FACULTY OF SOCIAL SCIENCES
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Essays on Economic Consequences of Complex Public Policies and Fertility Desires

Evidence from survey panels, registers and behavioral interventions

PhD Dissertation

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Introduction

This PhD dissertation consists of three self-contained chapters. The first chapter studies the role of fertility preferences of young women for their education, family formation and labor market outcomes. The second and third chapter share a common focus on the complexity of public policies, such as sanctions and work requirements, for job search and labor market outcomes of benefit recipients. The last chapters share a common methodology relying on evidence from recent large-scale field experiments among Danish jobseekers conducted in collaboration with The Danish Agency for Labor Market and Recruitment.

The first chapter is an empirical investigation of the link between fertility desires of young women and their later life outcomes. The paper bridges two extensive literatures on the role of childbearing for labor market outcomes of women. A mature structural literature on joint fertility and labor supply choices of women holds that economic consequences of childbirth and childrearing are fully foreseen, such that women, based on their joint preferences and earnings ability, make fully informed decisions about labor supply and childbearing trading-off utility from consumption with utility from children. The overarching assumption is that women with higher opportunity costs of childbearing will trade-off extensive childbearing for more intensive work lives with higher earnings (see e.g. Eckstein et al., 2019; Sommer, 2016; Conesa, 2002; Caucutt et al., 2002; Choi et al., 2011). In addition, in the structural model of Adda et al. (2017), fertility preferences influence not only expected childbearing, but also economic choices made in anticipation of childbirth, such as occupational choices. Women base their pre-birth choices on beliefs about occupation-specific childrearing compatibility, e.g. expected career costs arising at childbirth. Hence, fertility preferences have an effect on both pre- and and post birth economic outcomes of women.

At the same time, a rich applied literature seeks to identify causal post birth effects of child-

bearing on women's labor market outcomes, (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017). This literature relies on natural experiments to solve potential endogeneity issues due to women's capacity to (imperfectly) control their fertility. Relying on instrumental variables strategies, it identifies extensive and intensive margin causal effects of childbearing on women's labor market outcomes. The identified causal effects are generally sizable and long-lasting, (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017).

My research project bridges the idea of fully foreseen economic consequences of childbirth resulting from selection into motherhood, and arising both pre- and post birth, and the idea of unexpected post-birth economic consequences of exogenous increases in childbearing. Matching the Danish Longitudinal Survey of Youth, which elicits individual earnings potential in adolescence and fertility desires in early adulthood among a representative sample of 1500 women born around 1954, with detailed individual level register data on family formation, education and employment histories, I document that fertility desires in early adulthood matter for both childbearing and economic outcomes of women.

I find that desiring a large family is associated with anticipated childbearing and significant increases in realized fertility levels. Moreover, my results show that women desiring a large family face significantly lower earnings across working careers relative to women desiring a small family. Lower wage earnings reflect the influence of expected childbearing on choice of occupation, age at labor market entry, and labor supply across working career. This lends evidence to the importance of selection into motherhood for economic outcomes of women. Additionally, I find that expected income reductions from selection go hand in hand with negative labor market consequences of realized childbearing. The negative consequences of realized childbirth may reflect unexpected time constraints and changes in priorities occurring at birth, as well as unexpected childbearing reflecting imperfect transmission of fertility desires. Half of the women in my sample desiring a family end up realizing fertility below their desired level, while more than half of women with no desire for a family end up having children. One aspect of individual earnings potential, namely educational aspirations, increases the likelihood of experiencing a fertility deficit significantly on the intensive margin, though the impact of desired family size and delayed establishment of stable relationships is greater. Additionally, the variance of desires increases in earnings potential, such that both having no desire for children and desiring a large family of 3 or more children are more common among women expecting to complete Upper Secondary Education and with above median cognitive ability in adolescence.

My findings are relevant for the general validity of causal effects identified in the applied literature. Particularly, assumptions about the distribution of fertility desires among experimental populations have to be made explicit. In addition, my findings imply that relying on elicited fertility preferences in the estimation of structural models of fertility is warranted as some economic consequences of childbirth differ with the wantedness of children (Ejrnæs and Jørgensen, 2018).

Future avenues for research that I hope to pursue are the following: Accessing existing historical income data, I intend to split cumulative income reductions associated with fertility desires into average annual reductions occurring in anticipation of and after birth, respectively. Historical income data is a prerequisite for following income across fertile ages of women in my sample, including in their early childbearing years. In addition, I aim to test for heterogeneous consequences of fertility desires for women with fertility deficits, one-to-one transmission of desires, and excess fertility, respectively, to understand if the income reductions I find mainly reflect childbearing among women, who are successful in fulfilling their desires. Finally, gaining access to abortion data will allow me to assess to what extent fertility deficits are the result of altered childbearing intentions across fertile ages.

