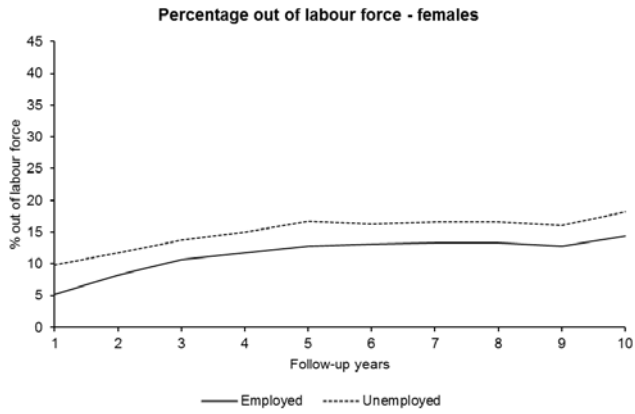
Source: Own calculations.

Appendix B

The start of the Rosenbaum bounding approach is the probability for individual i of being treated:

$$\pi_i = \Pr(D_i = 1 \mid x_i) = F(\beta x_i + \gamma u_i), \tag{B.1}$$

where u_i is an unobserved variable and γ is the effect of u_i on the probability of being in the treated group. If we have a matched pair of individuals, i and j, with the same observed characteristics x, the odds ratio $\left(\pi_i/(1-\pi_i)\right)\big/\left(\pi_j/(1-\pi_j)\right)$ (i.e. the relative odds of receiving treatment for these two individuals), given that we let $F(.)$ be the logistic distribution, may be written as:

$$\frac{\dfrac{\pi_i}{1-\pi_i}}{\dfrac{\pi_j}{1-\pi_j}} = \frac{\pi_i(1-\pi_j)}{\pi_j(1-\pi_i)} = \frac{\mathrm{e}(\beta x_i + \gamma u_i)}{\mathrm{e}(\beta x_j + \gamma u_j)} = e[\gamma(u_i - u_j)], \tag{B.2}$$

where e is the exponential function. The x vector cancels out given that the two matched individuals have the same observed characteristics. Assuming u to be binary, $-1 \le (u_i - u_j) \le 1$, equation (B.2) may be rewritten as:

$$\frac{1}{e^\gamma} \le \frac{\pi_i(1-\pi_j)}{\pi_j(1-\pi_i)} \le e^\gamma. \tag{B.3}$$

If $e^y = 1$, the two matched individuals have the same probability of being in the treated group. If a value of e^y slightly larger than 1 changes the inference about the effects of treatment, the estimated effects are interpreted as being sensitive to unobserved selection bias. In contrast, if a large value of e^y does not change the inference, the estimated effects are insensitive to unobserved selection bias. In line with Aakvik (2001), $e^y = 2$ is considered to be a very large number, changing the individual's

relative differences of receiving treatment by a factor of 2, i.e. 100 per cent.

There are two bounds related to the test procedure: a test statistic when the effects of treatment are overestimated, denoted Q_{MH}^+, and another test statistic when the effects of treatment are under-estimated, denoted Q_{MH}^-. Table B1 reports the p-values for both the upper and the lower Mantel-Haenszel (MH) bounds (see Mantel and Haenszel, 1959) for various values of e^y; i.e. $e^y = 1$ (the reference point of no unobserved selection bias), $e^y = 1.5$ and $e^y = 2$. It does this for the estimated effect of initial unemployment on being *unemployed*, *out of the labour force* and *back to school* in each follow-up year.

Table B.1 Robustness check, MH test

Follow-up years	1 mh+	1 mh–	2 mh+	2 mh–	3 mh+	3 mh–	4 mh+	4 mh–	5 mh+	5 mh–	6 mh+	6 mh–	7 mh+	7 mh–	8 mh+	8 mh–	9 mh+	9 mh–	10 mh+	10 mh–
Males																				
Unemployed																				
$e^{\gamma}=1.0$	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
$e^{\gamma}=1.5$	0	0	0	0	0	0	0	0	0.05	0	0.02	0	0.20	0	0.03	0	0.44	0	0.44	0
$e^{\gamma}=2.0$	0	0	0	0	0.11	0	0.40	0	0	0	0	0	–	0	–	0	0	0	0	0
Out of the labour force																				
$e^{\gamma}=1.0$	0	0	0.24	0.24	0.01	0.01	0	0	0	0	0	0	0	0	0.02	0	0.02	0	0.25	0.25
$e^{\gamma}=1.5$	0.06	0	–	0	–	0	–	0	–	0	–	0	–	0	–	–	–	–	–	–
$e^{\gamma}=2.0$	–	0	–	0	–	0	–	0	–	0	–	0	–	–	–	–	–	–	–	–
Back to school																				
$e^{\gamma}=1.0$	0	0	0.03	0.03	0.05	0.03	0.05	0.05	0.08	0.11	0.07	0	0.32	0	0	0	0.54	0.54	0.54	0
$e^{\gamma}=1.5$	–	0	–	0	–	0	0.11	0	0	0	0.36	0	0	0	–	–	–	–	–	–
$e^{\gamma}=2.0$	–	0	–	0	–	0	–	0	–	0	–	0	–	–	–	–	–	–	–	–
Females																				
Unemployed																				
$e^{\gamma}=1.0$	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
$e^{\gamma}=1.5$	0	0	0	0	0	0	0	0	0.07	0	0.02	0	0.16	0	0.20	0	0.25	0	0.25	0
$e^{\gamma}=2.0$	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Out of the labour force																				
$e^{\gamma}=1.0$	0	0	0	0	0	0	0	0	0.04	0	0.12	0	0.27	0	0.35	0	0.35	0	0.30	0
$e^{\gamma}=1.5$	0.04	0	–	0	–	0	–	0	–	0	–	0	–	0	–	0	–	0	–	0
$e^{\gamma}=2.0$	–	0	–	0	–	0	0.51	0	–	0	–	0	–	0	–	0	–	0	–	0
Back to school																				
$e^{\gamma}=1.0$	0.04	0.04	0.23	0.23	0.15	0.15	0.02	0.02	0.03	0.02	0	0.03	0.02	0	0.04	0.02	0.04	0.04	0.01	0.01
$e^{\gamma}=1.5$	–	0	–	0	–	0	–	0	–	0	–	0	–	0	–	0	–	0	–	0
$e^{\gamma}=2.0$	–	0	–	0	–	0	–	0	–	0	–	0	–	0	–	0	–	0	–	0

Source: Own calculations.

Note: *mh+* denotes the *p*-values of the upper bound and *mh–* denotes the *p*-values of the lower bound. A "–" denotes a negative treatment effect resulting from assuming a large positive unobserved heterogeneity bias.

Chapter 2

The effect of welfare reforms on benefit substitution

The Effect of Welfare Reforms on Benefit Substitution[*]

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Abstract

Policy-makers have confronted welfare dependence and poverty among single mothers by imposing work requirements and time limits on the receipt of welfare benefits. Reforms with such features have generally reduced programme case-loads and increased the employment and earnings of single mothers. There is little evidence, however, on the amount of benefit substitution associated with such reforms. In this paper, we test whether reductions in welfare dependence may be offset by increased participation in other benefit programmes. Evaluating the restrictive reforms of the welfare programme for single mothers in Norway, we find evidence of considerable benefit substitution. Hence, decreases in programme case-loads do not reflect equal reductions in welfare dependence.

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1. Introduction

Welfare dependence among single mothers has been a concern of policy-makers for decades. In an attempt to reduce dependence and stimulate single mothers to work, the US implemented a major nationwide reform of the welfare programme targeted at this group in 1996. The most important features of this reform were the introduction of time limits on the receipt of welfare and stringent work requirements. Overall, this reform increased the employment rates, earnings and family income of single mothers. The rate of poverty among single mothers also fell, as did the programme case-load and expenditures. Moffitt (2007, p. 31) reviews the research on this reform and declares: "That the 1996 welfare reform was a success, in overall terms and on average, is almost universally accepted by policy analysts and researchers".[1]

However, as noted by Moffitt, there is evidence of an uneven distribution of gains across different groups of single mothers, and suggestions that some were actually worse off because of the reform. According to Blank (2007), a growing share of former welfare recipients were not working, with estimates showing that 20–25 per cent of all low-income single mothers in 2004 fit into this particular category. Thus, even though the reform may be perceived as a great success *on average*, some single mothers apparently did not successfully manage the transition from welfare to work. Important information may be lost if the reform is not also evaluated with respect to the alternatives for these single mothers.

The aim of this paper is to investigate whether such restrictive reforms of welfare programmes for single mothers lead them to switch to alternative benefit programmes, so-called "benefit substitution". As most developed countries have comprehensive income security systems for their residents, single mothers may have the option to participate in one

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[1] In addition to Moffitt (2007), see Blank (2002) for an overview of this literature. There is also related literature on in-work tax benefits for low-income families in both the US and the UK, showing that employment among single mothers has increased in response to the expansion of these tax benefits. See, for instance, Meyer and Rosenbaum (2001) and Eissa and Liebman (1996) for the US, and Francesconi and van der Klaauw (2007) and Blundell *et al.* (2005) for the UK.

or more of several benefit programmes. Benefit substitution implies that decreases in the case-load or expenditure of a single welfare programme may not reflect equal reductions in welfare dependence.

To investigate this possibility, we exploit two nationwide reforms that reduced access to the welfare programme for single mothers in Norway in the late 1990s. The more important of the two Norwegian reforms has many similarities with the US reform in this area. As in the US, the aim in Norway was to stimulate mothers to work and thereby reduce their welfare dependence and improve their income, by introducing time limits on the receipt of benefits, lowering the maximum eligible age of the youngest child and implementing work requirements. Mogstad and Pronzato (2012) have already investigated the effects of this reform, finding that it increased average labour market participation and the earnings of single mothers. However, the reform also led to a reduction in disposable income and increased poverty among a sizable subgroup of single mothers. Notably, Mogstad and Pronzato (2012) do not evaluate the associated benefit substitution effects.

Two US studies in this area suggest that single mothers are able to switch between the welfare programme targeted specifically at them and the Supplementary Security Income (SSI) programme, which provides benefits to disabled individuals. Work by Schmidt and Sevak (2004) lies closest to the analysis presented here. Using data from the Current Population Survey, Schmidt and Sevak, exploit variation in state-level reforms of the welfare programme for single mothers (occurring prior to the nationwide reform in 1996) for identification. Their results showed that female-headed households in states with less attractive welfare programmes were about 20 per cent more likely to receive SSI compared with households with married mothers. In the second study, Garrett and Glied (2000) examine substitution of the SSI programme for single parent benefits in response to relaxed eligibility

criteria for children and found that child SSI participation increased more in those states with lower welfare payments to single mothers.

To our knowledge, our study is the first to analyse benefit substitution by single mothers that investigates an exhaustive list of all possible alternative benefit programmes, and it is thus the first to analyse total benefit substitution in response to such restrictive reforms. As the dataset used covers the entire resident population of Norway, it is highly representative and provides a large number of observations. Further, as this individual-level dataset is drawn from administrative registers, it does not suffer from the under-reporting of welfare participation, which is a major concern with survey data used elsewhere.

For identification, we use a difference-in-difference approach whereby we compare single mothers, the target group of the welfare programme, with married mothers, who are ineligible, in the periods before and after the reforms. We find evidence of considerable benefit substitution by single mothers in the form of switching to the alternative benefit programmes, namely, sickness insurance (SI), medical- and work-related rehabilitation, disability insurance (DI) and social assistance (SA). We find that while the reforms decreased the participation rate in the welfare programme for single mothers by 32 percentage points, this was offset by an estimated total benefit substitution effect of about 10 percentage points. Thus, the decrease in the case-load of the welfare programme for single mothers does not reflect an equal reduction in the number of single mothers who are welfare dependent. A substantial proportion of single mothers did not become self-sufficient, despite the reform's intent, and continued to depend on welfare received through these alternative programmes. The use of multiple comparison groups and a series of alternative specifications confirm these findings. This evidence of benefit substitution is in line with both the findings of the two US studies and the related literature on benefit substitution in general (i.e. *without* a particular focus on single mothers or single mother welfare programmes). See, for instance, Inderbitzin

et al. (2013), Bloemen *et al*. (2013), Karlström *et al*. (2008) and Henningsen (2008) for studies of benefit substitution in regard to reforms of unemployment or disability insurance programmes.

The remainder of the paper is organized as follows. Section 2 provides details of the reforms of the welfare programme for single mothers in Norway, offers a short description of the alternative benefit programmes for single mothers and discusses why the reforms could result in benefit substitution. Section 3 describes the data and presents descriptive evidence relating to the outcomes. Section 4 outlines the identification strategy and describes the characteristics of the sample. Section 5 presents the results and robustness tests. Section 6 concludes.

2. Background

2.1 The welfare programme for single mothers and the reforms

The welfare programme for single mothers in Norway, called the transitional benefit programme, secures income for mothers who are temporarily unable to support themselves by working because they are the sole caregiver for their children.[2] In 1998 and 1999, the Norwegian government introduced two reforms. Table 1 details the most important features of these reforms.

[Table 1 "Features of the 1998 and 1999 reforms of the transitional benefit programme" about here]

Before the reforms, there were no work requirements and a mother could technically receive benefits continuously until her youngest child finished third grade at primary school, i.e. when the child was 9–10 years old. In addition, mothers who were cohabiting could

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[2] Single fathers in Norway may also receive transitional benefits. The reason we did not include them in this study is that few single parents in Norway are men (only about 14 per cent of single parents in the 1990s were men, according to the income statistics for households) and they have significantly higher labour market attachment and earnings than single mothers (Andersen *et al.*, 2002).

receive benefits as long as they were not married and had no children in common with their current partner.

The main reform in 1998 imposed several new restrictions, namely a three-year time limit on the receipt of benefits, a reduction in the maximum eligible age of the youngest child and work requirements. The work requirements were for single mothers with children aged three years or older and included working for at least half of the hours of a standard working week in Norway (37.5 hours), studying for at least half of the hours of a full-time study or registering as unemployed. However, the same reform also increased the benefit levels to improve the incomes of those single mothers remaining eligible. The implementation of the reform was over a three-year period from 1 January 1998 to 1 January 2001. During this period, new applicants received benefits according to the new rules, while mothers who had applied for benefits before 1 January 1998 could continue to receive benefits according to the pre-reform rules. From 1 January 2001 onwards, all single mothers were subject to the new rules. An additional restriction introduced in the 1999 reform rendered cohabiting mothers in a stable relationship with a man other than the father of their children ineligible for the programme.[3]

Two other family-related welfare reforms took place in Norway at about the same time. The years 1998 and 1999 saw the introduction of cash subsidies for families with children aged one and two years that did not make full use of Norway's publicly subsidized day-care centres. By focusing in our analysis on single mothers of children aged 4–10 years, that is, those with the highest risk of losing their eligibility for transitional benefits, we ensured that these cash subsidies did not influence the estimated effects.[4] Likewise, in 1997,

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[3] Implemented for all single mothers on 1 July 1999.
[4] The cash subsidies were not limited to single mothers. However, to obtain consistent estimates of the benefit substitution effects of the reforms of the transitional benefit programme on single mothers with children younger than three years, it must hold that the cash subsidies had the same impact on single and married mothers. This assumption is doubtful. The cash subsidies reduced mothers' labour market participation (Drange and Rege,

the mandatory school starting age in Norway fell from seven to six years. Fortunately, this reform affected all mothers with children aged six in both the year before (1997) and the year after (2001) the implementation period for the transitional benefit reforms (see Section 4.1 for the identification strategy). Thus, there is no concern that these related reforms contaminated our results.

2.2 The alternative benefit programmes

The Norwegian income security system provides a variety of benefits to residents who are unable to support themselves by working. Hence, single mothers may have the option to participate in several benefit programmes, in addition to the transitional benefit programme, or to substitute one programme for another. Relevant alternative benefit programmes may include SI, medical- and work-related rehabilitation, DI and SA.[5] To be eligible for SI, medical- and work-related rehabilitation and DI benefits, the individual must have an illness, certified by a physician, which has led to a reduced ability to work. The eligible age range for medical- and work-related rehabilitation and DI benefits is 18–67 years (i.e. until the usual retirement age).

SI benefits are intended to compensate for 100 per cent of the loss of income for workers with a short-term illness lasting less than one year. To be entitled, the worker must have worked for at least two weeks prior to the injury or illness, and his/her work capacity must have been reduced by at least 20 per cent.[6] The employer pays SI benefits for the first 16 sick days.[7] If the worker has not recovered within one year and is unable to work more than 50 per cent because of the illness, he/she is then eligible for medical- or work-related

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2013; Naz (2004); Schøne (2004). However, the responses were heterogeneous in regard to mothers' education and earnings levels, and thus likely to differ across single and married mothers.

[5] Unemployment insurance (UI) is not included. The reason is that being unemployed counts as one of the work requirements in the 1998 reform. After the reforms, single mothers could register as unemployed in order to receive transitional benefits and not necessarily UI benefits. Our data do not allow us to separate the single mothers among the registered unemployed who received UI benefits from those who did not.

[6] In 2004, the minimum number of weeks increased from two to four.

[7] Until 1998, the number of days was 14.

rehabilitation benefits. Non-working residents who can document that they have been ill for at least one year are also entitled. These benefits are provided while the individual undergoes treatment to improve his/her ability to work, and may be received for several years.[8]

The DI benefit programme provides income to all individuals (including those with no employment record) who are unable to work more than 50 per cent because of enduring health-related reasons.[9] The process of applying for and receiving DI benefits is time consuming: the mean duration of the receipt of other benefits prior to being granted DI is approximately 2.5 years, and it is longer for younger age groups.[10] Unlike welfare recipients in other benefit programmes, individuals on DI benefits usually never fully return to work and are likely to depend on welfare for the rest of their lives.[11] In 2004, time-limited DI benefits were introduced to secure income for individuals for a maximum of four years in cases where (further) medical- or work-related rehabilitation was not considered beneficial but where it was likely that the individual's ability to perform work-related activities would improve within some years. Relatively young applicants were more frequently awarded time-limited DI benefits compared to older applicants (Bragstad, 2009).

SA payments are not health related and do not require documentation of an illness for eligibility. The purpose of SA payments is to ensure that everyone has a reasonable standard of living. They are means tested against income and assets, and the level of payments is set according to the applicant's needs. These payments are considered a last resort. To be eligible, the applicant must have exhausted all other opportunities to support him/herself economically,

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[8] The compensation rate is about two-thirds of the recipient's previous earnings from work, and the minimum payment was about 6,000 NOK per month in 1998. Earnings exceeding 272,000 NOK per year in 1998 went uncompensated by SI benefits and were not included in the calculation of medical- and work-related rehabilitation benefits. 100 NOK = approx. 14 EUR or 18 USD.

[9] The compensation rate is determined based on the workers' earning histories, and the minimum payment was about 6,800 NOK per month in 1998. Earnings exceeding 544,000 NOK per year in 1998 were not included in the calculation of DI benefits.