The second chapter studies how job seekers' understanding of complex unemployment insurance benefit rules affects their job search and labor market outcomes based on a randomized controlled trial conducted among the universe of Danish unemployment insurance recipients. Unemployment insurance systems in modern labor markets are riddled with a multitude of rules governing job seekers' benefit level and their incentives to search for employment, including work requirements for avoiding benefit sanctions (Lalive et al., 2005; Arni et al., 2013). While the rules aim at minimizing the moral hazard that typically arises in social insurance systems (Hopenhayn and Nicolini, 1997; Krueger and Meyer, 2002), their complexity is likely to distort individuals' job search incentives as it hampers their understanding of rule relevance for their personal economic situation. To investigate this topic, we conduct a large-scale field experiment among Danish unemployment insurance benefit recipients, in which we encourage individuals in our main treatment to make use of an online information tool providing up-to-date, personalized information on individuals' unemployment insurance benefit situation and corresponding rules. The digital tool is designed to increase people's understanding of rules concerning potential benefit duration and work requirements to avoid benefit sanctions. To evaluate the causal effect of our intervention, i.e. the impact of rule understanding on job search and labor market outcomes,

we match intervention evidence with detailed survey and register data. As the relevance of potential benefit duration depends crucially on time until benefit exhaustion, our analysis focuses on heterogeneous treatment effects with respect to elapsed benefit duration.

We document that job seekers have limited rule knowledge, and that increased use of the digital information tool improves the understanding of rules among treated job seekers by 8-10% relative to the control group. While knowledge improvements are similar across duration of benefit receipt, consequences for realized labor market outcomes depend crucially on the timing of treatment. Long-term benefit recipients, who are close to benefit expiration, tend to make greater use of marginal work opportunities, i.e. take up temporary and part-time jobs, in response to the treatment. The shift from regular to marginal jobs results in significantly fewer accumulated working hours and lower earnings over the first year after intervention. In contrast, treated job seekers who are at the beginning of their unemployment spell accept less part-time jobs and earn higher hourly wages. The differential effects of treatment are likely to reflect the different incentives that job seekers face over the course of the benefit period. For job seekers at the beginning of the benefit period, the prospect of extending the potential benefit duration reduces the pressure to accept low-quality jobs. However, those closer to benefit expiration have larger incentives to work in temporary or part-time jobs to actually extend their potential benefit duration, which has negative consequences for subsequent labor market performance.

Our findings have several implications. First, we show that online tools are a promising and potentially low-cost solution for improving individuals' understanding of complex rules. Especially, providing updated personalized information about underlying incentives generates behavioral changes among the target group (see also Fuentes et al., 2017). However, the consequences of greater knowledge about economic incentives depend on the actual design of the underlying policy. An improved understanding can have adverse effects on job seekers' subsequent labor market outcomes, if the underlying incentives are not suitable to promote sustainable employment. That is, our analysis questions the effectiveness of promoting marginal employment (see e.g. Booth et al., 2002; Heinrich et al., 2005) and supports the notion that locking-in effects may hamper the transition from unemployment to regular employment (see e.g. Fremigacci and Terracol, 2013; Kyyrä et al., 2013). Our project also contributes to an extensive empirical literature investigating the effects of the generosity of unemployment insurance systems on labor market outcomes. There is comprehensive evidence that extensions of the PBD period encourage individuals to search less intensively for new jobs (Lichter, 2016; Marinescu, 2017), increase the time spent in unemployment and non-employment (Katz and Meyer, 1990; Card and Levine, 2000; Lalive

et al., 2006; Van Ours and Vodopivec, 2006; Chetty, 2008; Schmieder et al., 2012, 2016), and result in ambiguous effects on the quality of subsequent job matches (Le Barbanchon et al., 2019; Centeno and Novo, 2009; Nekoei and Weber, 2017; Van Ours and Vodopivec, 2008).