[10] Ministry of Labour, Report to the Storting No. 9 (2006–2007): *Work, Welfare and Inclusion*, p. 45.

[11] Of those who left the DI program in 2003, 74 per cent entered the old-age pension programme, 19 per cent died and only 7 per cent no longer meet the eligibility criteria and may have returned to work (Kostøl and Mogstad, 2014).

including eligibility for other benefit programmes. However, there are no restrictions on receiving SA payments on top of earnings or other benefits, as long as the given level of income is considered insufficient.

2.3 Why reforms of the transitional benefit programme may cause benefit substitution

Prior to the reforms, fewer single mothers were working, and consequently fewer single mothers were eligible for SI benefits. In general, the costs of participating in the transitional benefit programme were lower than the costs of actively engaging in medical- or work-related rehabilitation programmes or going through the process of applying for DI benefits. Hence, the relatively high participation and/or application costs of the latter programmes may have outweighed any potential gain from increased benefit levels or relaxed time limits on receipt of payments by switching programmes.

After the reforms, an increasing number of working single mothers may have gained access to SI benefits, and single mothers facing the new transitional benefits rules may have found the medical- and work-related rehabilitation programmes and DI programme relatively attractive. Reports show that single mothers generally have poorer health than the rest of the population below the retirement age (Andersen *et al*., 2002) and that single mothers have poorer mental health and worse self-reported health than married or cohabiting mothers (Ugreninov, 2005). This implies that many single mothers may be eligible for health-related benefits. The remaining single mothers who (as a consequence of the reforms) were no longer eligible for transitional benefits, and were neither able to fully support themselves by working nor able to document having an illness, may have had to rely on SA payments.

3. Data and descriptive evidence

3.1 Data

Statistics Norway produced our data based on administrative registers. The data cover all Norwegian residents and include income security system registers from 1992 to 2008.

The sample consisted of single and married mothers whose youngest child was aged 4–10 years at the end of each calendar year. Mothers were defined as single in the data if they were neither married nor cohabiting with a partner with whom they had children. This implies that a cohabiting unmarried mother was considered single if her partner was not the father of any of her children.[12] The data did not allow us to separate mothers living *without* a partner from mothers living *with* a partner as long as they remained unmarried and did not have children in common. Mothers were defined as married if they were either actually married or cohabiting with a partner with whom they had children.[13] We focused on mothers aged 19 to 55 years at the end of each calendar year who were either single or married both at the beginning and at the end of the calendar year. We selected this age range so that the sample of mothers satisfied the age eligibility criteria for all the alternative benefits, and so that the old-age pension was not an option for these mothers in the near future. As we measured outcomes annually, we also wished to ensure that there were no overlaps between the groups within a given year. If we were to take a snap-shot of the data and compare single with married mothers based on their family status, say on 1 January each year, there could be mothers in the married group who later in the same year became single mothers, and vice versa.

We defined a mother as participating in transitional, SI, medical- or work-related or DI programmes in a given year if she was recorded in the relevant register in that year. This is regardless of the number of days she received benefits or the amount she was paid.[14] The register for medical- or work-related rehabilitation benefits we used was limited to the period 1992 to 2001. The register for DI benefits we used did not include the time-limited DI benefits introduced in 2004. If a mother is married, either she or her husband may be

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[12] These cohabiting single mothers had access to transitional benefits before, but not after, the 1999 reform that made mothers in stable relationships ineligible.

[13] Some 0.2 per cent of the observations for married mothers indicate the receipt of transitional benefits. As this is inconsistent with the fact that married mothers are strictly ineligible for transitional benefits, we exclude the observations for these mothers.

[14] To be in the SI register, the mother must have been ill for longer than the number of employer-paid sick days.

registered as the recipient of SA payments, depending on who applies on behalf of the family. Thus, a mother was defined as participating in SA payments in a given year if either she, her husband (if married) or both were recorded in the register in that year.

To control for local labour market conditions, we constructed yearly unemployment rates for 46 regional labour markets in Norway according to the commuting statistics (Bhuller, 2009).[15] We calculated the unemployment rates separately for females because the labour market in Norway is highly gender segregated. In addition, we included the following set of control variables based on the mother's characteristics: age, years of education, non-Norwegian country of birth, number of children and age of the youngest child. Instead of excluding mothers with missing data for the control variables, we constructed a missing value dummy variable and included this in the regressions.[16]

3.2 Descriptive evidence of the outcomes

[Figure 1 "Participation in the transitional benefit programme" about here]

Figure 1 depicts the rate of participation in the transitional benefit programme. In each year, from 1992 to 2008, the sample consisted of all single mothers whose youngest child was aged 4–10 years. As expected, there was a decrease in participation over the years from 1997 to 2001, leading up to the beginning of the blanket implementation of the reforms, with a large drop in 2001 when the phase-in period ended. In that year, many single mothers may have exhausted the three-year time limit on the receipt of benefits.

[Figure 2 "Participation in the sickness insurance (SI) programme" about here]

[Figure 3 "Participation in medical- and work-related rehabilitation benefits programmes" about here]

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[15] For this purpose, we use data from the Norwegian Social Science Data Services (NSD). NSD is not responsible for the analysis of the data or the interpretations drawn in this paper.

[16] Excluding the mothers with missing data (5 per cent of the sample) does not alter the results (results not shown).

[Figure 4 "Participation in the disability insurance (DI) programme" about here]

[Figure 5 "Participation in the social assistance (SA) programme" about here]

Figures 2–5 plot the yearly rates of participation in the SI, medical- and work-related rehabilitation, DI and SA programmes for both single and married mothers whose youngest child was aged 4–10 years. The two groups display similar pre-reform time trends. Note, however, with regard to participation in the SI and SA programmes (Figures 2 and 5), that the pre-trends are steeper for single mothers compared with married mothers. This may reflect an increasing labour market participation of single mothers in the period before the reforms, a time when the economy was recovering from a recession that took place in Norway in the early 1990s. Consistent with the benefit substitution hypothesis, we see a larger increase in the participation of single mothers compared with married mothers in all the alternative benefit programmes during the reform period. In addition, the figures show that the levels of participation in the various programmes differ between the two groups, with single mothers being generally more dependent on benefits than married mothers.

4. Identification strategy and descriptive characteristics

4.1 Identification strategy

For identification, we use a difference-in-difference strategy. We compare the outcomes for single and married mothers whose youngest child was aged 4–10 years in 1997, the year prior to the reforms, with those for single and married mothers whose youngest child was aged 4–10 years in 2001, the year the phase-in period ended and the new rules of the 1998 reform were implemented for all. The use of a comparison group of married mothers removes any time-specific confounding factors common to both single and married mothers. The assumption required for such a traditional difference-in-difference set-up (with one difference across time and another across groups) to produce consistent estimates is that single and

married mothers follow the same underlying time trend. However, the steeper time trends we observed in Figures 2 and 5 for participation in the SI and SA programmes for single mothers in the pre-reform period indicate that we may not satisfy the common trend assumption for these outcomes. To control for this single mother-specific time trend, we add a comparison over time of single and married mothers whose youngest child was aged 4–10 years in the years 1992 and 1996, i.e. in the pre-reform period.

We implement our trend-adjusted difference-in-difference estimator by estimating the following regression:

$$
\begin{aligned}
y_{itq} = \; & \alpha_1 + \alpha_2 Reform_q + \alpha_3 Last_t + \alpha_4 Single_i + \alpha_5\big(Reform_q \times Last_t\big) \\
& + \alpha_6\big(Reform_q \times Single_i\big) + \alpha_7(Last_t \times Single_i) \\
& + \mu\big(Reform_q \times Last_t \times Single_i\big) + X'_{itq}\theta + \varepsilon_{itq},
\end{aligned}
\tag{1}
$$

where the subscript i denotes the individual, subscript t denotes the year and subscript q denotes the reform period. y_{itq} is the outcome variable and takes a value of one if the mother is registered to receive the benefit of interest and zero otherwise. *Reform* is a binary variable taking a value of one if the year is within the reform period (1997 or 2001) and zero if the year is within the pre-reform period (1992 or 1996). *Last* is a binary variable taking a value of one if the year is the last year within either the pre-reform or reform period (1996 or 2001) and zero if the year is 1992 or 1997. *Single* is a binary variable taking a value of one if the mother was a single mother and zero if she was married. X_{itq} is a vector of observed characteristics comprising the mother's age, years of education, non-Norwegian country of birth, number of children, age of the youngest child and the local labour market unemployment rate for females. ε_{itq} is the error term. The coefficient of interest is μ. This coefficient measures the effect of the reforms, i.e. the change in the mean outcome over time

in the reform period relative to the pre-reform period for single mothers relative to married mothers.

For this trend-adjusted difference-in-difference estimator to produce consistent estimates, we assume that in the absence of the reforms, the average outcome of single mothers would have changed in the same way in the reform period (1997 and 2001) as in the pre-reform period (1992 and 1996) relative to the average outcome of married mothers with children in the same age range.

4.2 Descriptive characteristics of the sample

[Table 2 "Characteristics of mothers in the pre-reform (1992 and 1996) and reform (1997 and 2001) periods" about here]

Table 2 details the characteristics of single and married mothers whose youngest child was aged 4–10 years in the pre-reform period (1992 or 1996) and the reform period (1997 or 2001). As shown, on average single mothers were younger, were less educated and had fewer children compared with married mothers. However, these differences are not of concern given that we control for these differences by allowing for different intercepts in the estimations. However, a potential concern is whether there are any different trends in the characteristics of single and married mothers. Unless controlled for, any such differences in trends could bias the estimated effects of the reforms. For example, over time, the sample of mothers is becoming older, more educated and more likely to be non-Norwegian. It is, however, encouraging to find that these time trends are similar across single and married mothers. Consistent with this, a balancing test where these characteristics served as dependent variables in equation (1) produced rather small estimates. However, given that the majority of these

estimates were statistically significant, we still included these characteristics as controls in our main specification.[17]

5. Results

5.1 Main results

To investigate the benefit substitution effects of the reforms on single mothers, we estimated expression (1) using a linear probability model.[18] All standard errors were robust with respect to heteroscedasticity.

[Table 3 "Benefit substitution effects of the reforms on single mothers" about here]

Table 3 provides the estimated results. Every estimate is from a separate estimation of expression (1) for the relevant outcome. Overall, the estimates indicate strong evidence of benefit substitution by single mothers in response to the reforms. That is, the estimated effect of the reforms on the participation of single mothers in the transitional benefit programme is negative and statistically significant, while the estimates for participation in all the alternative benefit programmes (SI, medical- and work-related rehabilitation, DI and SA) are positive and statistically significant.

With regard to benefit substitution by switching to health-related benefit programmes, the results suggest that the reforms led to a 2.66 percentage points increase in the participation rate in the SI programme, 2.86 percentage points in the medical- and work-related rehabilitation programme and 0.97 percentage points in the DI programme for single mothers compared with married mothers. These estimates are large in relative terms: compared with

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[17] Excluding the controls does not significantly alter the results (see column (2) in Table 6).

[18] We could have applied non-linear logit or probit models. Another alternative, given that there are many outcomes (benefit programmes) to choose from, is the multinomial logit model. However, the estimates from these non-linear models do not facilitate straightforward interpretation. In addition, the multinomial logit model relies on a strict assumption (the independence of irrelevant alternatives) implying that removing or changing the characteristics of a third outcome does not affect the relative odds of the two outcomes considered. In this context, given that some of the benefit programmes are more similar than others (e.g. health-related benefits vs. SA payments), this assumption is not realistic.

the mean participation rates of single mothers in these programmes in 1997, the estimated effects correspond to about 14 per cent, 50 per cent and 35 per cent increases, respectively. This suggests that many single mothers are eligible for benefits designed to secure income for individuals who have either a temporary or a long-term illness. This is in line with the reports showing that single mothers have poorer health than the general population (see Section 2.3) and may suggest that the welfare state is in fact able to support those who need it. In the absence of the reforms, these single mothers may simply have delayed their participation in health-related benefit programmes until their transitional benefits eventually expired. The increased participation in SI benefits (2.66 percentage points) is also consistent with the scenario of more single mothers earning their right to receive this benefit by working.

Conversely, this benefit substitution may also indicate a worsening of the health of single mothers in response to the reforms, possibly caused by stress or difficulties combining work with caring for their children. Further, we cannot rule out the possibility that some single mothers in a difficult economic situation may have gained access to benefits for which they were not eligible. In this regard, the increased participation in the DI programme (0.97 percentage points) is of particular concern, in that single mothers on DI benefits are likely to depend on welfare for the rest of their lives.

Of course, it would have been interesting to follow the mothers in the sample until their youngest child had reached the age of 10 years, when all single mothers would have lost access to transitional benefits in the absence of the reforms, and see whether the reforms still had effects on participation in the DI programme. If so, this would indicate that the reforms have indeed caused some single mothers to enter the DI programme who would not otherwise have done so (even when their transitional benefits eventually expired). Unfortunately, this exercise is unlikely to produce reliable results given that we do not have data on time-limited DI benefits. The introduction of this benefit is likely to have affected single and married

mothers differently: single mothers are generally younger than are their married counterparts and are thus more likely to be in the target group for the time-limited DI programme.[19]

These findings of benefit substitution by single mothers by switching to health-related benefit programmes support the studies on substitution between the welfare programme for single mothers in the US and the SSI programme. Interestingly, our estimate of benefit substitution by switching to the DI programme (0.97 percentage points) is very close to Schmidt and Sevak's (2004) finding that female-headed households in states with less attractive welfare programmes were 0.6 percentage points (in relative terms 20 per cent) more likely to receive SSI than were households with married mothers.

Turning to SA payments, we estimate that the participation rate increased by 8.15 percentage points for single mothers relative to married mothers in response to the reforms. This corresponds to a 49 per cent increase compared with the level for single mothers in 1997. Thus, SA payments prove to be the main safety net for single mothers when access to transitional benefits declines. This is as expected, in that SA payments are the only alternative for single mothers who cannot document having an illness. In addition, mothers receiving their main income elsewhere may receive SA payments as a supplementary or intermediate source of income.[20] Following the mothers in the sample six years ahead (when their children were 10–16 years old and both pre- and post-reform single mothers lost access to transitional benefits regardless of the reforms), the estimated reform effect on the participation rate in SA (at that time) is –0.8 percentage points and statistically significant (results not shown). Thus,

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[19] What we can do is to follow the mothers a couple of years ahead (to 2002 and 2003) and see whether the single mothers unaffected by the reforms were catching up with the post-reform single mothers as their children aged. This does not seem to be the case. The estimated effects on DI participation one and two years ahead are stable at 1.04 percentage points and 1.00 percentage points, respectively (results not shown). Keep in mind, however, that qualifying for DI benefits is a time-consuming process, so time lags may contaminate these estimates.

[20] We should note that the reforms also led to a statistically significant increase in the yearly SA payments for single mothers compared with married mothers. In the full sample, the estimated reform effect on yearly SA payments is about 3,900 NOK (all money values are given at 1998 prices), while in a sample restricted to SA payment receivers, the estimated reform effect is about 12,600 NOK in 1998 (results not shown). Thus, in addition to being more dependent on SA payments in terms of participation, single mothers are also in need of, or at least apply for, larger payments after the reforms.

post-reform single mothers appear to substitute SA benefits for transitional benefits when their children are relatively young, but they do not seem to be more dependent on SA payments in the long run compared with pre-reform single mothers.

The estimated effect of the reforms on total benefit substitution, as measured by participation in *any alternative benefit programme* (SI, medical- and work-related rehabilitation, DI or SA), was 10.05 percentage points. By contrast, the estimated reduction in welfare dependence, as measured by participation in *any benefit programme* (all the alternative benefit programmes in addition to the transitional benefit programme), was 14.81 percentage points. Thus, despite the substantial amount of benefit substitution, the reforms on average reduced welfare dependence among single mothers. Note, however, that the estimated decrease in the participation rate in the transitional benefit programme for single mothers was 31.88 percentage points. This implies that the reduction in welfare dependence because of the reforms was far less than the reduction in the transitional benefit programme case-load.

We cannot extract similar information on welfare dependence from existing studies evaluating welfare reforms with respect to programme case-loads and/or labour market participation. For instance, Mogstad and Pronzato (2012) estimate that labour market participation among single mothers increased by about 4 percentage points because of the 1998 reform in Norway. They also show that the transitional benefit programme case-load fell from 65 per cent to 36 per cent within the same period. The corresponding decrease in welfare dependence would be overestimated if we interpret the decrease in the transitional benefit programme case-load as a pure dependence reduction, and correspondingly underestimated if we interpret the increase in labour market participation as the true dependence reduction.

To see this, we used Mogstad and Pronzato's (2012) definition of labour market participation and estimated single mothers' labour market participation in our sample to

increase by 4.71 percentage points due to the reforms (results not shown).[21] Note that this

increase is smaller than not only the decrease in the transitional benefit programme case-load

(31.88 percentage points) but also the decrease in overall welfare dependence (14.81

percentage points). Some single mothers may already be working while receiving welfare,

and they may increase their (existing) participation rather than starting afresh in the labour

force when they lose access to transitional benefits. Thus, more single mothers than those who

actually start to work may become self-sufficient. On the other hand, not all single mothers

who lose access to transitional benefits become self-sufficient. As this study shows, many

obtain access to benefits elsewhere, and these buffer the potential reduction in welfare

dependence. Note that the estimated decrease in the participation rate in the transitional

benefit program and the estimated increase in benefit substitution do not add up to the

estimated reduction in welfare dependence ($-31.88+10.05 \neq -14.81$). The reason is that some

single mothers were receiving alternative benefits in addition to transitional benefits. When

losing access to the latter, these mothers continued to be welfare dependent on the alternative

benefits without making a distinct switch between benefit programs. Alternative reform

responses of single mothers could be to rely on other household members, family or friends to

maintain some income. These alternatives are less relevant in this context, as less than 2 per

cent of the single mothers in the sample were neither working nor receiving benefits of any

kind.

To investigate the characteristics of single mothers who are prone to enter the specific

alternative benefit programmes in response to the reforms, we split the sample according to

the local labour market unemployment rates where the single mothers lived, their number of

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[21] We defined a mother as participating in the labour force if her measure of earnings, which included labour earnings in addition to welfare payments such as unemployment benefits, SI and parental leave benefits, exceeded one "basic amount" (a measure used by the Norwegian Social Insurance Scheme to determine a person's eligibility for a number of benefits and the magnitude of the payments he/she can receive). In 1998, one basic amount was 45,000 NOK (at 1998 prices).

years of education, their age and the age of their youngest child.[22] Table 4 displays the results. Table A1 in Appendix A details the subsample characteristics of single mothers in the pre-reform year of 1997.

[Table 4 "Benefit substitution effects of the reforms on single mothers by selected characteristics" about here]

We found that single mothers living in local labour markets with relatively high unemployment were considerably less likely to substitute SI benefits for transitional benefits in response to the reforms compared with single mothers in local labour markets with relatively low levels of unemployment (1.88 vs. 3.54 percentage points). The same holds for single mothers with fewer years of education relative to those with more years of education (2.31 vs. 3.38 percentage points). This is as expected, given that high local unemployment and low levels of education reduce the probability of finding employment and hence reduce the likelihood of becoming eligible for SI benefits. Note that the opposite holds true for all of the other alternative benefits. In particular, the statistically insignificant estimate for participation in medical- and work-related rehabilitation programmes for the sample of more-educated mothers (0.62 percentage points) is consistent with these mothers being eligible for SI benefits instead. Better-educated single mothers were also far less likely to be in need of SA payments compared with less-educated single mothers (3.97 vs. 7.33 percentage points).

Turning to older versus younger mothers, we see that older single mothers were less likely to receive SI benefits (1.72 vs. 4.57 percentage points) and were less dependent on SA payments (5.27 vs. 9.78 percentage points). By contrast, older mothers and those whose youngest child was aged 8–10 years were most likely to participate in the DI programme in

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[22] We split the sample according to whether the observation was above or below/equal to the median for single mothers in each year of the analysis (1992, 1996, 1997 and 2001). The median ranges for the years are 2.5–5.0 per cent for the local labour market unemployment rate, 32–35 years for the mothers' age and 10–12 years for their years of education.

response to the reforms (1.50 and 1.59 percentage points, respectively). This may of course be related to maternal age, but it may also be the case that these single mothers had relied on welfare for a longer period and hence experienced greater difficulty (re-)entering the labour market compared with younger single mothers with children aged 4–7 years.

5.2 Robustness

Married mothers constitute our preferred comparison group. Along with their ineligibility for transitional benefits, married mothers may face similar barriers to employment and challenges in the labour market resulting from having children to those of single mothers. Nevertheless, robustness tests using specifications with a range of other comparison groups increase the level of confidence in our results. We therefore supplemented the analysis by using single women without children and single mothers whose youngest child was aged 11–18 years as comparison groups.[23] In addition, we undertook a triple-difference analysis where *both* married mothers and single mothers whose youngest child was aged 11–18 years served as the comparison groups. This enabled us to simultaneously control for possible changes over time affecting mothers with children in the same age range *and* changes over time that affect single mothers differently from married mothers. Table 5 provides the results.

[Table 5 "Benefit substitution effects of the reforms on single mothers for alternative comparison groups" about here]

When using single women without children as the comparison group, we find that the estimates are somewhat larger than for married mothers, and when using single mothers whose youngest child was aged 11–18 years as the comparison group, the estimates are

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[23] As for the definition of single mothers whose youngest child was aged 4–10 years in Section 3.1, single women without children and single mothers whose youngest child was aged 11–18 years were defined as single if they were neither married nor cohabiting with a partner with whom they had children. These women were aged 19–55 years at the end of each calendar year, and were single either with or without children, both at the beginning and at the end of the calendar year.

smaller and less precise (except for SA payments). The estimates from the triple-difference test tend to be quite similar to the results when single mothers whose youngest child was aged 11–18 years are the comparison group. In the two latter specifications, the estimates for the SI and DI benefits are not statistically significant. However, the validity of the use of single mothers whose youngest child was aged 11–18 years as a comparison group is questionable. For instance, the reforms affect single mothers with children older than 10 years who experience a family dissolution, and cohabiting single mothers in stable relationships who have children in need of intensive supervision (see Table 1). Note also that some of the children who were older than 10 years after the reforms were 10 years or younger during the phase-in period of the reforms, and thus their mothers may have been affected. Nevertheless, we expect that single mothers whose youngest child was aged 11–18 years were less affected by the reforms than were single mothers whose youngest child was aged 4–10 years. Finding evidence of the reforms' effects with these alternative specifications, though of smaller magnitude and less precise in a statistical sense, is reassuring.

In addition, we constructed a series of alternative specifications for the main analyses using married mothers as the comparison group. We checked whether the results were sensitive to the exclusion of the control variables. Then we tested how the results changed if we did not include a comparison group, i.e. did not control for potential time-specific confounding factors common to both single and married mothers. This exercise elucidated whether the reforms changed the behaviour of single mothers independently of the relative behaviour of married mothers. Then we did not control for the pre-reform trends, i.e. we dropped the observations in the pre-reform period (1992 and 1996). In this specification, we ignored the fact that single mothers may respond somewhat differently to changes in the economic environment from married mothers. Further, we limited the sample to mothers whose youngest child was aged 6–10 years. Even though we constructed the sample so that

the mothers were ineligible for the cash subsidies for children aged one and two years at the time the outcomes were measured (see Section 2.1), some mothers in the sample with children aged 4 or 5 years in 2001 may have taken up these cash subsidies prior to 2001, in 1998 and 1999. We thus excluded these mothers. To investigate whether the increasing share of foreign (non-Norwegian) mothers may drive the results of the analysis over time, we limited the sample to Norwegian-born mothers only. Finally, we conducted a placebo test using only observations from the pre-reform years. In this specification, the pre-reform period consisted of the years 1992 and 1995, and the artificial reform period consisted of the years 1993 and 1996. Because there were no reforms within this period, we expected to identify no effects. Table 6 presents the results from these alternative specifications.

[Table 6 "Benefit substitution effects of the reforms on single mothers for alternative specifications" about here]

The estimates from the specification without controls are virtually identical to the main results. The relatively large estimates from the specification without a comparison group imply that single mothers do alter their behaviour in response to the reforms, and that we tend to overestimate the benefit substitution effects if we ignore time-specific factors. Not adjusting for the pre-trends causes the estimate of participation in the SA programme to decrease considerably (1.06 percentage points). This corresponds well with the descriptive evidence on SA payments: the pre-reform trend is steeper for single mothers than married mothers (see Figure 5). Thus, controlling for time-specific factors both common to single and married mothers *and* specific to single mothers is crucial for obtaining consistent estimates of benefit substitution. The estimates for the specifications for mothers whose youngest child was aged 6–10 years and Norwegian-born mothers only are essentially identical to the main results. It is reassuring to find that the placebo test produces rather small and, for the most part, statistically insignificant estimates.

Finally, we implemented an alternative difference-in-difference estimator whereby we utilized the fact that we have data for every year from 1992 to 2008. Here, we compared the outcomes for single and married mothers whose youngest child was aged 4–10 years over the period prior to the reforms (1992–1997) against those for single and married mothers whose youngest child was aged 4–10 years in the phase-in reform period (1998–2000) and the post-reform period (2001–2008). In addition to year dummies, we included a single mother-specific time trend.[24] The advantage of this difference-in-difference set-up is that it allows us to control for both linear and quadratic differences in single mother-specific trends. Table 7 displays the results.

[Table 7 "Benefit substitution effects of the reforms on single mothers using an alternative difference-in-difference specification" about here]

Columns (1) and (2) in Table 7 show the estimated effects when a linear trend is controlled for, while columns (3) and (4) show the estimated effects when both linear and quadratic trends are controlled for. In both specifications, the estimates for the phase-in period (columns (1) and (3)) are rather small compared with the estimates for the post-reform period (columns (2) and (4)). This is as expected: the reforms affect fewer single mothers in the phase-in period, so evidence of benefit substitution effects in this period is limited. The estimates for the post-reform period (columns (2) and (4)) are similar to those for the main specification (see Table 3). One notable exception is that the substitution of medical- and work-related rehabilitation benefits for transitional benefits is somewhat sensitive to how we control for the trend. In the specification with a quadratic trend, this estimate is insignificant

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[24]   Formally: $y_{itq} = \beta_1 + \beta_2 Single_i + \gamma_1(Phasein_q \times Single_i) + \gamma_2(Post_q \times Single_i) + \beta_3(t_t \times Single_i) + \lambda_t + X'_{itq}\theta + \varepsilon_{itq}$, where the notation is similar to expression (1). *Phasein* takes a value of one if the year is within the phase-in period of the reforms (1998–2000) and zero otherwise. *Post* takes a value of one if the year is within the post-reform period (2001–2008) and zero otherwise. *t* is a trend variable numbering 1 (1992) to 17 (2008) and λ_t is a vector of yearly fixed effects. The reform effects are the coefficients γ_1 and γ_2; these are the changes in the mean outcome over time in the phase-in period relative to the pre-reform period, and the post-reform period relative to the pre-reform period, respectively, for single mothers relative to married mothers.

(–0.02 percentage points). Note also that the estimates for the substitution of SI benefits for transitional benefits are larger (4.12 and 3.07 percentage points) and the estimates for the substitution of SA payments are smaller (4.04 and 6.09 percentage points) than for the main specification. Thus, in the longer run, more single mothers with children aged 4–10 years may have entered the SI programme because of the reforms, and correspondingly fewer may have joined the SA programme. These results should, however, be interpreted with some caution. In the years following 2001, there have been further reforms to both the DI programme (the introduction of time-limited DI benefits) and the SI programme that may have affected single and married mothers differently.[25] In addition, we have no data on medical- and work-related rehabilitation benefits after 2001, and both the the cash subsidies for children aged one and two years and the lowering of the school starting age (see Section 2.1) could potentially influence the results. Thus, our main specification, although more restricted, is preferred. Overall, the robustness specifications support our main results.

5.3 Threats to validity

One potential concern may be that the mother's marital status could be endogenous with respect to the reforms. That is, if the reforms make it more costly to be a single mother, the likelihood that single mothers become married mothers may increase. If so, the estimated results are inconsistent. To investigate this, we examined the yearly transition rates from single to married motherhood, and vice versa, for mothers whose youngest child was aged 4–10 years (or those most affected by the reforms) and mothers whose youngest child was aged 11–18 years (or those less affected by the reforms). It is reassuring to find that the two groups of mothers follow the same trends, and that there are no changes in the relative probabilities of becoming a single or married mother within the reform period (results not shown).

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[25] Reforms to the SI programme in 2004 increased the number of weeks persons had to work to be entitled to benefits from two to four weeks, and required recipients who had been ill for more than eight weeks to engage in some work-related activity in order to qualify (unless they were unable to do so for medical reasons).

The reforms could also potentially influence the choice of mothers to have another child. The 1998 reform introduced both a three-year time limit on the receipt of benefits and a three-year implementation period from 1998 to 2001. Thus, single mothers receiving transitional benefits continuously since 1997 would have needed to have another child in 2000 to remain eligible for benefits. Investigating the yearly probabilities of having another child for single and married mothers whose youngest child was aged 4–10 years, we do in fact find a small (but statistically significant) relative increase in the probability of having another child among single mothers in 2000 (results not shown). Given that single mothers who speculate about having another child in order to stay on transitional benefits may also be more prone to take up alternative benefits, the exclusion of these mothers from the sample could bias the estimates of benefit substitution downwards. However, the probability of becoming a single mother through having one's first child also increased somewhat in 2000. We believe it is unlikely that the reforms influenced these first-time mothers, so there would appear to be some factor unrelated to the reforms that influenced births among single mothers in 2000. Whatever the cause, there was only an increase of about 200 births among single mothers whose youngest child was aged 4–10 years in 2000. Relative to the approximately 40,000 single mothers in the sample each year, this is a rather small amount, so any resulting bias is likely to be small. This corresponds well with what Moffitt (2007) and Blank (2002) find in reviewing the US literature, namely that the effects of welfare reform on marriage and fertility are very small, if they exist at all.

6. Conclusion

Policy-makers in developed countries have confronted welfare dependence and poverty among single mothers by imposing reforms on welfare programmes targeted at this group. Such reforms have included work requirements and time limits on the receipt of welfare, and thus they have operated to restrict single mothers' access to these benefits. These reforms

have served to reduce programme case-loads and generally increased the employment and earnings of single mothers. However, it is unlikely that all single mothers have successfully managed the transition from welfare to work. Given that most developed countries have comprehensive income security systems for their residents, single mothers may have the option to switch to other income security programmes when access to their own specific welfare programme is reduced, i.e. so-called benefit substitution.

We investigated whether reforms that decreased access to the transitional benefit programme targeted at single mothers in Norway encouraged single mothers to substitute alternative benefits for their prior welfare payments. Using a difference-in-difference approach, we found evidence of considerable benefit substitution by single mothers by switching to alternative benefit programmes. While the reforms decreased the participation rate in the transitional benefit programme by 32 percentage points, the estimated effect of the reforms on total benefit substitution was 10 percentage points. Overall, we found that welfare dependence among single mothers decreased by 15 percentage points because of the reforms. A series of robustness checks confirmed these findings.

These results imply that single mothers who left the transitional benefit programme did not necessarily become self-sufficient, and many continued to depend on welfare. Thus, decreases in programme case-loads do not reflect equal reductions in welfare dependence. From a policy perspective, this paper stresses the importance of taking into account the entire income security system when designing welfare reforms aimed at getting individuals off welfare and into work.

More specifically, the phenomenon of single mothers switching to health-related benefits should be of concern for policy-makers. The increased uptake of disability insurance (DI) benefits is especially worrying in that these mothers are likely to remain dependent on DI benefits for the rest of their work-aged lives. It is an open question as to whether, in the

absence of these reforms, single mothers would have delayed their participation in health-related benefit programmes until their transitional benefits eventually expired, or whether the reforms made them sick or influenced them to access benefits they were not entitled to. Although this question is yet to be answered, this study may justify increased scrutiny by general physicians and stricter screening processes for health-related benefits.

Some single mothers may also need extra time or support to manage the transition from welfare to work. The increased take-up of social assistance (SA) payments, generally considered a benefit of last resort, may also indicate that the three-year time limit on the receipt of benefits is too short or perhaps that the work requirements are too hard to fulfil, at least for some single mothers. A final interesting question for future research is how the reforms affect the welfare dependence of single mothers over the longer run, i.e. among single mothers who have never been part of the pre-reform programme. Single mothers who enter motherhood in the post-reform period may view welfare as less attractive and consequently alter their behaviour in order to be less welfare dependent.

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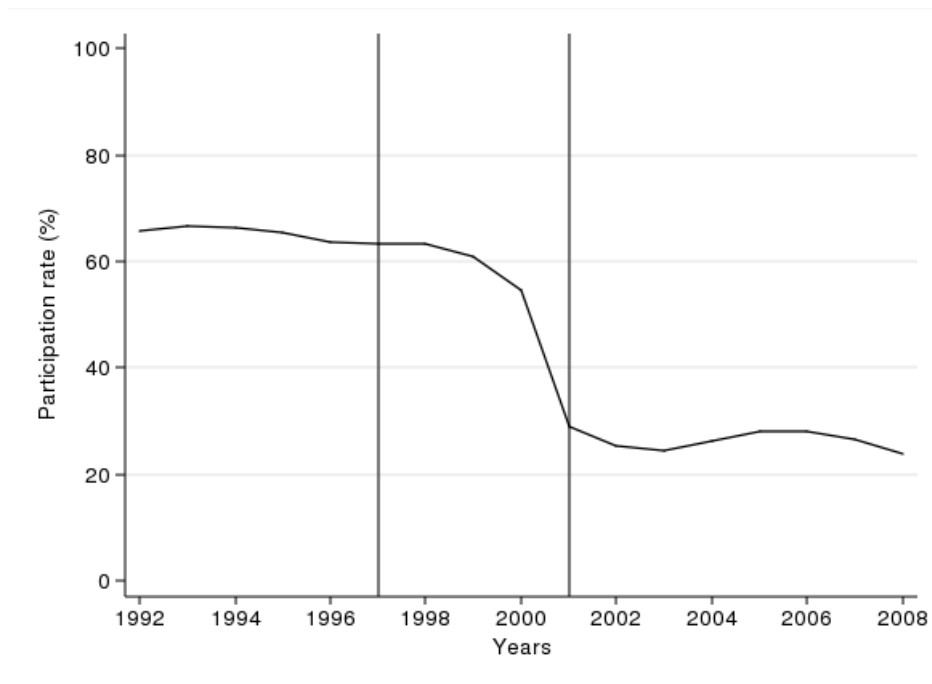
Figure 1: Participation in the transitional benefit programme

Notes: In each year, the sample comprises single mothers whose youngest child was aged 4–10 years. The vertical lines indicate the years before (1997) and after (2001) the phase-in period of the reforms.

Figure 2: Participation in the sickness insurance (SI) programme

Notes: In each year, the sample comprises single and married mothers whose youngest child was aged 4–10 years. The vertical lines indicate the years before (1997) and after (2001) the phase-in period of the reforms.
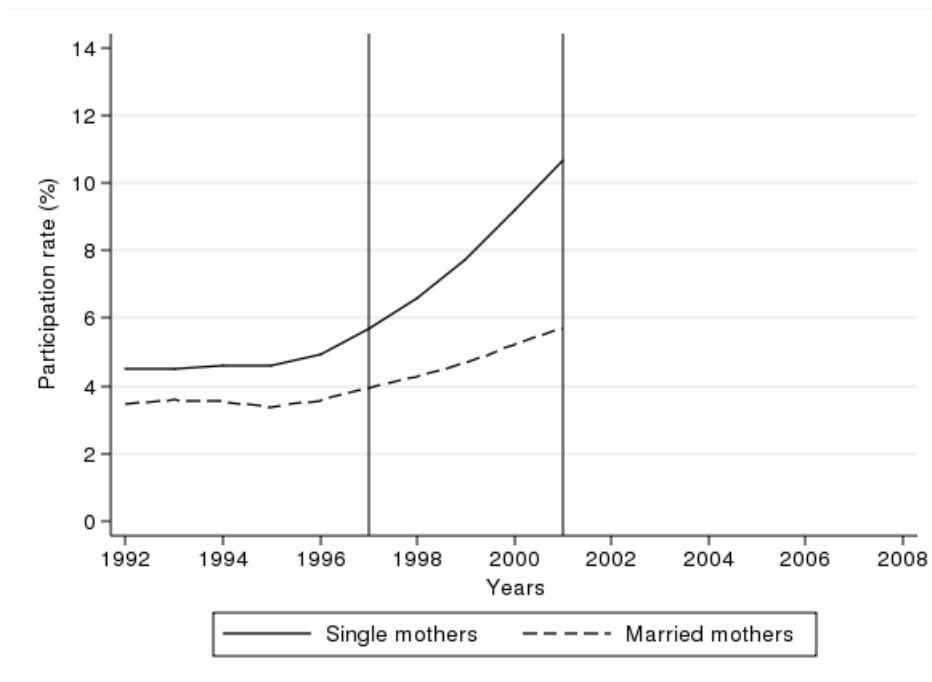
Figure 3: Participation in medical- and work-related rehabilitation benefits programmes

Notes: In each year, the sample comprises single and married mothers whose youngest child was aged 4–10 years. The vertical lines indicate the years before (1997) and after (2001) the phase-in period of the reforms. The data on medical- and work-related rehabilitation benefits are limited to the years 1992 to 2001.
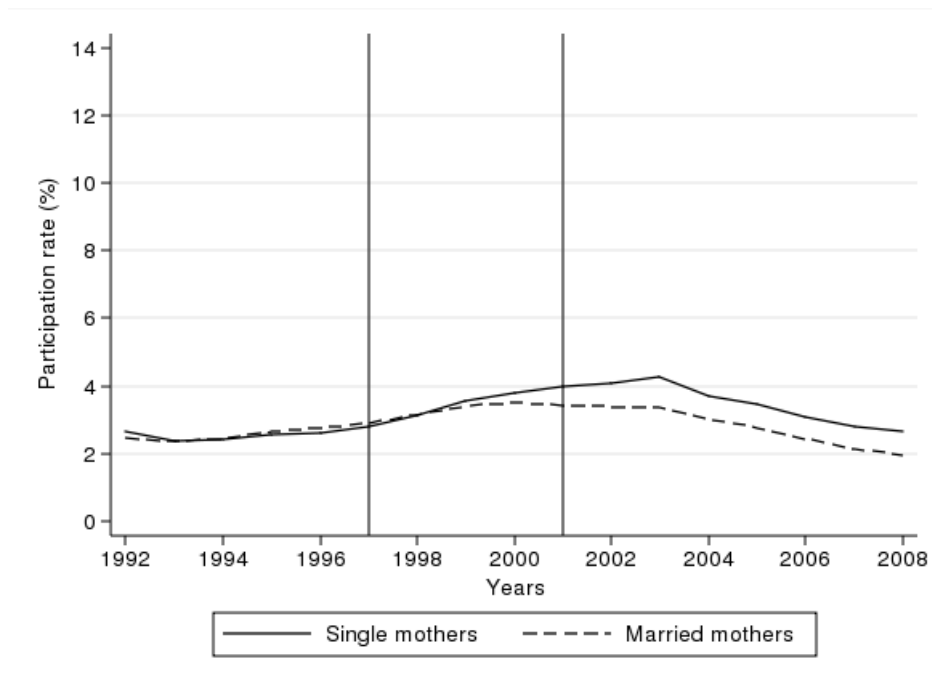
Figure 4: Participation in the disability insurance (DI) programme

Notes: In each year, the sample comprises single and married mothers whose youngest child was aged 4–10 years. The vertical lines indicate the years before (1997) and after (2001) the phase-in period of the reforms.