The third chapter examines how the threat of sanctions and the imposition of sanctions affect job search and employment among recipients of social benefits. Our evidence is based on a randomized controlled trial among Danish recipients of Social Assistance. Sanctions and monitoring schemes are commonly used to enforce rule compliance in social security systems. Social planners aim to guarantee a minimum income allowing unemployed workers to smooth their consumption and improve subsequent job match quality, without disproportionately distorting their incentives to search for and accept new employment. This leads many developed countries to combine the provision of a minimum income with job search and work requirements enforced by the threat of sanctions. In this project, we investigate, how the threat of being sanctioned, respectively the enforcement of benefit sanctions, affect the labor market performance of welfare recipients. We report the results of a large-scale field experiment conducted among the universe of welfare recipients in the Danish labor market. Individuals who receive welfare benefits are required to work at least 225 hours a year, and non-compliance leads to a permanent reduction of the monthly benefit level. We exogenously vary the degree of information that welfare recipients receive about the requirement and potential benefit sanctions within a 2×3 between-subjects design. In a first treatment arm, individuals, who are *at-risk* to incur a reduction of their benefit level receive monthly warning messages with *general information* about the underlying rules emphasizing the threat of potential sanctions. This allows us to examine how warnings affects their subsequent labor market outcomes. A second treatment group, consisting of individuals who are *already sanctioned*, similarly receive monthly messages informing them that they are currently sanctioned, and on general rules on how to ensure compliance and lift the sanction. This allows us to analyze the effect of making it salient that they are exposed to sanctions (see Arni et al., 2017 for a further discussion of the differential effects). We also analyze the role of *personalized information* about the individual's own situation with regard to the work requirement. Therefore, in addition to the message, individuals in a second treatment arm gain access to an online tool that provides continuously updated personal information regarding the number of working hours that are missing to comply with the work requirement and the specific deadline for rule compliance, i.e. the potential benefit reduction date. The tool treatment allows us to investigate how information frictions about one's own situation influence the effectiveness of warnings

and benefit sanctions. Our experimental approach provides new insights and overcomes various limitations of the existing literature. First, it allows us to provide a clean identification of causal effects regarding various aspects of work requirements and benefit sanctions that are crucial to assess the overall effectiveness of such a policies. Prior evidence indicates that warnings as well as the imposition of benefit sanctions increase transition rates to employment (Abbring et al., 2005; Van den Berg et al., 2004; Lalive et al., 2005; Svarer, 2011), but lead to lower wages and reduced job stability (Arni et al., 2013; Van den Berg and Vikstrøm, 2014). Due to the absence of experimental variation, existing studies rely on strong identification assumptions, the violation of which can bias estimation results. Moreover, our study is the first that considers information frictions regarding the individual's own situation in the context of work requirements and benefit sanctions. Finally, in contrast to the vast majority of papers, we focus on welfare rather than unemployment insurance benefit recipients (see Van den Berg et al., 2004; Van der Klaauw and Van Ours, 2013, for two exceptions), who have very limited attachment to the labor market (Moffitt, 1992). We find evidence of substantial threat effects among individuals *at risk of being sanctioned*. Receiving repeated warnings, including information regarding the general rules, reduces the likelihood of being sanctioned as the treated are more likely to leave the welfare system compared to the control group. However, this does not translate into a higher probability of finding paid employment, but leads to a greater usage of other types of income support, such as educational benefits, and a higher likelihood to retire. Thus, warnings stimulate exits from the welfare system, but reduce subsequent earnings and working hours. Additionally, we find that combining warnings with personalized information about work requirements counteracts the negative effects of pure warnings, and can even improve the labor market performance for those, who stand to incur a sanction. Considering treated individuals who make use of the online tool, the provision of personal information results in improved labor market outcomes, including higher earnings and more working hours. Considering *already sanctioned* welfare recipients

receiving benefits at a reduced level, we find that the effectiveness of the information treatment crucially depends on the individual's marital status. This is not surprising as the partner's income typically reduces the individual benefit level and the incentives to react to the information treatment. Our results show that both treatments (general and personal information) increase earnings and working hours relative to the control group of welfare recipients without a partner living in the same household. However, personal information is more effective than pure warnings. Additionally, we find evidence that both personal and general information encourage

individuals to accept different types of jobs. General information, rendering the sanction more salient, leads to acceptance of jobs with a higher hourly wage, whereas personalized information encourages individuals to start jobs with a higher number of working hours.Our findings are in line with previous evidence for unemployment insurance benefit recipients. Specifically, warnings are found to be an effective tool to stimulate exits from the unemployment insurance system in Switzerland (Lalive et al., 2005; Arni et al., 2013), Sweden (Lombardi, 2019), and Germany (Boockmann et al., 2014). However, we show that this does not lead to higher employment rates as welfare recipients switch to other types of income support that are not subject to the sanction regime. Taken together, our results indicate that the consumption smoothing aspect of welfare benefits is particularly important (Card et al., 2007a,b; Chetty, 2008), whereas the threat of sanctions might be less effective in restoring search incentives. This argument finds further support in the fact that (general and personalized) warnings lead to a substantial reduction of the individual's total income, i.e. the sum of benefit payments and labor earnings.

For convenience, the three abstracts are listed below.