Figure 5: Participation in the social assistance (SA) programme

Notes: In each year, the sample comprises single and married mothers whose youngest child was aged 4–10 years. The vertical lines indicate the years before (1997) and after (2001) the phase-in period of the reforms. A woman was also defined as participating in the SA programme if her husband participated.

Table 1. Features of the 1998 and 1999 reforms of the transitional benefit programme

Characteristics	Pre-reform	Post-reform
1998 reform		
Time limit[a]	None	3 years
Age limit	Youngest child has finished third grade of primary school (9–10 years old)	Youngest child is less than 8 years old
Work requirement[b]	None	Youngest child is aged 3 years or older
Max. benefit level[c]	6,171 NOK (1998 prices) per month in 1998	6,995 NOK (1998 prices) per month in 1998
Short-term (1–2 years) eligibility after family dissolution (child > age limit)	Youngest child between 10 and 18 years of age	Youngest child between 8 and 10 years of age
Child requires intensive supervision because of disability, illness or severe social issues	Child less than 18 years of age	Child less than 18 years of age
Asset means-tested	No	No
1999 reform		
Cohabitation status	Not eligible if the couple have children in common or are married	Not eligible if the relationship has lasted for at least 12 of the past 18 months

Notes: a) The time limit relates to the mother's youngest child and resets to three years for every newborn child. Benefits may be awarded in non-consecutive periods. b) Work requirements include working for at least half of the hours of a standard working week in Norway (37.5 hours), studying for at least half of the hours of a full-time study or registering as unemployed at the government agency of the Labour and Welfare Service. c) The maximum benefit level was obtained if the mother had earnings from work, or received SI benefits, below a threshold of 1,891 NOK (1998 prices) per month in 1998, and did not receive medical- or work-related rehabilitation or DI benefits. 100 NOK = approx. 14 EUR and 18 USD.

Table 2. Characteristics of mothers in the pre-reform (1992 and 1996) and reform (1997 and 2001) periods

Characteristics (mean)[a]	Single mothers				Married mothers				DD estimate (no controls)[c]
	Pre-reform		Reform		Pre-reform		Reform		
	1992	1996	1997	2001	1992	1996	1997	2001	
Age	32.88	33.57	33.88	34.94	36.73	37.17	37.35	37.98	0.17**
									(0.07)
No. of obs.	32,919	42,477	43,705	45,629	13,5437	143,516	146,131	153,893	743,707
Years of education	10.87	11.27	11.36	11.71	11.28	11.73	11.83	12.23	–0.01
									(0.02)
No. of obs.	31,732	40,810	41,939	44,299	12,9928	136,579	138,886	148,305	712,478
Non-Norwegian (%)	4.84	6.46	6.77	7.91	5.49	7.67	8.00	9.70	–0.64***
									(0.24)
No. of obs.	32,207	42,477	43,705	45,629	13,1408	143,516	146,131	153,893	738,966
No. of children	1.68	1.71	1.73	1.83	2.32	2.34	2.35	2.39	0.05***
									(0.01)
No. of obs.	32,919	42,477	43,705	45,629	13,5437	143,516	146,131	153,893	743,707
Age of youngest child	6.74	6.78	6.86	7.02	6.80	6.64	6.66	6.74	–0.11***
									(0.02)
No. of obs.	32,919	42,477	43,705	45,629	13,5437	143,516	146,131	153,893	743,707
Unemployment rate (%)	5.18	4.50	3.69	2.50	5.18	4.47	3.66	2.49	–0.05***
									(0.01)
No. of obs.	32,919	42,477	43,705	45,629	13,5437	143,516	146,131	153,893	743,707

| Proportion of single mothers[b] | 0.196 | 0.228 | 0.230 | 0.229 | 0.222 |

Notes: In each year, the sample comprises single and married mothers whose youngest child was aged 4–10 years. a) Mothers with missing information for specific characteristics were excluded from the calculation of the relevant means. b) The proportion of single mothers among all mothers (including mothers with missing information). c) Difference-in-difference (DD) estimates obtained after specifying each characteristic separately as the dependent variable in OLS estimations of expression (1).

***, ** and * indicate significance at the 1%, 5% and 10% level, respectively (robust standard errors).

Table 3. Benefit substitution effects of the reforms on single mothers

Dependent variable	Estimate	Single mothers' outcome (%) 1997[c]
Transitional benefits	–31.88*** (0.45)	63.20
SI benefits	2.66*** (0.43)	18.99
Medical- and work-related rehab. benefits	2.86*** (0.26)	5.68
DI benefits	0.97*** (0.19)	2.80
SA payments	8.15*** (0.39)	16.60
Total benefit substitution[a]	10.05*** (0.53)	37.91
Welfare dependence[b]	–14.81*** (0.47)	77.15
No. of obs.	*743,707*	*43,705*

Notes: Each estimate is from a separate OLS estimation of expression (1) for the respective outcome. Controls were included. a) The outcome of total benefit substitution is defined as participation in *any alternative benefit programme* (SI, medical- and work-related rehabilitation, DI or SA). b) The outcome of welfare dependence is defined as participation in *any benefit programme*, i.e. any of the alternative benefit programmes in addition to the transitional benefit programme. c) Single mothers' outcome (%) 1997 refers to the mean outcome of single mothers in the pre-reform year of 1997.
***, ** and * indicate significance at the 1%, 5% and 10% level, respectively (robust standard errors).

Table 4. Benefit substitution effects of the reforms on single mothers by selected characteristics

Dependent variable	(1) High local unemp.	(2) Low local unemp.	(3) Highly educated	(4) Less educated	(5) Older mother	(6) Younger mother	(7) Youngest child 8–10	(8) Youngest child 4–7
Transitional benefits	−34.12*** (0.63)	−30.27*** (0.65)	−27.66*** (0.76)	−37.75*** (0.56)	−29.81*** (0.67)	−35.66*** (0.57)	−42.38*** (0.73)	−23.11*** (0.58)
SI benefits	1.88*** (0.61)	3.54*** (0.61)	3.38*** (0.74)	2.31*** (0.56)	1.72*** (0.62)	4.57*** (0.65)	2.02*** (0.71)	2.77*** (0.54)
Medical- and work-related rehab. benefits	3.06*** (0.37)	2.64*** (0.36)	0.62 (0.38)	3.19*** (0.36)	3.14*** (0.38)	3.34*** (0.39)	3.00*** (0.43)	2.67*** (0.33)
DI benefits	1.27*** (0.28)	0.63** (0.26)	0.97*** (0.23)	1.02*** (0.28)	1.50*** (0.33)	0.69*** (0.20)	1.59*** (0.35)	0.47** (0.21)
SA payments	8.85*** (0.57)	7.39*** (0.55)	3.97*** (0.47)	7.33*** (0.56)	5.27*** (0.52)	9.78*** (0.60)	7.51*** (0.59)	8.69*** (0.52)
Total benefit substitution[a]	10.39*** (0.76)	9.70*** (0.74)	7.67*** (0.85)	9.21*** (0.71)	7.36*** (0.74)	13.41*** (0.81)	9.29*** (0.85)	10.24*** (0.68)
Welfare dependence[b]	−15.59*** (0.65)	−14.49*** (0.68)	−16.57*** (0.87)	−17.59*** (0.57)	−13.49*** (0.71)	−16.28*** (0.68)	−20.68*** (0.80)	−10.04*** (0.58)
No. of obs.	366,064	377,643	299,847	412,631	509,215	234,492	279,210	464,497

Notes: Each estimate is from a separate OLS estimation of expression (1) for the respective outcome for the relevant subsample of mothers. Controls were included. The sample was split according to whether the observation was above or below/equal the median for single mothers in each year of the analysis (1992, 1996, 1997 and 2001). a) The outcome of total benefit substitution is defined as participation in *any alternative benefit programme* (SI, medical- and work-related rehabilitation, DI or SA). b) The outcome of welfare dependence is defined as participation in *any benefit programme*, i.e. any of the alternative benefit programmes in addition to the transitional benefit programme.

***, ** and * indicate significance at the 1%, 5% and 10% level, respectively (robust standard errors).

Table 5. Benefit substitution effects of the reforms on single mothers for alternative comparison groups

Dependent variable	(1) Baseline (married mothers)	(2) Single women, no child	(3) Single mothers, child 11–18	(4) Married and single mothers, child 11–18
Transitional benefits	−31.88*** (0.45)	−31.61*** (0.46)	−29.49*** (0.48)	−30.34*** (0.49)
SI benefits	2.66*** (0.43)	2.55*** (0.43)	1.15* (0.60)	0.94 (0.66)
Medical- and work-related rehab. benefits	2.86*** (0.26)	3.75*** (0.27)	1.23*** (0.38)	1.75*** (0.41)
DI benefits	0.97*** (0.19)	0.77*** (0.22)	−0.44 (0.34)	0.59 (0.38)
SA payments	8.15*** (0.39)	9.54*** (0.41)	9.62*** (0.52)	9.13*** (0.54)
Total benefit substitution[a]	10.05*** (0.53)	11.80*** (0.54)	8.55*** (0.71)	8.87*** (0.78)
Welfare dependence[b]	−14.81*** (0.47)	−12.94*** (0.49)	−15.01*** (0.67)	−15.23*** (0.75)
No. of obs.	*743,707*	*699,327*	*305,020*	*1,402,904*

Notes: Each estimate in columns (1–3) is from a separate OLS estimation of expression (1) for the respective outcome for the relevant subsample of women. Column (4) uses a trend-adjusted triple-difference specification, whereby both married mothers whose youngest child was aged 4–10 years and single mothers whose youngest child was aged 11–18 years (relative to married mothers whose youngest child was aged 11–18 years) were the comparison groups. Controls were included. a) The outcome of total benefit substitution is defined as participation in *any alternative benefit programme* (SI, medical- and work-related rehabilitation, DI or SA). b) The outcome of welfare dependence is defined as participation in *any benefit programme*, i.e. any of the alternative benefit programmes in addition to the transitional benefit programme.

***, ** and * indicate significance at the 1%, 5% and 10% level, respectively (robust standard errors).

Table 6. Benefit substitution effects of the reforms on single mothers for alternative specifications

Dependent variable	(1) Baseline	(2) No controls	(3) No comparison group	(4) No control of pre-reform trend	(5) Youngest child 6–10	(6) Norwegians only	(7) Placebo
Transitional benefits	−31.88*** (0.45)	−31.84*** (0.47)	−28.19*** (0.44)	−33.95*** (0.30)	−37.93*** (0.55)	−31.35*** (0.47)	−2.62*** (0.48)
SI benefits	2.66*** (0.43)	2.67*** (0.43)	4.02*** (0.39)	3.88*** (0.31)	2.53*** (0.53)	2.86*** (0.45)	0.60 (0.41)
Medical- and work-related rehab. benefits	2.86*** (0.26)	2.90*** (0.26)	4.65*** (0.24)	3.18*** (0.18)	3.20*** (0.32)	2.83*** (0.27)	0.32 (0.24)
DI benefits	0.97*** (0.19)	1.02*** (0.19)	1.31*** (0.17)	0.47*** (0.13)	1.13*** (0.25)	0.85*** (0.19)	−0.02 (0.18)
SA payments	8.15*** (0.39)	8.29*** (0.40)	8.82*** (0.38)	1.06*** (0.25)	8.10*** (0.46)	7.42*** (0.40)	0.15 (0.43)
Total benefit substitution[a]	10.05*** (0.53)	10.21*** (0.53)	13.61*** (0.48)	4.53*** (0.36)	10.08*** (0.64)	9.68*** (0.55)	0.70 (0.54)
Welfare dependence[b]	−14.81*** (0.47)	−14.68*** (0.48)	−9.08*** (0.41)	−18.15*** (0.33)	−18.26*** (0.59)	−15.08*** (0.49)	−1.96*** (0.47)
No. of obs.	*743,707*	*743,707*	*164,730*	*389,358*	*492,893*	*683,250*	*703,892*

Notes: Each estimate is from a separate OLS estimation of expression (1) for the respective outcome for the relevant sample of mothers. Married mothers were the comparison group. Controls were included. a) The outcome of total benefit substitution is defined as participation in *any alternative benefit programme* (SI, medical- and work-related rehabilitation, DI or SA). b) The outcome of welfare dependence is defined as participation in *any benefit programme*, i.e. any of the alternative benefit programmes in addition to the transitional benefit programme.

***, ** and * indicate significance at the 1%, 5% and 10% level, respectively (robust standard errors).

Table 7. Benefit substitution effects of the reforms on single mothers using alternative difference-in-difference specifications

Dependent variable	Linear trend		Linear + quadratic trend		
	(1) Phase-in period (1998–2000)	(2) Post-reform period (2001–2008)	(3) Phase-in period (1998–2000)	(4) Post-reform period (2001–2008)	(3) *No. of obs.*
Transitional benefits	–3.25*** (0.20)	–33.78*** (0.29)	–2.04*** (0.25)	–32.68*** (0.33)	*3,255,978*
SI benefits	2.68*** (0.20)	4.12*** (0.31)	1.52*** (0.23)	3.07*** (0.32)	*3,255,978*
Medical- and work-related rehab. benefits[a]	0.90*** (0.15)	2.26*** (0.24)	–0.03 (0.21)	–0.02 (0.42)	*1,857,099*
DI benefits	0.21** (0.09)	0.81*** (0.13)	0.28*** (0.10)	0.87*** (0.14)	*3,255,978*
SA payments	–1.77*** (0.16)	4.04*** (0.24)	0.49** (0.20)	6.09*** (0.28)	*3,255,978*
Total benefit substitution[b]	0.76*** (0.23)	6.37*** (0.36)	2.15*** (0.29)	7.64*** (0.38)	*3,255,978*
Welfare dependence[c]	–2.10*** (0.21)	–16.87*** (0.34)	–0.88*** (0.25)	–15.77*** (0.35)	*3,255,978*

Notes: The estimates in each row of columns (1) + (2) [and columns (3) and (4)] are from separate OLS estimations of the expressions $y_{itq} = \beta_1 + \beta_2 Single_i + \gamma_1(Phasein_q \times Single_i) + \gamma_2(Post_q \times Single_i) + \beta_3(t_t \times Single_i) + [\beta_4(t_t \times Single_i)^2] + \lambda_t + X'_{itq}\theta + \varepsilon_{itq}$ for the respective outcomes. Columns (1) + (3) and columns (2) + (4) display estimates of γ_1 and γ_2, respectively. Controls were included. a) The register for medical- or work-related rehabilitation benefits was limited to the years 1992 to 2001. b) The outcome of total benefit substitution is defined as participation in *any alternative benefit programme* (SI, medical- and work-related rehabilitation, DI or SA). c) The outcome of welfare dependence is defined as participation in *any benefit programme*, i.e. any of the alternative benefit programmes in addition to the transitional benefit programme.
***, ** and * indicate significance at the 1%, 5% and 10% level, respectively (robust standard errors).

Appendix A

Table A1. Description of single mothers in the pre-reform year 1997 by selected characteristics

	(1) High local unemp.	(2) Low local unemp.	(3) Highly educated	(4) Less educated	(5) Older mother	(6) Younger mother	(7) Youngest child 8–10	(8) Youngest child 4–7
Outcomes (%)								
Transitional benefits	68.06	58.73	49.67	72.51	49.10	76.91	55.18	68.35
SI benefits	18.75	19.21	18.33	19.87	20.97	17.06	21.44	17.41
Medical- and work-related rehab. benefits	5.89	5.49	4.83	6.46	6.28	5.10	6.23	5.33
DI benefits	2.88	2.73	1.27	3.90	4.62	1.03	3.72	2.21
SA payments	17.12	16.12	7.16	21.88	13.93	19.19	13.44	18.63
No. of obs.	*20,934*	*22,771*	*17,516*	*24,423*	*21,542*	*22,163*	*17,101*	*26,604*
Characteristics (mean)								
Age	33.50	34.23	34.34	33.42	38.79	29.11	36.43	32.24
Years of education	11.30	11.42	13.32	9.96	11.63	11.11	11.45	11.31
Non-Norwegian (%)	4.44	8.91	4.05	3.68	8.00	5.56	6.53	6.92
No. of children	1.76	1.71	1.59	1.81	2.08	1.39	1.81	1.68
Age of youngest child	6.85	6.87	6.88	6.85	7.49	6.24	8.94	5.52
Unemployment rate (%)	4.40	3.03	3.67	3.72	3.64	3.73	3.67	3.70

Chapter 3

Single mothers and their children:
Evaluating a work-encouraging welfare reform

Single Mothers and their Children: Evaluating a Work-Encouraging Welfare Reform[*]

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Abstract: Using rich administrative data from Norway, we evaluate a 1998 work-encouraging reform targeted at single parents. We especially focus on educational performances for children of the involved single mothers. For all children of single mothers, the effect on school grades at completion of junior high school at age 16 is near zero and insignificant. If one concentrates on younger single mothers, those most likely to be affected by the reform, the grade point average of their children drops significantly by 7% of a standard deviation. We isolate groups of mothers that are affected by the reform either primarily by having less time at home, or by reduced income. The children of both groups of mothers experience drops in school grades. We conclude that the mechanism behind these grade-drops then must both be reduced available parental time and reduced income.