1. The role of fertility desires for family formation and economic outcomes of women

Matching data from the Danish Longitudinal Survey of Youth with rich administrative records, I conduct an empirical investigation of the direct link between young women's fertility desires and their educational, labor market and family formation outcomes, controlling for a rich set of factors related to earnings potential and socioeconomic background of individuals. I find that fertility desires translate into earlier childbearing and are associated with significant increases in realized fertility levels. None-the-less half of individuals in my sample experience fertility levels below desires and the likelihood of a fertility deficit increases in educational aspirations. While desired family type is uncorrelated with educational attainment, desiring a large family is associated with significant reductions in cumulative gross income and wage earnings over working career of 7-8% of mean sample income. Wage reductions associated with desires go hand in hand with wage reductions from realized fertility of 3.4% per childbirth. Income reductions reflect occupational choices, delayed entry to the labor market and decreased labor supply over working career. Realized childbirths exacerbate negative labor supply consequences. Surprisingly, having no desire for a family is associated with even larger reductions in gross income over working career of 11% of mean income, which cannot be accounted for by differences in labor supply nor in wage earnings.

2. Complexity and the Effectiveness of Public Policy

with Steffen Altmann, Robert Mahlstedt, and Alexander Sebald

We study how job seekers' understanding of complex UI benefit rules affects their job search and labor market outcomes based on a randomized controlled trial conducted among the universe of Danish UI benefit recipients. Our intervention exogenously promotes the usage of an online information tool that provides individuals with personalized information on how their accumulated working time can be used to prolong their potential benefit period. We match the data from our experiment with an online survey and comprehensive administrative records. The intervention improves job seekers' understanding of prevailing labor market rules significantly, while consequences for realized labor market outcomes crucially depend on timing of the treatment. For long-term benefit recipients, who are close to benefit expiration, the treatment increases their probability of working in temporary and part-time jobs, but reduces their overall working hours and earnings. Individuals treated at the beginning of their benefit spell on the contrary tend to accept jobs of higher quality.

3. Punishing the Poor? Work Requirements and Benefit Sanctions for Welfare Recipients

with Robert Mahlstedt

Monitoring and benefit sanctions are one of the major tools to enforce compliance with job search and work requirements among unemployed workers. We present results from a large-scale field experiment. Based on a 2×3 between-subjects design, we identify the causal effects of warnings and information about enforced sanctions on subsequent labor market outcomes of welfare recipients. We disentangle the effect of providing (i) general information about existing work requirements and (ii) personalized information regarding the individual's own situation. Although warnings generally reduce the likelihood of being sanctioned, the provision of general information reduces subsequent earnings and working hours. Providing access to personalized information counteracts the negative effect of the warning and stimulates exits from welfare. The latter is partly explained by a greater usage of other types of income support not subject to the sanction regime. Information about already enforced sanctions improve the labor market performance of welfare recipients without a partner.

Introduktion

Denne ph.d.-afhandling består af tre selvstændige kapitler. Det første kapitel undersøger, hvilken rolle unge kvinders fertilitetspræferencer spiller for deres uddannelsesvalg, familiedannelse og beskæftigelse gennem arbejdslivet. Andet og tredje kapitel handler om kompleksiteten af offentlige politikker og regler, såsom beskæftigelseskrav til ydelsesmodtagere i det danske dagpenge- og kontanthjælpssystem. Disse kapitler bygger på omfattende adfærdsøkonomiske feltinterventioner blandt arbejdsløse i Danmark gennemført i samarbejde med Styrelsen for Arbejdsmarked og Rekruttering.

Det først kapitel i afhandlingen er en empirisk undersøgelse af den direkte sammenhæng mellem unge kvinders ønskede fertilitet og hvordan de klarer sig økonomisk set. Kapitlet bygger bro mellem to omfattende litteraturer, der beskæftiger sig med sammenhængen mellem børnefødsler og kvinders arbejdsliv. Den strukturelle teoretiske litteratur om kvinders samtidige valg af arbejdsudbud og fertilitet antager, at kvinder fuldt ud kan forudse fremtidige økonomiske konsekvenser forbundet med fødsler. Baseret på deres præferencer og indtjeningsevne foretager de således en afvejning af nytte og karriereomkostninger ved børnefødsler. Det resulterer i, at kvinder med højere alternativomkostning ved børnepasning vil vælge at opgive en højere realiseret fertilitet for et arbejdsliv med højere indtjening (see e.g. Eckstein et al., 2019; Sommer, 2016; Conesa, 2002; Caucutt et al., 2002; Choi et al., 2011). Derudover, antages det i den strukturelle model i Adda et al. (2017), at fertilitetspræferencer ikke kun påvirker familiedannelsen, men også økonomiske valg, der træffes forud for familiedannelsen i forventning om fremtidige børnefødsler, fx erhvervsmæssige valg. Givet deres fertilitetspræferencer, baserer kvinder således deres valg af erhverv på forventede barselsomkostninger i bred forstand og erhvervets kompatibilitet med livet som småbørnsmor. Det indebærer, at fertilitetspræferencer får en effekt på kvinders valg af erhverv, beskæftigelse og indkomst både før og efter realiseringen af børnefødsler. Over