Keywords: Welfare reform, single mothers, child development, time and money investments.
JEL codes: I24, I38, J13.

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I. Introduction

Single parenthood is often associated with low attachment to the labor market, dependence on welfare, low income and reduced opportunities for children, a concern for many policy makers. In 1998 Norway introduced a work-encouraging reform targeted at single parents, where the maximum benefit period for support was substantially reduced. We study the long-term consequences of this reform, with particular emphasis on the educational outcomes for the children of the involved single parents. One could hope that the reform would lead to higher incomes for single parent headed households and perhaps also to different attitudes towards work, education and welfare benefits.[1] This could in turn lead to better school outcomes for children. We find that for the whole population of single mothers, there is no significant impact on school grades upon leaving junior high school (at age 16). When we isolate the younger single mothers, their children actually experience a significant fall in school grades.

The Norwegian single parent reform is similar in spirit to reforms that have taken place in many other countries. The 1996 welfare reform in the US was a source of inspiration for the reforms that followed suit many other places, and is also the reform that has been most intensely evaluated.[2] Welfare benefits for the poor are in the US largely targeted at low-income families with children, and most of these are headed by a single mother. In 1996 the Aid to Families with Dependent Children (AFDC) was renamed Temporary Assistance to Needy Families (TANF). Credible and enforceable work requirements were introduced, as was time limits on the receipt of benefits. Following the reform, the employment rates of single mothers rose, income went up and poverty rates dropped.[3] Evidence on how the 1996 US welfare reform affected long-term outcomes for children is scant and points in various directions. Dunifon, Kalil and Danziger (2003) use survey data to investigate the effects of mothers moving from welfare to work. They conclude that "moving from welfare-reliance to combining welfare and work is associated with a decrease in harsh parenting, an increase in positive parenting, and decreases in both internalizing and externalizing behavior problems among children". Paxson and Waldfogel (2003) present a darker picture. They use state level data to suggest that welfare reforms may have increased child maltreatment. The studies closest to our are Miller and Zhang (2009, 2012). They look at the effects of welfare reforms in the US on academic

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[1]Theoretical and empirical work on intergenerational transmission of work attitudes can be found in Crompton and Harris (1998), Fernández and Fogli (2009), Fernández (2013), Dahl, Kostøl and Mogstad (2013), Haaland, Rege, Telle and Vortruba (2013), and Alesina and Giuliano (2013).

[2]See for example Danziger, Corcoran, Danziger and Heflin (2000), Blank (2002), Moffitt (2003, 2007) and Grogger and Karoly (2009).

[3]Card and Blank (2008) cautions that while average earnings may have risen, jobs and earnings can also have become more unstable, and when public assistance is less available, within-year variability of income may rise. Kaushal, Gao and Waldfogel (2007) point out that while single mothers may have experienced an increase of income, expenditure data reveal that much of this income hike was spent on transportation, work clothes and the like, while little was used on what the authors term "learning and enrichment items" for children.

performance. Contrary to our findings of no (or negative) effects they find a positive effect of welfare reforms on children's education in the US. Our data allow us to delve deeper into questions related to heterogeneity among single parents and also the precise mechanisms through which welfare arrangements influence school outcomes. To cite Moffitt (2007), "... the reform has had generally positive average effects on employment, earnings, and income, and generally negative effects on poverty rates, although the gains are not evenly distributed across groups. A fraction of the affected group appears to have been made worse off by the reform". This precisely seems to suggest that the light should be turned on heterogeneity in response to single parent welfare reforms.

The US welfare reforms were a source of inspiration for many other countries, even countries with more comprehensive welfare systems than the US. Many countries enacted work requirements and limited benefit duration to get welfare recipients out of a perceived trap of benefit dependency, poverty and inactivity. Single mothers were often not the main target for these reforms, and some places single mothers were exempted from work requirements. For example, only in 2008, Britain introduced the Lone Parent Obligations which mandated that single parenthood alone should not entitle anyone to seek income support, and that single parents in general were expected to seek suitable work.[45] Why is the Norwegian case interesting? The reform followed rather shortly after the US one, so long-term outcomes have had time to play out, opposite many other countries. In particular, children affected by the reform now start to finish junior high school, so school outcomes can be studied. In Norway, researchers have access to excellent administrative registry data, covering the whole population, which obviously is an advantage if one wants to study heterogeneity in effects and underlying mechanisms behind the results. The Norwegian reform is very similar to the US experience, but unlike the US, Norway is a comprehensive welfare state. Since many countries seek inspiration in the US reform, it is important that this type of reform is evaluated in countries with different welfare systems. Mogstad and Prozato (2012) provided the first evaluation of the Norwegian reform, with an eye on outcomes for mothers. Similar to the US experience, they find that the reform increased labor market participation and earnings among single mothers. However, they find reduced income and increased poverty among a subgroup of single mothers who had been single for a prolonged period. Reiso (2014) shows that Norwegian welfare reforms increased single mothers' take-up of alternative benefits such as health-related benefits and social assistance. This

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[4]Britain initially relied on the so-called New Deal for Lone Parents, which was a voluntary program offering single parents advice and assistance to increase their employability. See Dolton and Smith (2011) for an evaluation this program.

[5]Australia, as part of a series of 'work first' welfare reforms, in 2006 demanded that single parents with children older than six should seek employment. The Netherlands is another case where work-first type welfare reforms were enacted from the 1990s onwards. In 1996 work requirements were extended to single parents, but rules have varied, and since 2008 single parents with small children can apply for exemption from job search requirements (see Finn and Gloster (2010) for further details on the reforms in Britain, Australia, and The Netherlands). For futher information see for instance Ochel (2005) for details on the German Hartz reforms, and Knoef and van Ours (2014) for a report on a Dutch field experiment to encourage single mothers to leave welfare for work.

illustrates the importance of heterogeneity when analyzing welfare reform effects on the children of single mothers. It also points to an important difference between comprehensive welfare states and the US, those single parents who do not find jobs after work-encouraging reforms will to a lesser degree fall into dire poverty when there are last-resort social assistance arrangements that always will provide everyone with some income. Norway has also a battery of family policy measures that provide the population with generous maternity benefits, various forms of cash support and highly subsidized day care.[6] This of course makes it different to be a single mom in a comprehensive welfare state than in the US, but some of the qualitative effects will remain. A work-encouraging reform will entice some single parents to a path with higher income and closer labor market integration. Others will not find work, and suffer from worsened benefit availability. The effects on children's outcomes should be expected to be as diverse as the effects on their mothers.

We use a differences-in-differences method where we define the treatment group as being single when the child is aged two and the control group as being married or cohabiting (with a common child) when the child is aged two. Throughout the paper we refer to the control group as married mothers, but this includes mothers cohabitating with the father of any of their children. We study children who are aged two in the ten years predating the reform (1988-1997), and we have final year school grades or all these individuals at completion of junior high school. The children of the single and married groups are split into three segments. The first segment, aged two in 1988-1990, is untreated throughout childhood (aged 3-10). The second segment, aged two in 1991-1994, is partially treated at the end of the childhood period (aged 6-10). Finally, the third segment, aged two in 1995-1997, is treated throughout childhood (aged 3-10). Notice that for each cohort within the segments, children get one more year of treatment. We explore this in a robustness test using a linear treatment variable. The main challenge to our estimation strategy is that single and married/cohabiting mothers are quite different, and face different labor market trends over time. This means that our samples of single and married mothers when child is aged two are not constant over time. We conduct several robustness tests to separate the reform effect from any other differences between single and married mothers over time. This includes controlling for a range of observable characteristics, and to match the group of single mothers to a similar group of married mothers.

The reform had little average effects on children's school grades in junior high school. However, the heterogeneity in the reform responses by the mothers makes the average effects of the reform less interesting. When we split the sample into younger and older mothers, defined as above/below the median age of single mothers, we find a negative effect for the children of younger single

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[6]For presentations and evaluations of elements of Norwegian family policy, see Havnes and Mogstad (2011a, 2011b), Dahl, Løken, Mogstad and Salvanes (2013), Drange and Rege (2013), Rege and Solli (2013), Black, Devereux, Løken and Salvanes (2014), Carneiro, Løken and Salvanes (2014), and Dahl, Løken and Mogstad (2014).

mothers. The effect is about 7% of a standard deviation both for grade point average and written/oral exams in the final year of junior high school. For the children of older single mothers, there is no significant effect on the educational outcomes. When studying the responses of younger single mothers to the reform, we find that younger single mothers worked more, however, just enough to offset the loss in benefits. Thus, apparently for this group, there is no income effect of the reform. However, there is a time effect away from home as these mothers work more. A further analysis where the sample is split according to mothers' pre-reform labor market attachment, reveals that for mothers not working pre-reform, there is no effect on work, however a big loss in income due to lower benefits. For mothers working pre-reform, there is an increase in work, however only a marginal increase in income. For both subgroups, we find negative effects on children's school grades. Linking this to mechanisms, we suggest that children of mothers not working pre-reform are affected through a reduction in income, and not a time effect, since these mothers do not work more post-reform (this is supported by Dahl and Lochner (2012) and Løken, Mogstad and Wiswall (2012) who find positive effects on child outcomes of increased family income in poor families). Mothers working pre-reform suffered no negative income effect, so the likely mechanism here is that mothers were more away from home. Depending on the quality of both the alternative care and maternal care, and also the age of the child, this reduction in time at home could be either positive or negative for the child (Becker, 1981; Baker, Gruber and Milligan, 2008; Havnes and Mogstad, 2011b; Carneiro, Løken and Salvanes, 2014). Since we find negative effects, it is likely that children are more at home unsupervised or that the quality of alternative after-school care is not of the same standard as mothers' time. Consistent with unsupervised time or low quality of after-school care, we also find stronger reform effects on the children when the mothers have a weak social network, measured as distance to the single mothers' own parents. Grandparents seem to be a good substitute to mothers' time at home.

The remainder of the paper proceeds as follows. Section II gives background information on single mothers and welfare reforms in Norway. In Sections III and IV, we discuss our data, and threats to identification. Section V presents our main findings, and Section VI explores possible mechanisms. Finally, Section VII offers some concluding remarks.

II.   Background

A.   *Descriptives of single mothers in Norway*

In Appendix Table A.1 we compare characteristics of single to married mothers in Norway. We see that single mothers are younger, less educated, work less and earn substantially less in the labor market. They also have fewer children, presumably because they are less likely to be in, or have been in, stable relationships. Single motherhood is much more prevalent among teenage mothers

compared to older mothers, 14% compared to 2%. Among single mothers, 70% take up the single parents support, or transitional benefits as it is called.[7] Even though institutions vary considerably among countries, the traits associated with single parenthood in Norway are the same as elsewhere.[8] They find themselves in the lower end of the income distribution and are more likely to live in poverty. What distinguishes the Norwegian case from less comprehensive welfare states is that other social assistance schemes become available as transitional benefits are cut. Reiso (2014) shows that single mothers utilize these options. Thus, single mothers may respond somewhat different to welfare reforms in Norway compared to the US (where work is the primary alternative to welfare benefits), and hence the mechanisms for child outcomes are likely to differ. However, both in Norway and the US, some mothers are worse off by the welfare reforms and may experience large income losses (in Norway even after changing to other benefits). For this group, the mechanisms for child outcomes are likely to be similar.

There is a lot of heterogeneity in the population of single mothers. A very important sample split throughout the paper is the split by mothers' age. Appendix Table A.1 also shows pre-reform characteristics of younger and older single mothers (split by median age which is around 26). Noteworthy is the large difference in the reason for being a single mother. Older single mothers are much more likely to be single following a divorce. They are also better off in terms of education and earnings. Younger single mothers usually have one child, while the older single mothers have closer to two children. In addition, younger single mothers are more likely to live close to their own parents. The effects of the welfare reforms are therefore likely to hit these mothers differently as they have very different backgrounds.

B.   *The reforms to the transitional benefit program for single mothers in Norway*

The transitional benefit program secures income to single mothers, i.e. mothers who are sole caregivers for their children.[9] This welfare program has traditionally been relatively generous. Previously, single mother could receive benefits non-stop until their youngest child had finished 3rd grade of primary school (when the child was 9-10 years old). Also cohabiting single mothers, who were not married, could receive benefits as long as they were cohabiting with someone other than the father of their children. Two reforms with restrictive features were introduced in 1998 and 1999. Table 1 displays the main changes introduced by these reforms.

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[7]The data also tells us that 2% of mothers identified as married take up transitional benefits. This suggests that we have identified the single mothers group almost correctly.

[8]See for example comparisons across OECD countries in: http://ec.europa.eu/justice/gender-equality/files/documents/140502_gender_equality_workforce_ssr3_en.pdf.

[9]Single fathers may also be eligible for transitional benefits. The vast majority of single parents in Norway, however, are women. The uniqueness of being a single father suggests that this group is different from the group of single mothers. In fact, 14% of single parents were men in the 1990s, and their characteristics do differ from those of single mothers (Andersen, Birkeland, Epland and Kirkeberg, 2002). Thus, we focus solely on single mothers in this study.

Table 1. Features of the 1998 and 1999 reforms to the transitional benefit program

Characteristics	Before the reforms	After the reforms
1998 reform		
Time limit	None	Max 3 years of benefit receipt (may be taken non-consecutive)
Age limit	Youngest child finished 3rd grade of primary school (9-10 years old)	Youngest child less than 8 years old
Work requirement	None	Youngest child 3 years or older
Max benefit level	6 171 NOK(1998) per month	6 995 NOK(1998) per month
Means-tested in regard to assets	No	No
1999 reform		
Cohabitation status	Not eligible if children in common or married	Not eligible if lasted for at least 12 of the last 18 months

Notes: The time limit is related to the mother's youngest child and is reset to three years for every newborn child. Work requirements include working at least half time, taking education at least half time or being registered as unemployed at the government agency of Labour and Welfare Service. Benefits are reduced by 40% of excess earnings exceeding a level of 1 891 NOK(1998) per month. Also, benefits are reduced if the mother receives other types of benefits from The Norwegian income security system, like for instance sickness- or disability benefits. 100 NOK equals approx. 14 EUR and 17 USD.

The aim of the 1998 reform was to stimulate work and thereby reduce welfare dependency and lift income. A three year time limit was introduced, the age limit of the youngest child for eligibility was lowered, and work requirements for single mothers with youngest child aged three or older were implemented. On the other hand, benefit levels were increased to improve incomes for those still eligible. The reform was implemented over a three year period from the 1st of January 1998 to the 1st of January 2001.Within this implementation period, new applicants were awarded benefits according to the new rules, while mothers who were entitled to and had applied for benefits before the 1st of January 1998, could continue to receive benefits according to the old rules. A further restriction was imposed by the 1999 reform: single mothers in stable relationships with someone other than the father of their children were made ineligible. This reform was implemented for all the 1st of July 1999. In our data we cannot observe which single mothers were unaffected by the 1999 reform, and thus the 1999 reform is evaluated jointly with the 1998 reform. Note, however, that we do not aim at disentangle the effects of the different features of the 1998 reform either.

Birth year	Age of child										16	Treatment status
	1	2	3	4	5	6	7	8	9	10		
1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	2002	Untreated
1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	2003	Untreated
1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	2004	Untreated
1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2005	Partially
1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2006	Partially
1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2007	Partially
1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2008	Partially
1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2009	Fully
1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2010	Fully
1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2011	Fully

Figure 1. Treatment status by cohort and age of child

Notes: Non-shaded cells refer to years children (within the respective cohorts) are unaffected by the reforms to the transitional benefit program. Light-shaded cells refer to years children (within the respective cohorts) of some single mothers are potentially affected (the implementation period of the 1998 reform). Dark-shaded cells refer to years children of all single mothers (within the respective cohorts) are potentially affected.

Since our aim is to estimate the effect of these reforms on children's school grades, we need to know which children are affected. Figure 1 displays in what years a single mother is exposed to the reforms, depending on the birth year of her child. For instance, a single mother with a child aged two in 1993 is unaffected by the reforms during the years her child is 0-6 years old. These are pre-reform years (1991-1997). When her child is 7-10 she is potentially affected by the new rules. These are implementation and post-reform years (1998-2001). As Figure 1 shows, single mothers are increasingly exposed to the reforms the later their children are born. Single mothers with children aged two in 1988 are not affected at all, while single mothers with children aged two in 1997 are potentially affected by the reforms all years when the their children are 3-10 years old. In our analysis we exploit this variation across cohorts in single mothers' exposure to the reforms. Note that, given the three year implementation period of the 1998 reform, no cohorts are fully exposed to the reforms before the age of 6.

C. Other reforms

Cash subsidies to families with children aged one and two years old, who did not (or only partly) made use of publicly subsidized daycare centers were introduced in 1998 and 1999, respectively. These cash-for-care subsidies reduced mothers' labor market participation (Naz, 2004; Schøne, 2004; Drange and Rege, 2013). Even though cash-for-care could not be received for children in the cohorts of this study, these children may have been indirectly affected by having younger siblings. This is confirmed by Bettinger, Hægeland and Rege (2014) who find a small, but statistically significant, positive cash-for-care effect on grade point average the final year of junior high school among children with younger siblings eligible for cash-for-care. Note that cash-for-care did not target single mothers in particular. Heterogeneous cash-for-care responses in regard to education and earnings levels (Naz, 2004; Drange and Rege, 2013), however, indicate that its impact on single and married mothers may differ. Thus, to separate out the effect of the reforms to the transitional benefit program for single mothers from a potential cash-for-care effect, we exclude children with younger siblings in a robustness analysis. Reassuringly, the results from this exercise are similar to the main results.

In 1997 an educational reform that lowered mandatory school starting age from seven to six was implemented. Thus, children aged two in 1993-1997 have one more year of mandatory schooling compared to children aged two in 1988-1992. This additional year of schooling is, however, more comparable to a year of kindergarten than a year of formal schooling. Learning through play was essential.[10] According to Drange, Havnes and Sandsør (2012), 89% of all non-immigrant families had enrolled their six year old in a kindergarten program in the year prior to the change in school stating age, i.e. in 1996. Thus, for most children, the additional year of schooling is unlikely to present a significant change in educational attainment. Focusing on the group of children that is most likely to be affected (unlikely to be enrolled in kindergarten at age six), Drange et al. (2012), find no effect of the additional year of schooling on children's long-run educational performance. Most importantly, they show that this applies to a subsample of children of single parents as well. Thus, we are not worried that our findings are contaminated by this educational reform.

Another educational reform was implemented the school year 2007/2008, and may have affected children aged two in 1994-1997. The aim of the reform was to increase the overall quality in elementary school. A content-oriented curriculum was replaced with a goal-oriented one. Schools and teachers were given more autonomy and freedom. There was also an increased focus on the development of basic skills (defined as reading and writing, calculus, oral presentation skills, and computer skills). However, there were no changes to the main structures of elementary schooling, and in junior high school courses and the number of teaching hours per course remained mainly

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[10]See Drange, Havnes and Sandsør (2012) for further details.

unchanged (Bakken and Elstad, 2012, p. 31-32). The fact that the reform was not specifically targeted towards weak students, or especially vulnerable groups, makes it likely that children of single and married mothers were affected in the same manner. If not so, we would expect children of single mothers to be similarly affected by this reform in all sample splits. Finding significant estimates only in certain sub-samples (younger mothers) as we do, is not consistent with school reform effects.

III. Data

A. Data

We use data from Statistics Norway drawn from administrative registers, covering all Norwegian residents. Families are linked through personal identifiers. The data contains information on a variety of demographic and socioeconomic characteristics, in addition to employment and income records. Information on educational performance the final year of junior high school is available for the years 2002 to 2011, and we have information on welfare use from the income security system registers from 1992 to 2008.

The sample consists of children aged two in 1988-1997, for which we have measures of children's school grades in 2002-2011. It is split in two groups depending on the mother's pre-reform marital status in the beginning of the year their child turns two years old: a treatment group of children of single mothers and a comparison group of children of married mothers. Mothers are defined as single in the data if they are neither married nor cohabiting with a partner with whom they have children. Thus, mothers defined as single, may be cohabiting with someone other than the father of their children. In the data, unmarried mothers cohabiting with someone other than the father of their children are not distinguishable from mothers living alone. It follows that children of mothers with unclear material status the year their child turns two, and children not in the family registers by age two, are excluded.[11] In addition, the sample is restricted to children who turn 16 years old during their final year of junior high school, which is the norm. It is uncommon in Norway to repeat classes. The remaining sample of 534 977 children constitutes 88% of all individuals with registered grades the final year of junior high school for the years 2002-2011.

Our outcomes of educational performance are based on children's grade records the final year of junior high school. The main outcome is the overall grade point average (GPA) of 13 teacher-awarded grades in 13 different courses.[12] In addition, we use outcomes of average grades in

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[11]These could be children who had not yet moved to Norway by the age of two.

[12]These 13 courses are: written (two courses) and oral Norwegian, written and oral English, mathematics, nature and science, social science, religion, home economics, physical education, music, and arts and crafts. The educational reform of 2007/2008 introduced an additional grade in (foreign) language. For consistency, this grade is not included in the calculated GPA. In 2008, the GPA is based on the average of 12 grades since students were awarded only one grade

randomly drawn written and oral exams. The written exams are equal across the country, and are graded by external sensors. Also the oral exams are evaluated by an external sensor in addition to the teacher. The grading scale ranges from one to six, where one indicates inadequate competence and six excellence. In the analysis, we use standardized grades with mean of zero and standard deviation of one.

To control for possible compositional changes across cohorts, we include a number of child and mother characteristics measured the year the child turns two, i.e. prior to the reforms to the transitional benefit program for all cohorts. These controls are: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation.[13] The earnings measure includes labor earnings, in addition to welfare benefits such as unemployment benefits, sickness benefits and parental leave benefits. Earnings are measured yearly in fixed NOK 1998 prices. In line with the study by Mogstad and Pronzato (2012), a mother is defined as participating in the labor market if her earnings exceed one "basic amount" that year. Basic amounts are used by the Norwegian Social Insurance Scheme to determine the magnitude of and eligibility for a number of benefits as unemployment benefits and old age pension. In 1998, a basic amount was about 45 000 NOK, corresponding to about $7500. We do not exclude children with missing information on control variables. Rather we construct a dummy variable for missing that is included in the analysis.

To study mechanisms, we analyze mothers responses to the reforms using a variety of outcomes measured in the years when the child is 6-10: Number of years receiving transitional benefits, number of years working (yearly earnings exceed one basic amount), number of years working full-time (yearly earnings exceed four basic amounts), number of years with ongoing education, average yearly earnings, average yearly transitional benefit payments, and average yearly income.[14] The income measure includes earnings, transitional benefit payments, in addition to other welfare benefits being disability benefits and social assistance payments. Payments from these other welfare benefits are important to capture as one response of the mothers to the reforms, apart from working more in the labor market, is to switch to other benefits (Reiso, 2014).

in written Norwegian that year. For students with less than 13 grades (immigrants may be exempted certain courses), the average GPA is calculated based on the attained number of grades.

[13] The 46 regional labor markets are defined according to commuting distances statistics (Bhuller, 2009).

[14] By the 1st of January 2002, and the 1st of January 2004, medical and work-related rehabilitation benefits and time limited disability benefits are included in the earnings measure, respectively. For consistency, when measuring mothers' outcomes, medical- and work-related rehabilitation payments are included in the earnings measure for the years 1992-2001.

IV. Identification strategy

A. Identification strategy

To estimate the effect of the reforms on children's school grades, we exploit the variation in exposure across cohorts. As Figure 1 depicts, children aged two in 1988-1990 are not exposed to the reforms. These children are referred to as "untreated". Children aged two in 1991-1994 are, as they are getting older, increasingly exposed to the reforms, and children aged two in 1995-1997 are exposed throughout childhood. These children are referred to as "partially treated" and "fully treated", respectively. Of main interest is the comparison of school grades of the fully treated children relative to those of the untreated children. In addition, we also compare school grades of the partially treated children to those of the untreated children. To avoid confounding the effects of the reforms with unrelated cohort effects, we use children of married mothers as a comparisons group. Formally, this difference-in difference (DinD) model may be expressed as:

$$y_{ic} = \alpha_1 + \alpha_2 Single_i + \gamma(Part_c \times Single_i) + \mu(Full_c \times Single_i) + \lambda_c + X_{ic}\theta + \varepsilon_{ic} \qquad (1)$$

where y_{ic} is the outcome of child i in birth cohort c. *Single* is a binary variable taking the value 1 if the child's mother is single, and 0 if the child's mother is married. *Part* and *Full* are binary variables taking the value 1 if the child is aged two in 1991-1994 or 1995-1997, respectively, and 0 otherwise. λ_c is a vector of year fixed effects. X_{ic} is a vector of child and mother's pre-reform characteristics; child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. ε_{ic} is the error term. The coefficient of interest is μ, which measures the difference in mean outcome between fully treated and untreated children of single mothers – relative to those of married mothers. Correspondingly, γ captures the difference in mean outcome between partially treated and untreated children of single mothers – relative to those of married mothers. We measure the intention to treat (ITT) effect of the reforms. Unfortunately, we do not have take-up rates of transitional benefits for all cohorts when the child is aged two (these data starts in 1992, thus 1990 is the first cohort for which we have this information). However, when we look at take-up rates for later cohorts (child aged two in 1992-1997), it is as high as 70% for the total sample and 80% for the sample of younger single mothers. This means that most of our sample is affected by the reforms, and hence ITT should not be far from the average treatment effect (ATE). The reforms are affecting a whole range of outcomes of the single mothers so we will only be able to estimate the reduced form effect of the reforms. However, with our rich data, we will explore likely mechanisms.

B. Threats to identification

The underlying assumption for this DinD model to produce consistent estimates is that, in the absence of the reforms, the average outcome of children of single mothers would have changed in the same way across cohorts as the average outcome of children of married mothers. This assumption is commonly referred to as the common trend assumption. As we will show in Section V.B., the common trend assumption holds for the untreated cohorts. The difference in children's school grade measures of single and married mothers are constant in the pre-reform period (see Figure 5). In addition, the characteristics of single and married mothers across cohorts should not have patterns similar to the reforms (for instance, single mothers' earnings should not start to increase for treated cohorts relative to untreated cohorts). If so, the reform effects may be confounded with changes in these underlying characteristics. This is the main challenge of the paper as single and married mothers (although defined pre-reform and at the same age of the child) do not follow the same trends in education, labor market earnings and age over time. Single mothers are lagging somewhat behind on educational attainment and earnings compared to married mothers. They are also becoming relatively older across our sample cohorts. Figures 2-4 illustrate these patterns. Figure 2 shows the average level of education of the mothers when the child is aged two. The upper two figures are the raw total sample, and the lower two figures are the sample after a matched control group of married mothers to the whole sample of single mothers is constructed. In the figures to the left, the solid line is for single mothers and the dashed line is for married mothers. In the figures to the right we take the difference between the two groups with a 95% confidence interval. Married mothers have one more year of educational attainment compared to single mothers. The level of education is increasing across cohorts, however, single mothers are slightly lagging behind married mothers. After matching, the groups are much more similar and the differences over time are smaller, although we are not able to eliminate the differences completely. In Figure 3 showing mothers labor earnings, the matching is more successful. Here we see a large divergence between single and married mothers over time. Notice that this difference is almost linear; there is no tendency of a reform pattern, i.e. no effect for the untreated cohorts compared to treated cohorts (indicated by vertical lines). After matching, the groups are very similar. Finally, when we study mothers age in Figure 4, we see that single mothers become slightly older compared to married mothers across cohorts. Matching is not fully taking care of this, however, we are closer to similar samples. Our strategies to deal with the single and married mothers not having the same pre-reform characteristics over time is firstly to control for as many observable characteristics as possible. We will show later that after controlling for education, earnings and age, no other controls (child's gender, number of siblings, mother's labor market participation, non-Norwegian country of birth, regional labor market of residence) matter. Our modified underlying assumption is then that after controlling for observable characteristics, the average outcomes of children of single mothers

would have changed in the same way across cohorts as the average outcome of children of married mothers, in the absence of the reforms. A second strategy is to use matching as shown in the Figures 2-4. We use a one-to-one nearest-neighbor propensity score matching method to create a control group of married mothers that is as similar as possible to out treatment group of single mothers. The propensity scores are estimated using a logistic regression of mothers' single status on all controls used in the main analysis. Then, the estimated propensity score of each single mother is matched with the nearest propensity score of a married mother. This gives a matched control group of one married mother per single mother. Our last strategy is to dig into subgroups that we believe will respond differently to the reforms (while we would not expect to find different effects if the results were driven by changes in underlying characteristics).
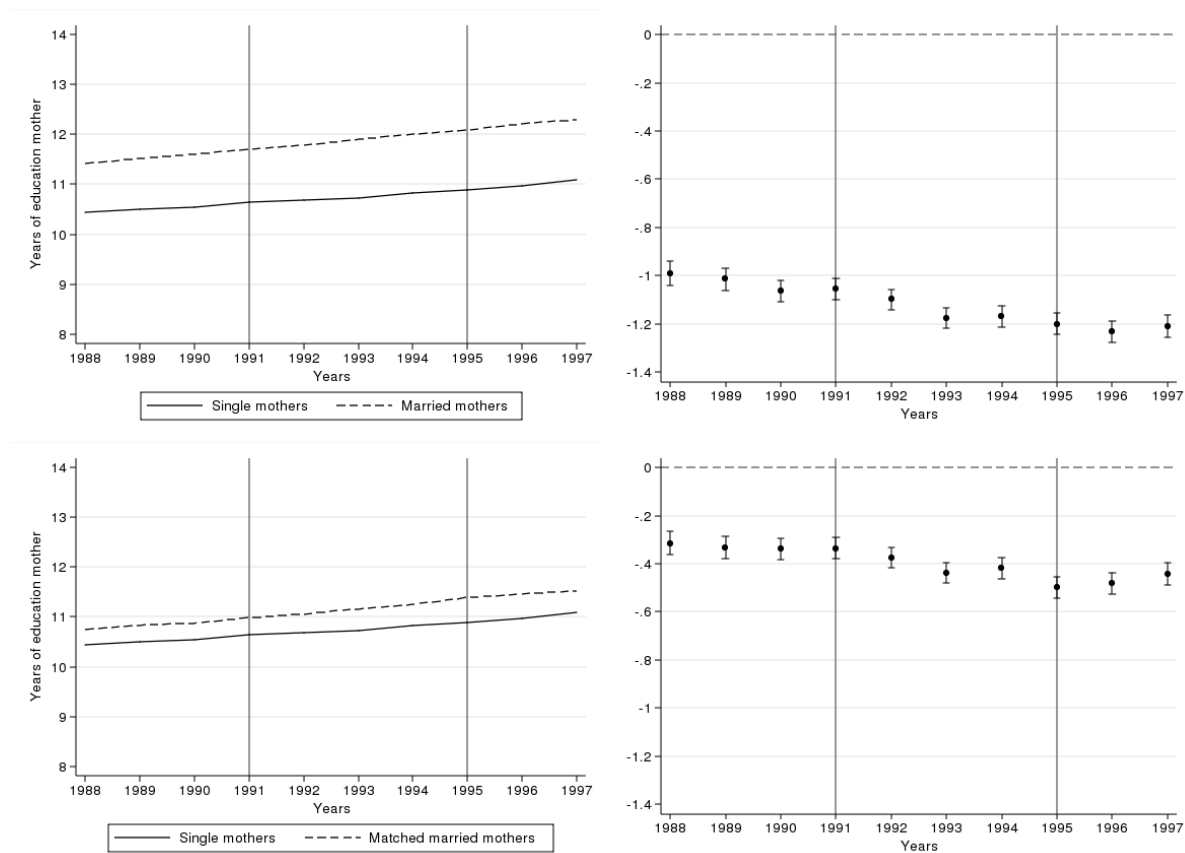
Figure 2. Pre-reform characteristics of the mothers: years of education

Notes: The two left figures show the average years of education for single (solid line) versus married (dashed line) mothers in the raw total sample (upper figure) and the matched sample (lower figure). The two right figures show the corresponding differences between single mothers and married mothers with a 95% confidence interval. The vertical lines indicate untreated, partially treated and fully treated cohorts of children.
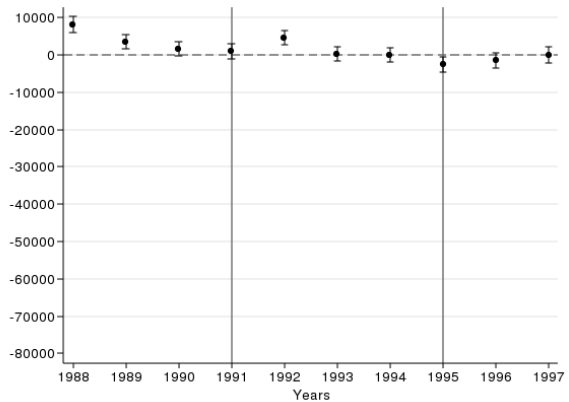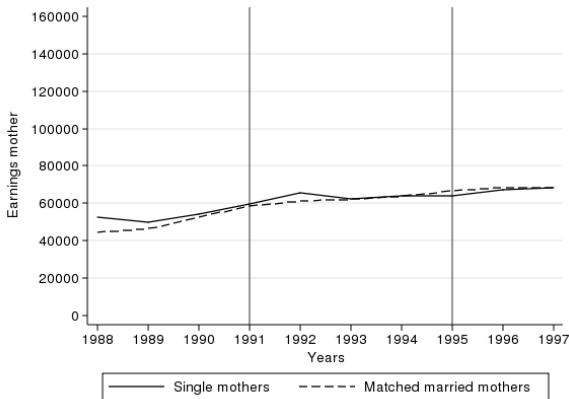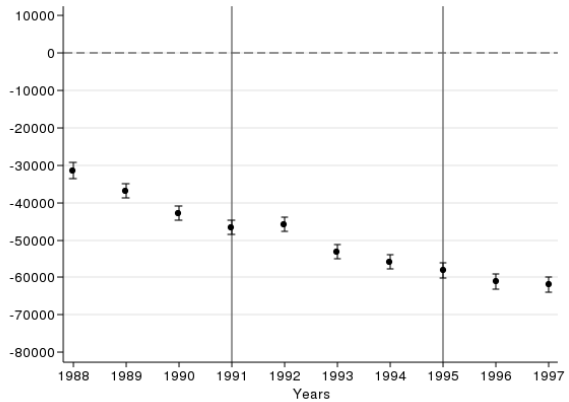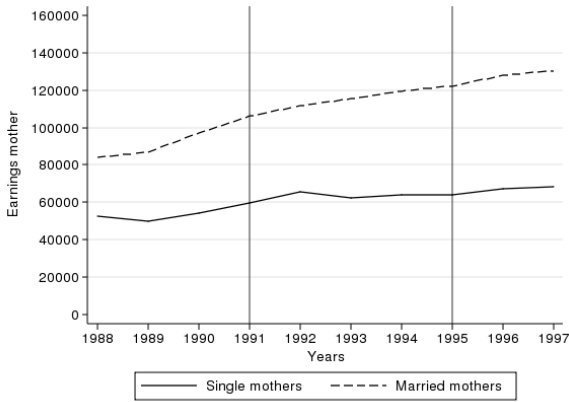
Figure 3. Pre-reform characteristics of the mothers: earnings

Notes: The two left figures show the average yearly earnings of single (solid line) versus married (dashed line) mothers in the raw total sample (upper figure) and the matched sample (lower figure). The two right figures show the corresponding differences between single mothers and married mothers with a 95% confidence interval. The vertical lines indicate untreated, partially treated and fully treated cohorts of children.
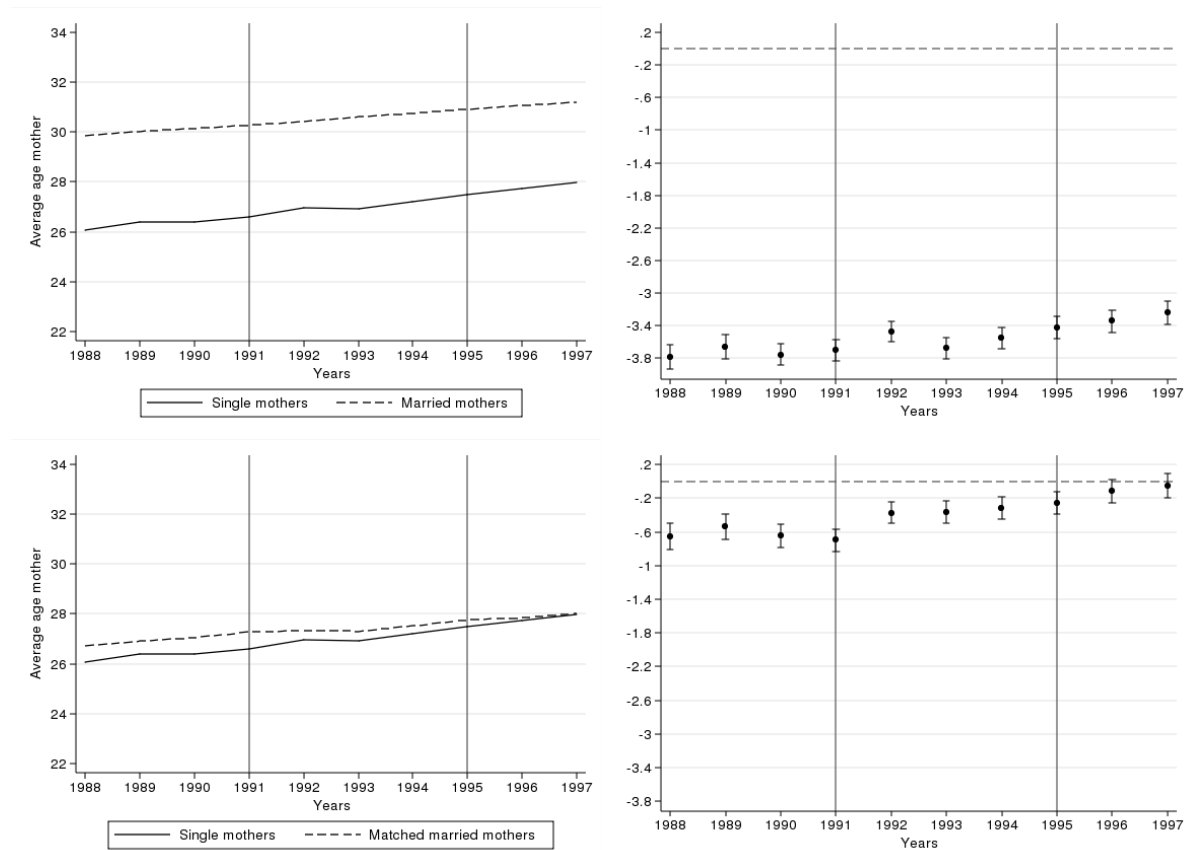
Figure 4. Pre-reform characteristics of the mothers: age

Notes: The two left figures show the average age of single (solid line) versus married (dashed line) mothers in the raw total sample (upper figure) and the matched sample (lower figure). The two right figures show the corresponding differences between single mothers and married mothers with a 95% confidence interval. The vertical lines indicate untreated, partially treated and fully treated cohorts of children.