for den strukturelle litteratur står en rig anvendt litteratur, der er optaget af at identificere år-sagseffekterne af fødsler på kvinders arbejdsmarkedsresultater (se fx Bronars og Grogger, 1994; Rosenzweig og Wolpin, 1980; Angrist og Evans, 1998; Lundborg et al., 2017; Angelov et al., 2016; Kleven et al., 2018; Rosenbaum, 2019). Denne litteratur anvender naturlige eksperimenter til at løse potentielle endogenitetsproblemer, der følger af kvinders evne til (ufuldstændigt) at kontrollere deres fertilitet. På baggrund af instrumentel variabel-strategier identificerer den an-vendte litteratur betydelige og langvarige kausale effekter på den intensive og ekstensive margin af børnefødsler på kvinders arbejdsmarkedsresultater, (se fx Bronars og Grogger, 1994; Rosenzweig og Wolpin, 1980; Angrist og Evans, 1998; Lundborg et al., 2017). Mit forskningsprojekt bygger bro mellem disse to litteraturer, dvs. mellem ideen om forudsete økonomiske konsekvenser både før og efter børnefødsler drevet af fertilitetspræferencer, indtjeningsevne og økonomiske valg, og ideen om en kausal årsagssammenhæng mellem realiserede børnefødsler og kvinders arbe-jdsudbud og lønindkomst. Jeg baserer mit forskningsprojekt på Ungdomsforløbsundersøgelsen, særligt spørgsmål vedrørende fertilitetspræferencer og indtjeningsevne blandt en stikprøve af 1500 unge kvinder født omkring 1954. Disse paneldata kombinerer jeg med detaljerede danske registerdata om fødsler, pardannelse, uddannelse og arbejdsliv på individniveau. Baseret på dette datasæt foretager jeg en empirisk undersøgelse af, hvilken betydning fertilitetspræferencer i den tidligere voksenalder isoleret set har for, hvordan kvinder danner familie og hvordan de klarer sig på arbejdsmarkedet. Mine resultater viser, at kvinder, der ønsker sig en stor familie med 3 børn eller mere får flere børn end kvinder, der ønsker sig en lille familie på maksimalt to børn, og at de får børn tidligere. Kvinder, der ønsker sig en stor familie, har betydeligt lavere lønindkomst henover arbejdslivet end kvinder, der ønsker sig en lille familie, når der kon-trolleres for indtjeningsevne og socialøkonomiske baggrund. Den lavere lønindkomst afspejler betydningen af fertilitetsønsker for valg af erhverv, alder ved indtrædelse på arbejdsmarkedet og det samlede arbejdsudbud henover arbejdslivet. Disse resultater bekræfter isoleret set den økonomiske betydning af kvinders selektion ind i moderskabet. Jeg finder imidlertid, at den negative sammenhæng mellem fertilitetsønsker og lønindkomst går hånd i hånd med negative arbejdsmarkedskonsekvenser af realiserede børnefødsler. Det kan skyldes et større end ventet tidsforbrug til børnepasning, ændrede prioriteringer, eller afspejle konsekvensen af uplanlagte fødsler på kvinders arbejdsliv. Halvdelen af kvinderne i min stikprøve, der ønsker sig en familie, får færre børn, end de ønskede sig, og mere end halvdelen af de kvinder som ikke ønsker en familie ender med at få børn. Forventninger til uddannelse øger sandsynligheden for at opleve et fertilitetsunderskud, men forsinket pardannelse og det at ønske sig en stor familie har relativt

større betydning. Jeg finder desuden, at variansen i kvinders fertilitetsønsker øges med deres indtjeningspotentiale, således at kvinder, der forventer at gennemføre en gymnasial uddannelse eller har kognitiv intelligens over middel er mere tilbøjelige til at ønske sig ikke at stifte familie eller at stifte en stor familie. Mine resultater har betydning for den generelle validitet af de kausale effekter af børnefødsler som identificeres i den anvendte litteratur. Disse undersøgelser bør forholde sig til fordelingen af fertilitetspræferencer i de undersøgte populationer, da effekten vil afspejle underliggende præferencer hos populationen. Derudover peger mine resultater på, at indsamlede data om kvinders fertilitetsønsker med fordel kan indgå i estimationen af strukturelle modeller om kvinders valg af fertilitet og arbejdsudbud. Desuden bør de strukturelle modeller indrettes, så der tages højde for, at de økonomiske konsekvenser af børnefødsler varierer på tværs af planlagte og uventede børnefødsler. Særligt vil valg, der træffes forud for fødslen, kun være påvirket af forventede fødsler, mens fx konsekvenserne af børnefødsler på arbejdsudbud vil være mere ensartet på tværs (Ejrnæs og Jørgensen, 2018). I min fremtidige forskning ønsker jeg på baggrund af historiske indkomstdata at opdele sammenhængen mellem fertilitetsønsker og det samlede indkomstfald over arbejdslivet i sammenhængen før og efter første fødsel. Desuden ønsker jeg at teste, om de økonomiske konsekvenser af fertilitetsønsker varierer med, hvorvidt kvinden oplever henholdsvis fertilitetsunderskud, balance mellem ønsker og fertilitet eller fertilitetsoverskud. Endelig vil adgang til abortdata gøre det muligt for mig at vurdere, i hvilket omfang fertilitetsunderskud skyldes ændrede fertilitetsintentioner over tid.

Det andet kapitel i afhandlingen undersøger, hvordan dagpengemodtageres forståelse af komplekse dagpengeregler påvirker deres jobsøgning og beskæftigelse. Arbejdsløshedsforsikringssystemer på tværs af den moderne verden består af en lang række komplicerede regler, der fastsætter ydelsesniveau, ydelsesperiode, samt understøtter aktiv jobsøgning via beskæftigelseskrav og sanktioner (Lalive et al., 2005; Arni et al., 2013). Mens reglerne samlet set sigter mod at minimere moral hazard, der typisk kan opstå i sociale forsikringssystemer (Hopenhayn og Nicolini, 1997; Krueger og Meyer, 2002), kan deres kompleksitet betyde, at de arbejdsløse mister overblikket over, hvilke regler der er relevante for netop deres situation. For at undersøge betydningen af forståelse af dagpengereglerne for jobsøgning og fremtidig beskæftigelse, gennemfører vi et omfattende felteksperiment blandt samtlige danske dagpengemodtagere. De jobsøgende opfordres til at gøre brug af et online informationsværktøj, *dagpengetælleren*, der indeholder opdateret detaljeret information om den enkeltes dagpengesituation. Det digitale værktøj er designet til at øge folks forståelse af reglerne om mulige dagpengeforlængelser og opfyldelse af beskæftigelseskravet på

144 timer hver fjerde dagpengemåned. For at evaluere effekten, dvs. virkningen af regelforståelse på jobsøgning og arbejdsmarkedsresultater, matcher vi data fra eksperimentet med detaljerede spørgeskema- og registerdata. Da relevansen af dagpengeforlængelser afhænger direkte af, hvor tæt man er på at have opbrugt sine dagpenge, fokuserer vi på heterogene behandlingseffekter baseret på, hvor længe man har modtaget dagpenge. Vi dokumenterer i første omgang, at danske dagpengemodtagere har en begrænset viden om reglerne for dagpengeforlængelser og opfyldelse af beskæftigelseskravet, inklusiv sanktioner. Dernæst dokumenterer vi, at øget brug af dagpengetælleren forbedrer forståelsen af reglerne med 8-10% blandt de behandlede jobsøgende i forhold til kontrolgruppen. Mens effekten på forståelsen er ensartet på tværs af dagpengevarighed, finder vi at konsekvensen af øget forståelse for jobsøgning og beskæftigelse varierer signifikant med tidspunktet for behandlingen. De langtidsledige, der har modtaget dagpenge i et år eller mere, gør i højere grad brug af marginale beskæftigelsesmuligheder i form af midlertidig og deltidsbeskæftigelse. Det resulterer samlet set i færre arbejdstimer og lavere lønindkomst over det første år efter eksperimentet målt i forhold til kontrolgruppen. Dagpenge-modtagere, som netop er indtrådt i systemet, benytter sig mindre af deltidsjob og accepterer i gennemsnit job med højere timeløn end individer i kontrolgruppen. Disse forskelle afspejler for-modentlig, at dagpengemodtagere i begyndelsen af deres ydelsesperiode oplever, at muligheden for at forlænge dagpengeperioden reducerer presset til blot at acceptere det første og bedste job, som tilbydes, uden at skele til kvaliteten, mens de langtidsledige, der er tæt på dagpengeudløb, bliver klar over, at de ved at acceptere småjob rent faktisk får mulighed for at forlænge deres dagpengeperiode. Det får så negative konsekvenser for deres efterfølgende beskæftigelse. Vores resultater bidrager med indsigt på flere punkter. For det første viser vi, at onlineværktøjer er en lovende og potentielt billig løsning til at forbedre de jobsøgendes forståelse af komplekse regler. Især personlige og opdaterede onlineværktøjer, der oplyser om de underliggende inci-tamenter kan have stor adfærdsmæssig betydning blandt målgruppen (se også Fuentes et al., 2017). Konsekvenserne af øget viden om de økonomiske incitamenter afhænger dog direkte af den underliggende politik. Hvis de underliggende incitamenter ikke er egnede til at fremme varig beskæftigelse, kan en øget forståelse af reglerne således have negative konsekvenser for indkomst og beskæftigelse blandt målgruppen. Dermed sætter vores resultater spørgsmålstegn ved effektiviteten af at fremme marginal beskæftigelse blandt ledige (se f.eks. Booth et al., 2002; Heinrich et al., 2005). Samtidig er resultaterne i overensstemmelse med tidligere studier, som påviser, at småjob kan hæmme overgangen fra arbejdsløshed til almindelig beskæftigelse (se f.eks. Fremigacci og Terracol, 2013; Kyyrä et al., 2013). Vores projekt bidrager også til