V. Results

A. *Regression results*

We will first show regression-based estimates. The DinD model is estimated using a linear probability model. Table 2 presents the baseline DinD estimates of the reforms on children's school grades in junior high school for the total sample of single and married mothers. The following control variables are included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. These are all measured when the child is aged two (pre-reform).

Column 2 of Table 2 displays estimates of the intention to treat (ITT) effects of the reforms on the partially treated cohorts, and column 3 displays estimates of the ITT effects of the reforms on the fully treated cohorts (both compared to the untreated). Column 1 shows the average of the

Table 2. Difference-in-difference estimates of the reforms on children's school grades

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
GPA	3.60	-0.004	-0.010	534,977
		(0.010)	(0.011)	[60,782]
Written exam	3.09	0.027**	0.001	512,791
		(0.010)	(0.011)	[56,766]
Oral exam	3.88	-0.016	-0.014	473,243
		(0.011)	(0.012)	[52,029]

Notes: Partially refers to children aged two in 1991-1994, and Fully refers to children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.

outcome variables for the untreated (pre-reform) cohorts of children of single mothers. Finally the last column gives the number of observations in total and in brackets the number of single mothers. For the three outcomes, grade point average (GPA), written exam, and oral exam, we see no effect of the reforms on the fully treated cohorts. The estimates are close to zero and insignificant. The same holds for GPA and oral exam for the partially treated cohorts, while there is a small positive effect on written exam. Studying the figure for this outcome (not shown), it turns out that this is driven by a relatively high average written exam score for the cohort of children of single mothers aged two in 1992. We do not consider this to be related to the reforms (excluding this cohort from the analysis produces an insignificant estimate).

In Table 3 the sample is split by mothers' age (median age of single mothers). In panel A, we see negative effects for the fully treated cohorts on all three school grade outcomes. We also see that the effects are about half for the partially treated cohorts (only present for GPA and oral exam). The effects for the fully treated cohorts are about 7% of a standard deviation.[15] In Section VI.E., after having studied mechanisms in more detail, we will compare this estimate to other studies focusing on work-encouraging/discouraging welfare reforms.

B. Graphical results

We now turn to graphical analysis of the results in Section V.A. As there is no effect for the full sample nor the sample of older mothers, we will only show figures for the sample of younger single mothers. If we are to believe the negative estimates, we need to convince that the common trend assumption holds. Figure 5 shows the average outcomes for GPA, written exam and oral exam,

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[15]We have studied whether the effects vary by gender and find very similar effects for boys and girls.

Table 3. Difference-in-difference estimates of the reforms on children's school grades by mothers' age

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
A: Younger mothers				
GPA	3.51	-0.031**	-0.069***	131,423
		(0.015)	(0.016)	[32,607]
Written exam	2.98	0.007	-0.052***	125,050
		(0.015)	(0.016)	[30,441]
Oral exam	3.77	-0.032*	-0.066***	115,704
		(0.017)	(0.018)	[27,998]
B: Older mothers				
GPA	3.71	-0.006	0.000	403,554
		(0.015)	(0.016)	[28,175]
Written exam	3.20	0.020	0.018	387,741
		(0.015)	(0.017)	[26,325]
Oral exam	4.02	-0.022	-0.002	357,539
		(0.016)	(0.018)	[24,031]

Notes: The sample is split such that the younger mothers are aged equal to- or below the median age of single mothers in each respective cohort. Partially refers to children aged two in 1991-1994, and Fully refers to children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.

respectively. The three upper graphs show the average outcomes for children of single mothers (solid line) and children of married mothers (dashed line). We see that children of single mothers in general perform worse on all school grade outcomes than children of married mothers. There is a slight upward trend in school grades across cohorts, however after the reforms, children of single mothers are starting to lag further behind children of married mothers for all three outcomes. The lower three figures better illustrate the common trend assumption and the differences between children of single and married mothers. For the untreated (pre-reform) cohorts, children of single mothers score around 20% of a standard deviation lower on GPA than children of married mothers. The common trend assumption looks good: it is stable for the untreated cohorts. Then there is a divergence for the partially treated cohorts, and for the fully treated cohorts the difference is around 30% of a standard deviation. For written exam, the common trend assumption looks even better and we see a very similar pattern as for GPA. The figure for oral exam is not as convincing, however, it shows a similar pattern as for GPA and written exam.
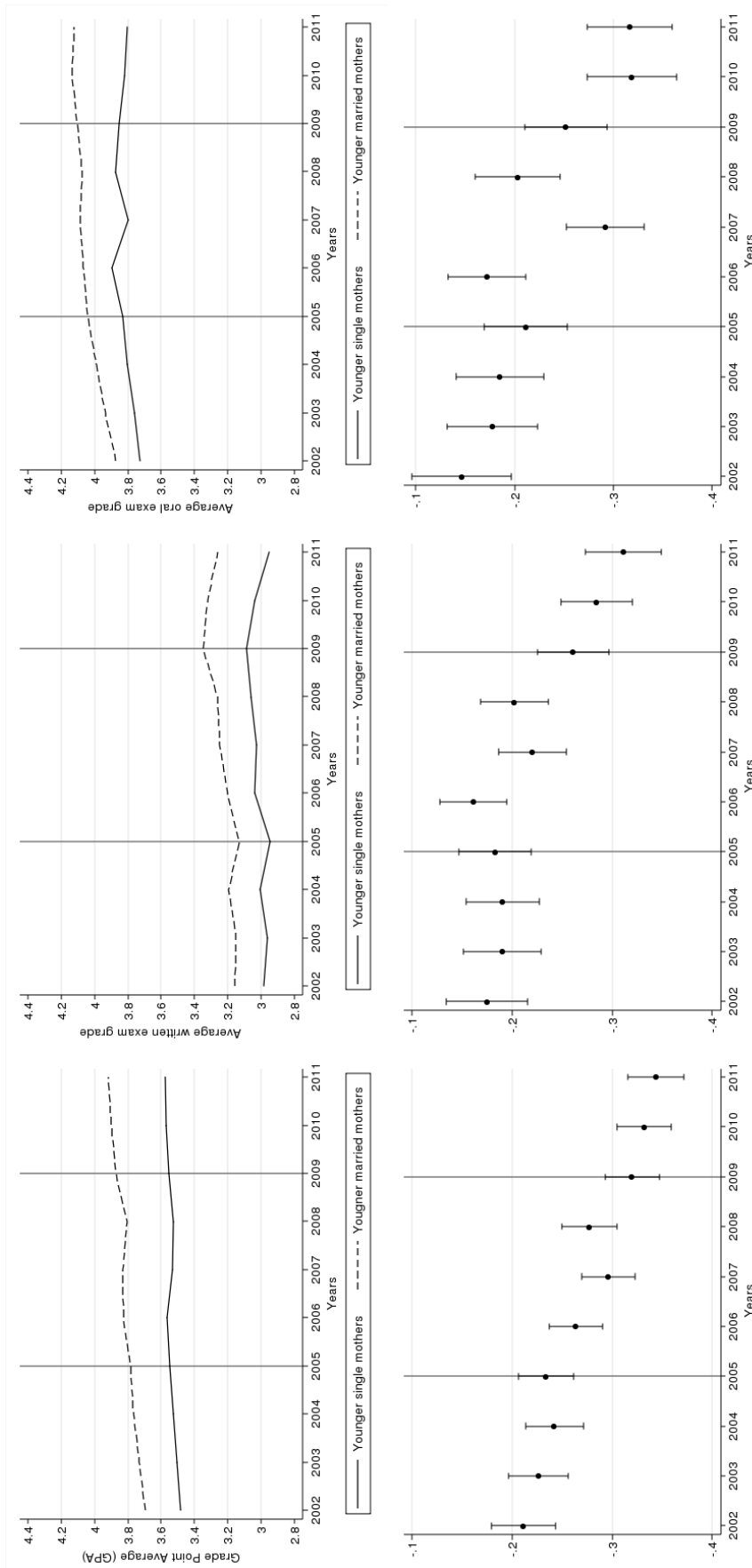
Figure 5. Children's school grades

Notes: The upper three figures show the average outcomes (grade point average, written exam and oral exam, respectively) for children of younger single mothers (solid line) versus children of younger married mothers (dashed line). The lower three figures show the corresponding differences between children of younger single mothers and younger married mothers with a 95% confidence interval. We have kept the y-axis similar for all outcomes to highlight that the average outcomes vary across different types of school grade measures. The vertical lines indicate untreated, partially treated and fully treated cohorts of children.

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C. Robustness checks

In this section we probe the stability of our baseline estimates to alternative specifications. We conclude that our estimated negative effects on children's school grades in the group of younger single mothers are remarkably robust to a large number of alternative specification checks.

In Table A.2, we present estimates for children's school grades after we have matched a group of similar married mothers to the group of single mothers (one-to-one matching). See Section IV.B. for details. We see that the main estimates of ITT for the fully treated are very similar to the baseline in Table 2. If anything, they are slightly more negative. This is very reassuring given that we saw the levels and trends of the most important background variables for single and matched married mothers are almost identical.

The next exercise is to look more closely at control variables. In Table A.3 we first show the baseline estimates from Table 2. Then we compare this to specifications with no control variables, and only education, earnings, and age as controls. The specification without any control variables produces substantially larger estimates than the baseline estimates. This suggests that if we do not control for pre-reform characteristics we overestimate the effect of the reforms. However, after controlling for education, earnings and age, the estimates are very similar to the baseline estimates. Adding additional controls do not move the estimates much.

We perform a variety of additional robustness checks. First we estimate the effect for a subsample where the child does not have younger siblings at age 16. This is in order to show that our effect is not driven by the cash-for-care subsidy (see Section II.C.). We see from Table A.4, panel A, that our results hold up for this subsample. Next we drop teenage mothers from the sample of younger mothers. The reason is that teenage mothers may differ from older mothers along multiple dimensions (they are for instance more likely to live with their parents), and we want to show that our results are not driven by this particular group of single mothers. We see from panel B that the results excluding teenage mothers are very similar to our baseline estimates. In panel C, we exclude non-Norwegian born mothers. The results hold up, albeit a little weaker, suggesting that some of the effect is also present for children of foreign born mothers. Unfortunately, we do not have the necessary sample size to dig further into the group of foreign born mothers. Finally, we check whether our results for younger mothers are driven by the way we split the sample; by the median age of single mothers in each respective cohort (when the child is aged two). Panel D confirms that this is not the case. If we split by age 26 instead, we get very similar results [16]

We can also study treatment by year of birth. Table A.5 shows a pattern consistent with the reforms. There are no effects for the first two cohorts relative to the cohort aged two in 1988. The effects are very close to zero and, if anything, slightly positive. For the next four cohorts (partially

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[16]Also, splitting the sample by ages 25 and 27, give negative, significant effects for child outcomes.

treated) we also see few significant effects compared to the cohort aged two in 1988. However, many of the coefficients have started to turn negative. Finally for the last three cohorts, fully affected by the reforms, all coefficients are negative and 6 of 9 are statistically significant. It is important to note that this model is expected to have higher standard errors than a three-split DinD model.

Another alternative to the three-split DinD is to use a model with a linear treatment variable which is 0 for the untreated cohorts, 1 for the first cohort (partially) effected, 2 for the second, and so on. As we saw in Figure 5, this seems like a valid setup as the differences gradually increase over time after the reforms. Table A.6 shows that the effect is around 1% of a standard deviation for each additional year of treatment for all the three school grade outcomes.

As a final check, we run a placebo test. Mothers that earn more than about 195 000 NOK (in 1998 prices) pre-reform are not affected by the reforms as they are not eligible for transitional benefits. In the sample of younger single mothers there are too few mothers who earns above this threshold to perform a placebo test, however, we can run a placebo for the total sample of single mothers earning above the threshold. This is still useful since if there are other reasons (apart from the reforms) why we find diverging trends in outcomes between children of single and married mothers, they are likely to show up also for this sample. Finding no effects for this sample is therefore reassuring. Indeed Table A.7 shows that there is no effects of the reforms in the placebo sample on any of the children's school grade outcomes.

VI. Mechanisms

A. Mothers' responses

To understand why we find negative effects of the reforms on a subgroup of single mothers (younger), we need to analyze how the mothers responded to the reforms. An important aspect of this is the underlying background characteristics. Although both younger and older mothers might respond to the reforms by working more, it could be different to work more from a basis of not working than from already being attached to the labor market. We look at the following outcomes for the mother: How many years she received transitional benefits when the child was aged 6-10 (maximum 5 years). How many years she was in the labor force when the child was aged 6-10, and how many years she worked full-time. Years of ongoing education when the child was aged 6-10, average yearly earnings when the child was aged 6-10, and average yearly payments of transitional benefits when the child was aged 6-10. Finally, we measure average yearly income when the child was aged 6-10. The results are shown in Table 4, for younger mothers (panel A) and older mothers (panel B), separately. First, we focus on the younger mothers and the effects for the fully treated cohorts (column 3). We see that single mothers in the untreated cohorts received, on average, transitional benefits for 2.4 years (out of 5 years) when their child was aged 6-10 (column 1). After the reforms,

for the treated cohorts, this dropped by almost an entire year. The single mothers responded to this by increasing their labor market participation, both on the extensive and intensive margin. They worked an additional .25 years from a base of 3.12 years and were more likely to work full time (.19 years from a base of only 1.7 years out of 5 years). There was no response on years of ongoing education for the fully treated cohorts. Turning to earnings and benefits, we see an increase in earnings and a decrease in transitional benefit payments. Finally, we see that there was no effect on income. This means that single mothers were, on average, only able to work exactly enough to offset the loss in benefits, holding income constant. This turns out to be very useful when discussing mechanisms as there was no income effect for the family. We can therefore focus on the time effect - how does it affect children to have mothers more away from home as they work more? In panel B we present the results for older mothers. Basically, the estimates are very similar to the ones for younger mothers. Note, however, that the baseline pre-reform averages are very different. This means that although the families of older single mothers also experienced changes because of the reforms, the changes happened at different margins than for the families of younger mothers. This could be the reason why we do not see negative reform effects for the children of older mothers.[17]

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[17]For example, having a mother more away from home if she is already working could be very different from having a mother more away from home with being at home as a starting point.

Table 4. Difference-in-difference estimates of the reforms on mothers' outcomes by mothers' age

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
A: Younger mothers				
Years of trans.benefit take-up	2.41	-0.205***	-0.890***	13,1423
when child is 6-10 (max:5)		(0.030)	(0.029)	[32,607]
Years of work	3.12	0.134***	0.255***	131,423
when child is 6-10 (max:5)		(0.028)	(0.029)	[32,607]
Years of full-time work	1.70	0.112***	0.189***	131,423
when child is 6-10 (max:5)		(0.028)	(0.030)	[32,607]
Years of ongoing education	0.58	0.045***	-0.018	131,423
when child is 6-10 (max:5)		(0.017)	(0.018)	[32,607]
Average yearly earnings	94925	3629***	6012***	131,423
when child is 6-10		(1184)	(1314)	[32,607]
Average yearly trans. benefit payments	21907	-1186***	-7619***	131,423
when child is 6-10		(331.449)	(322)	[32,607]
Average income	121244	2494**	-123	131,423
when child is 6-10		(1085)	(1207)	[32,607]
B: Older mothers				
Years of trans.benefit take-up	1.97	-0.249***	-1.095***	403,554
when child is 6-10 (max:5)		(0.030)	(0.028)	[28,175]
Years of work	3.57	0.071***	0.251***	403,554
when child is 6-10 (max:5)		(0.024)	(0.026)	[28,175]
Years of full-time work	2.46	0.063**	0.191***	403,554
when child is 6-10 (max:5)		(0.027)	(0.029)	[28,175]
Years of ongoing education	0.42	-0.005	-0.039**	403,554
when child is 6-10 (max:5)		(0.015)	(0.016)	[28,175]
Average yearly earnings	134748	3850***	7209***	403,554
when child is 6-10		(1240)	(1439)	[28,175]
Average yearly trans. benefit payments	17100	-1441***	-9197***	403,554
when child is 6-10		(318)	(296)	[28,175]
Average income	156311	2484**	-593	403,554
when child is 6-10		(1151)	(1350)	[28,175]

*Notes: The sample is split such that the younger mothers are aged equal to- or below the median age of single mothers in each respective cohort. Partially refers to mothers of children aged two in 1991-1994, and Fully refers to mothers of children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.*

B.   Pre-reform attachment to the labor market

To dig further into mechanisms, we split the sample of younger mothers in two groups by their pre-reform work status (when child is aged two). In panel A, of Table 5, we present the results for children of mothers not working pre-reform, and in panel B, we present the results for children of mothers working pre-reform. Interestingly, we see negative effects of the reforms for both groups, although the effects for children having a mother working pre-reform are almost double the size. The mechanisms for these two groups are likely to be quite different. This we see in Table 6 where we present mothers' outcomes for these two groups. In panel A, we see no response in the labor market for mothers not working pre-reform. However, they received less transitional benefits, and therefore they experienced a large drop in income. A likely mechanism for the negative effect we observe for the children of non-working mothers is therefore the reduction in family income that they experienced. In panel B, we have a very different picture. Here the mothers were already working pre-reform, and they increased their work amount in response to the reforms. Income increased somewhat (although not significant). The large negative reform effect for the children of these mothers is not consistent with an increase in income. Thus, a likely mechanism here is that these mothers were more away from home after the reforms. Also, the finding that these mothers were more likely to work full-time after the reforms supports the idea that these mothers were less present at home when their children came home from school. Consistent with this, we find stronger negative reform effects on children of younger mothers who worked in sectors where shift-work is overrepresented (the health sector and the hotel industry) compared to other industries (not shown in table). To be able to increase their work load in these shift-work industries, the mothers may have had to work more afternoons and irregular hours after the reforms, causing them to have even less after-school time with their children.

Table 5. **Effects by mothers' pre-reform work status -children's school grades, younger single mothers**

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
A: Non-work (child 2)				
GPA	3.45	-0.024	-0.073***	57,673
		(0.020)	(0.021)	[22,137]
Written exam	2.93	0.008	-0.056**	54,216
		(0.020)	(0.022)	[20,519]
Oral exam	3.71	-0.032	-0.095***	49,933
		(0.022)	(0.024)	[18,834]
B: Work (child 2)				
GPA	3.66	-0.090***	-0.129***	73,750
		(0.025)	(0.026)	[10,470]
Written exam	3.13	-0.053**	-0.134***	70,834
		(0.026)	(0.027)	[9,922]
Oral exam	3.91	-0.062**	-0.095***	65,771
		(0.028)	(0.030)	[9,164]

*Notes: The sample is split according to the mothers' labor market participation pre-reform (the year the child turns two). Partially refers to children aged two in 1991-1994, and Fully refers to children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.*

Table 6. Effects by mothers' pre-reform work status -mother outcomes, younger single mothers

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
A: Non-work (child 2)				
Years of trans.benefit take-up	2.65	-0.189***	-0.909***	57,673
when child is 6-10 (max:5)		(0.039)	(0.038)	[22,137]
Years of work	2.73	0.055	0.071*	57,673
when child is 6-10 (max:5)		(0.039)	(0.041)	[22,137]
Years of full-time work	1.30	-0.039	-0.054	57,673
when child is 6-10 (max:5)		(0.035)	(0.038)	[22,137]
Years of ongoing education	0.61	0.057**	-0.010	57,673
when child is 6-10 (max:5)		(0.022)	(0.024)	[22,137]
Average yearly earnings	76854	-518	-1528	57,673
when child is 6-10		(1489)	(1711)	[22,137]
Average yearly trans. benefit payments	25297	-796*	-7770***	57,673
when child is 6-10		(447)	(438)	[22,137]
Average income	107476	-1069	-7071***	57,673
when child is 6-10		(1375)	(1579)	[22,137]
B: Work (child 2)				
Years of trans.benefit take-up	1.74	-0.049	-0.554***	73,750
when child is 6-10 (max:5)		(0.052)	(0.049)	[10,470]
Years of work	4.19	0.043	0.106***	73,750
when child is 6-10 (max:5)		(0.038)	(0.038)	[10,470]
Years of full-time work	2.78	0.074	0.176***	73,750
when child is 6-10 (max:5)		(0.052)	(0.054)	[10,470]
Years of ongoing education	0.48	0.031	-0.014	73,750
when child is 6-10 (max:5)		(0.028)	(0.029)	[10,470]
Average yearly earnings	144147	-31	5032**	73,750
when child is 6-10		(2309)	(2445)	[10,470]
Average yearly trans. benefit payments	12672	507	-3383***	73,750
when child is 6-10		(482)	(463)	[10,470]
Average income	158747	516	2044	73,750
when child is 6-10		(2153)	(2295)	[10,470]

*Notes: The sample is split according to the mothers' labor market participation pre-reform (the year the child turns two). Partially refers to mothers of children aged two in 1991-1994, and Fully refers to mothers of children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.*

C. *More evidence that time matters*

If time is the mechanism for the strong negative effects we find for children's school grades, access to networks that can help mothers to take care of children after school might be very important. In table 7 we see a much stronger effect if there is no grandparents living close (on both mother's and father's side). We find similar effects if we only condition on having grandparents on mother's side living close.

Table 7. Effects by access to network (grandparents) -younger single mothers

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
At least one grandparent live close (both father's and mother's side)				
GPA	3.49	-0.038**	-0.052***	106,035
		(0.016)	(0.017)	[27,298]
Written exam	2.97	0.004	-0.034*	100,894
		(0.017)	(0.018)	[25,490]
Oral exam	3.75	-0.035*	-0.051***	93,352
		(0.018)	(0.020)	[23,424]
No grandparents live close (both father's and mother's side)				
GPA	3.57	0.006	-0.135***	25,388
		(0.038)	(0.039)	[5,309]
Written exam	3.08	0.014	-0.132***	24,156
		(0.039)	(0.041)	[4,951]
Oral exam	3.85	-0.018	-0.129***	22,352
		(0.042)	(0.044)	[4,574]

*Notes: The sample is split into mothers who live in the same municipality as at least one of the grandparents of their child (both mother's and father's side) when child is two, and mothers who do NOT live in the same municipality as at leat one of the grandparents of their child (both mother's and father's side) when child is two. Partially refers to children aged two in 1991-1994, and Fully refers to children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.*

One source to which mothers are important in the after-school time is through help with homework. In Table 8 we see stronger effects at the top of the grade distribution. This is consistent with mothers having less time and energy for after school care as it is less likely that lower ability students will get parental help with homework anyway (Hill and Tayler, 2004).

Table 8. Effects on educational distribution -younger single mothers

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
GPA above 2	0.97	0.003	0.001	131,423
		(0.003)	(0.003)	[32,607]
GPA above 3	0.70	-0.016**	-0.016**	131,423
		(0.007)	(0.007)	[32,607]
GPA above 4	0.27	-0.006	-0.026***	131,423
		(0.007)	(0.008)	[32,607]
GPA above 5	0.02	-0.007***	-0.019***	131,423
		(0.003)	(0.003)	[32,607]

Notes: Partially refers to children aged two in 1991-1994, and Fully refers to children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.

D. Other potential mechanisms

We define treatment (single) and control group (married) when the child is aged two, while we study mothers' outcomes when the child is aged 6-10. This means that mothers could have changed single status as a response to the reforms by the time we measure their outcomes. Another response to the reforms could be to have more or fewer children. By having another child, single mothers may remain eligible for transtional benefits also after the reforms. From Table 9 we see that there is no effect of the reforms on changing single status. Our estimates are therefore not driven by single mothers being more likely to remarry. However, there is some evidence that the reforms affect fertility. If having more siblings have a negative effect on the child's school grades this could be part of the mechanism. However, Black, Devereux and Salvanes (2005) do not find a relationship between family size and educational attainment. Also, remember that in the robustness test using a sample without younger siblings, we still find negative reform effects (although conditioning on number of younger siblings might be endogenous to the reforms so we have to be careful).

Table 9. Effects on single status and fertility -younger single mothers

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
Single when child is 10	0.50	-0.010 (0.008)	-0.010 (0.008)	127,232 [31,087]
No. children when child is 10	2.13	0.026** (0.013)	0.047*** (0.013)	131,423 [32,607]

Notes: Mothers with uncertain status (neither single nor married) when child is aged 10 are excluded in the single status analysis. Partially refers to mothers of children aged two in 1991-1994, and Fully refers to mohters of children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.

E.   Comparisons to other studies

Are the negative effects we find for children's school grades large or small? To look into this, we compare our estimates with other studies that have also looked at time vs income mechanisms. Dahl and Lochner (2012) study the effect of increased income on children's test scores using EICT (Earned Income Tax Credit) reforms in the US. They find that a $1000 increase in income increases math and reading test scores by 6% of a standard deviation. For the group of younger mothers who are not working pre-reform, we also interpret the mechanism for negative reform effects as an income effect and our estimate is very similar to Dahl and Lochner (2012): A 7700 NOK (approx. $1300) decrease in income corresponds to a 7% of a standard deviation reduction in school grades. Bettinger, Hægeland and Rege (2014) looks at the effects on school grades of having mothers more at home when the children are around 10 years old, using a reform in Norway (cash-for-care) giving mothers incentives to stay more at home with younger children (indirectly affecting older siblings). They find that a 3% points drop in labor force participation of mothers when the child is 10, increases GPA by 3% of a standard deviation. Our estimate for the group of children where time is a likely mechanism (mothers working pre-reform) is around 10% of a standard deviation. For this group, we see an increase in mothers working full-time when the children are aged 6-10 corresponding to an estimate of about 2-3% points. Thus, our school grade estimates are in the same range, though somewhat larger compared to their findings. However our measure of working full time is not the same.

How to relate the positive findings in Miller and Zhang (2009, 2012) to our finding of negative effects for a subgroup of younger mothers? They find an overall effect around 5-10% of a standard deviation in math test scores in fourth grade. They cannot link their data to mothers' responses and thereby dig into mechanisms - it is therefore hard to directly compare their studies to our study.

However, there are other studies from the US suggesting that the US reform was more successful than the Norwegian reform in increasing income and getting single mothers out of poverty. As we do not find positive income effects of the Norwegian reform in our total sample, finding no effects for the children in the total sample is consistent with the positive findings for children in the US if income is a likely mechanism. Our result of negative effects for children of mothers who were worse off by the reforms is therefore a likely scenario also for subgroups facing negative income effects in the US.

VII. Conclusion

Investigating the effects of a work-encouraging welfare reform targeted at single mothers in Norway, we find that, for the majority, the educational performance of single mothers' children were unaffected. However, children of younger single mothers compared to children of younger married mothers perform relatively worse in junior high school after the reform. Using the rich administration data available, we disentangle the likely mechanisms through which this negative effect may work. For children of younger single mothers working pre-reform, the likely mechanism is that mothers are more away from home as they now increase their time in the labor market. This implies that the alternative care for these children (after-school care, unsupervised time at home or informal networks) is not a perfect substitute for mothers' time. For children of younger single mother with low attachment to the labor market pre-reform, the likely mechanism is a reduction in income as these mothers are not able to work enough to offset a big drop in welfare benefits.

Taken together, our results have important implications for the full evaluation of welfare programs targeted at single mothers. Policy makers should take into account the potential negative effects on child development. Encouraging single mothers to work could still be a positive policy, however, policy makers then need to ensure that there are good substitutes to maternal time at home. An interesting avenue for future research is whether work-encouraging welfare reforms effect mothers health. For instance, difficulties combining work with having the sole responsibility for children could affect the mother's health and levels of stress which in turn could influence the child (Berger, Hill and Waldfogel, 2005). For this paper, we do not have access to health data so we cannot study this potential mechanism.

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Appendices

Table A.1. Pre-reform characteristics of mothers

Means (Sd)	(1) Single mothers	(2) Married mothers	(3) Younger single	(4) Older single
Age	27.01	30.55	22.96	31.69
	(5.45)	(4.84)	(2.10)	(4.25)
Years of education	10.74	11.87	10.38	11.18
	(1.75)	(2.22)	(1.28)	(2.10)
Work	0.42	0.71	0.32	0.54
	(0.49)	(0.45)	(0.47)	(0.50)
Earnings NOK(1998)	61,294	111,068	39,016	87,077
	(79,280)	(89,872)	(56,806)	(92,869)
Number of children	1.56	2.10	1.28	1.87
	(0.84)	(0.98)	(0.53)	(1.0)
Teenage mother	0.14	0.02	0.26	0
	(0.35)	(0.12)	(0.44)	0
Non-Norwegian	0.05	0.07	0.04	0.07
country of birth	(0.22)	(0.25)	(0.19)	(0.25)
Take-up transitional benefits	0.70	0.02	0.80	0.58
	(0.22)	(0.25)	(0.40)	(0.49)
Divorced/widowed	0.20	0.03	0.07	0.35
	(0.40)	(0.17)	(0.26)	(0.48)
At least one grandparent	0.75	0.67	0.84	0.66
live close	(0.43)	(0.47)	(0.37)	(0.47)
(both father's and mother's side)				

Notes: The sample is split such that the single mothers are not married nor cohabiting with the father of any of their children when the child turn two years old. The sample is further split such that the younger mothers are aged equal to- or below the median age for single mothers in each respective cohort. Characteristics are measured the year the child turns two years old. Mothers take-up shares of transitional benefits are calculated for the cohorts aged two in 1992-1997 (data available from 1992). 100 NOK equals approx. 14 EUR and 17 USD.

Table A.2. Matched sample -younger single mothers

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
GPA	3.51	-0.037*	-0.082***	61,575
		(0.019)	(0.020)	[32,607]
Written exam	2.98	-0.008	-0.081***	58,008
		(0.019)	(0.021)	[30,441]
Oral exam	3.77	-0.034	-0.092***	53,622
		(0.021)	(0.022)	[27,998]

*Notes: Partially refers to children aged two in 1991-1994, and Fully refers to children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.*

Table A.3. The role of control variables -younger single mothers

Dependent variable	(1) Mean singles untreated	Baseline		No control variables		Only education, earnings and age controls		(10) No of obs. [singles]
		(2) ITT Partially	(3) ITT Fully	(4) ITT Partially	(5) ITT Fully	(8) ITT Partially	(9) ITT Fully	
GPA	3.51	-0.031**	-0.069***	-0.051***	-0.129***	-0.030*	-0.079***	131,423
		(0.015)	(0.016)	(0.016)	(0.017)	(0.016)	(0.016)	[32,607]
Written exam	2.98	0.007	-0.052***	-0.006	-0.095***	0.007	-0.060***	125,050
		(0.015)	(0.016)	(0.016)	(0.017)	(0.016)	(0.017)	[30,441]
Oral exam	3.77	-0.032*	-0.066***	-0.042**	-0.107***	-0.030*	-0.073***	115,704
		(0.017)	(0.018)	(0.017)	(0.018)	(0.017)	(0.018)	[27,998]

Notes: Partially refers to children aged two in 1991-1994, and Fully refers to children aged two in 1995-1997. Controls included in baseline specifcation: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.

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Table A.4. Alternative samples -younger single mothers

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
A: No younger siblings (while child<16)				
GPA	3.53	-0.015	-0.074***	59,480
		(0.025)	(0.026)	[12,340]
Written exam	3.02	0.008	-0.054**	56,446
		(0.025)	(0.027)	[11,480]
Oral exam	3.82	-0.035	-0.083***	51,904
		(0.028)	(0.029)	[10,468]
B: Teenage mothers excluded				
GPA	3.55	-0.025	-0.062***	115,485
		(0.018)	(0.018)	[24,092]
Written exam	3.04	0.002	-0.066***	110,072
		(0.018)	(0.019)	[22,485]
Oral exam	3.81	-0.017	-0.044**	101,905
		(0.020)	(0.021)	[20,695]
C: Norwegian born mothers only				
GPA	3.51	-0.012	-0.045***	122,181
		(0.015)	(0.016)	[30,900]
Written exam	2.99	0.024	-0.032*	116,254
		(0.015)	(0.017)	[28,845]
Oral exam	3.77	-0.018	-0.049***	107,587
		(0.017)	(0.018)	[26,549]
D: Alternative age split mothers **<=26 years (child 2)**				
GPA	3.52	-0.030**	-0.044***	133,189
		(0.014)	(0.016)	[32,802]
Written exam	3.00	0.004	-0.034**	126,706
		(0.014)	(0.016)	[30,631]
Oral exam	3.78	-0.031**	-0.050***	116,947
		(0.016)	(0.018)	[28,148]

Notes: Partially refers to children aged two in 1991-1994, and Fully refers to children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.

Table A.5. Treatment by year of birth -younger single mothers

	Dependent variable		
	(1)	(2)	(3)
Linear treatment	GPA	Written exam	Oral exam
2003*Single mother	0.024	0.019	0.002
	(0.030)	(0.030)	(0.034)
2004*Single mother	0.001	0.014	-0.006
	(0.029)	(0.030)	(0.033)
2005*Single mother	0.011	0.018	-0.034
	(0.029)	(0.030)	(0.033)
2006*Single mother	-0.011	0.051*	0.011
	(0.028)	(0.029)	(0.031)
2007*Single mother	-0.046	-0.001	-0.086***
	(0.028)	(0.029)	(0.032)
2008*Single mother	-0.041	0.003	-0.024
	(0.029)	(0.029)	(0.033)
2009*Single mother	-0.069**	-0.039	-0.049
	(0.028)	(0.029)	(0.032)
2010*Single mother	-0.055*	-0.035	-0.084**
	(0.028)	(0.029)	(0.033)
2011*Single mother	-0.056**	-0.050*	-0.071**
	(0.029)	(0.030)	(0.032)
No of obs.	131,423	125,050	115,704

*Notes: 20XX*Single mother (interaction terms between cohorts aged 16 in year 20XX and having a single mother) measure yearly treatment effects relative to year 2002 (children aged two in 1988). Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. $*p<0.10$, $**p<0.05$, $***p<0.01$.*

Table A.6. Linear treatment -younger single mothers

Dependent variable	(1) Mean singles untreated	(2) ITT Linear	(3) No of obs. [singles]
GPA	3.51	-0.011***	131,423
		(0.002)	[32,607]
Written exam	2.98	-0.010***	125,050
		(0.003)	[30,441]
Oral exam	3.77	-0.011***	115,704
		(0.003)	[27,998]

*Notes: The linear treatment variable is an interaction variable between a coutning variable which takes the value 0 for children aged two in 1988-1990, the value 1 for children aged two 1991,..., and 7 for children aged two in 1997, – and the indicator variable for being a single mother. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.*

Table A.7. Placebo test -mothers earning more than treshold for receiving transitional benefits pre-reform (child 2)

Dependent variable	(1) Mean singles untreated	(2) ITT Partially	(3) ITT Fully	(4) No of obs. [singles]
GPA	4.06	0.065*	0.037	85,392
		(0.036)	(0.037)	[4,733]
Written exam	3.61	-0.008	-0.007	82,777
		(0.040)	(0.042)	[4,529]
Oral exam	4.42	0.027	-0.016	76,568
		(0.039)	(0.041)	[4,152]

*Notes: The treshold for receiving transitional benefits pre-reform was about 195 000 NOK in 1998 prices. Partially refers to children aged two in 1991-1994, and Fully refers to children aged two in 1995-1997. Controls included: child's gender, number of siblings, and mother's age, years of education, non-Norwegian country of birth, regional labor market of residence, earnings and labor market participation. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.*