en omfattende empirisk litteratur, der undersøger virkningen på jobsøgning og beskæftigelse af at øge generøsiteten i arbejdsløshedsforsikringsordninger. Tidligere studier finder at udvidelser af ydelsesperioden reducerer jobsøgningsintensiteten blandt arbejdsløse (Lichter, 2016; Marinescu, 2017), samt forlænger perioden for modtagelse af dagpenge og uden beskæftigelse (Katz og Meyer, 1990; Card og Levine, 2000; Lalive et al., 2006; Van Ours og Vodopivec, 2006; Chetty, 2008; Schmieder et al., 2012, 2016). Vores resultater peger dog på, at effekten af øget generøsitet vil afhænge af, hvor i dagpengeperioden man befinder sig.

Det tredje kapitel undersøger, hvordan truslen om sanktioner og tildelingen af sanktioner påvirker jobsøgning og beskæftigelse blandt modtagere af sociale ydelser. Sanktioner og overvågning anvendes ofte til at håndhæve reglerne i sociale forsikringssystemer. Systemernes arkitekter sigter overordnet mod at garantere fx arbejdsløse ydelsesmodtagere en minimumsindkomst, der gør det muligt at udjævne deres forbrug og finde et nyt jobmatch, uden at deres incitamenter til at søge efter og acceptere et nyt job forvrides unødigt. Derfor kombineres tilbuddet om en garanteret minimumsindkomst ofte med jobsøgnings- og beskæftigelseskrav, der understøttes af truslen om overvågning og sanktioner. I dette projekt undersøger vi, hvordan truslen om at blive sanktioneret, henholdsvis håndhævelse af ydelsessanktioner, påvirker arbejdsmarkedsresultaterne blandt modtagere af velfærdsydelser. Vores resultater er baseret på et omfattende felteksperiment udført blandt kontanthjælpsmodtagere i Danmark. Danske kontanthjælpsmodtagere er omfattet af et beskæftigelseskrav kaldet *225-timersreglen*, der stiller krav om, at den enkelte arbejder mindst 225 timer om året. Manglende overholdelse fører til en permanent reduktion af det månedlige ydelsesniveau. I eksperimentet varierer vi eksogent graden af information om beskæftigelseskravet og tildeling af sanktioner. I en første behandlingsarm modtager vores første behandlingsgruppe af kontanthjælpsmodtagere, der er *i fare* for at pådrage sig en reduktion af deres ydelsesniveau, månedlige advarsler med *generel information* om de underliggende regler, der understreger truslen om potentielle sanktioner. Dette giver os mulighed for at undersøge, hvordan advarsler påvirker modtagernes arbejdsmarkedsresultater. Vores anden behandlingsgruppe, der består af personer, der allerede *er sanktioneret*, modtager ligeledes månedlige beskeder, der informerer dem om, at de i øjeblikket er sanktionerede og om de generelle regler for, hvad de selv kan gøre for at ophæve sanktionen. Dette giver os mulighed for at analysere hvad der sker, når ydelsesmodtagerne bliver opmærksomme på, at de aktuelt er underlagt permanente sanktioner (se Arni et al., 2017). I en anden behandlingsarm sætter vi fokus på betydningen af at modtage *personlig information* om ens egen kontanthjælps- og beskæftigelsessituation i forhold

til beskæftigelseskravet. Ud over meddelelsen med generel information får individer i den anden behandlingsarm adgang til et onlineværktøj, der løbende opdateres med personlige oplysninger om antallet af arbejdstimer, der mangler for at overholde arbejdskravet og den specifikke frist for overholdelse af kravet, dvs. datoen for en potentiel reduktion af kontanthjælpssatsen. Denne behandling giver os mulighed for at undersøge, hvordan informationsfriktioner om ens egen situation påvirker effektiviteten af advarsler og sanktioner. Vores eksperimentelle tilgang bidrager med ny indsigt og overvinder forskellige begrænsninger i den eksisterende litteratur. For det første giver den os mulighed for en ren identifikation af årsagseffekter vedrørende forskellige aspekter af beskæftigelseskrav og sanktioner, der er afgørende for at vurdere den samlede effektivitet af sådanne politikker. Tidligere studier peger på, at advarsler såvel som tildeling af sanktioner øger afgangen til beskæftigelse (Abbring et al., 2005; Van den Berg et al., 2004; Lalive et al., 2005; Svarer, 2011), men fører til lavere lønninger og nedsat jobstabilitet (Arni et al., 2013; Van den Berg og Vikstrøm, 2014). På grund af manglen på eksperimentel variation er tidligere studier dog afhængige af stærke identifikationsantagelser, hvis overtrædelse kan påvirke validiteten af resultaterne. Desuden er vores undersøgelse den første, der overvejer informationsfriktioner vedrørende den enkeltes egen situation i forbindelse med arbejdskrav og tildelingen af sanktioner. Endelig, i modsætning til langt størstedelen af den tidligere forskning, fokuserer vi på modtagere af velfærdsydelser med en meget begrænset tilknytning til arbejdsmarkedet (Moffitt, 1992), frem de arbejdsløshedsforsikrede (se Van den Berg et al., 2004; Van der Klaauw og Van Ours, 2013). Vi identificerer signifikante effekter af trusler om sanktioner blandt individer *i fare for at blive sanktioneret*. Modtagelse af gentagne advarsler, herunder information om de generelle regler, reducerer sandsynligheden for at blive sanktioneret, da de behandlede er mere tilbøjelige til at forlade velfærdssystemet sammenlignet med individer i kontrolgruppen. Dette indebærer dog ikke en større sandsynlighed for at finde lønnet beskæftigelse, men øger brugen af andre ydelsesordninger fx SU og folkepension. Advarsler stimulerer således udgangen fra velfærdsydelsen, men reducerer efterfølgende lønindkomst og arbejdstid. Derudover finder vi, at kombinationen af advarsler og personlig information modvirker de negative effekter af rene advarsler og endda kan forbedre arbejdsmarkedstilknytningen blandt individer *i fare for at blive sanktioneret*. Blandt individer, der rent faktisk gør brug af onlineværktøjet, resulterer adgangen til personlig information forbedrede arbejdsmarkedsresultater såsom øget lønindkomst og flere arbejdstimer i det første år efter behandlingen. Blandt de ydelsesmodtagere, som allerede *er sanktioneret*, finder vi, at effekten af behandlingen afhænger af den enkeltes civilstand. Dette er ikke overraskende, da tilstedeværelsen af en partner reducerer den forsikringsmæssige værdi af

ydelsen, samtidig med, at det individuelle ydelsesniveau typisk sænkes, da partnerens indkomst bliver modregnet. Det betyder, at incitamenterne til at reagere på behandlingen reduceres. Blandt ydelsesmodtagere uden en partner i samme hustand, der allerede *er sanktioneret*, øger både behandlingen med generel og med personlig information lønindkomsten og antallet af arbejdstimer i forhold til kontrolgruppen. Derudover finder vi, at henholdvis personlig og generel information tilskynder de behandlede ydelsesmodtagere til at acceptere forskellige typer af job. Generel information, der gør sanktionen mere fremtrædende, fører til accept af job med en højere timeløn, mens personlig information tilskynder til at starte i job med et højere antal arbejdstimer. Vores resultater er i tråd med tidligere studier, der dog fokuserer på de arbejdsløshedsforsikrede. Specifikt har advarsler vist sig at være et effektivt værktøj til at øge afgangen fra arbejdsløsheds-forsikring i Schweiz (Lalive et al., 2005; Arni et al., 2013), Sverige (Lombardi, 2019) og Tyskland (Boockmann et al., 2014). Vi viser dog, at advarsler ikke øger sandsynligheden for afgang til beskæftigelse, da ydelsesmodtagerne blot skifter til andre typer af overførselsindkomst, der ikke er underlagt sanktioner. Samlet set indikerer vores resultater, at det forbrugsudjævnende aspekt af velfærdsydelser er særlig vigtigt (Card et al., 2007a, b; Chetty, 2008), hvorimod truslen om sanktioner måske er mindre effektiv i forhold til at stimulere jobsøgningen. Dette argument finder yderligere støtte i det faktum, at både generelle og personlige advarsler fører til en bety-delig reduktion af den enkeltes samlede indkomst af ydelser og lønindkomst. Endelig peger vores resultater på betydningen af informationsfriktioner i forhold til arbejdskravet, og betydningen af kommunikation af offentlige regler mere generelt i forhold til at sikre reglernes effektivitet. Ved at øge adgangen til digitale informationsværktøjer kan informationsfriktioner i systemer for velfærdsydelser reduceres, og de marginale omkostninger per bruger er lave. Det er en vigtig pointe, da intransparens og misforståelser af gældende regler eller økonomiske incitamenter kan forvride både jobsøgning (Altmann et al., 2018) og beslutninger om arbejdsudbud (Chetty og Saez, 2013; Liebman og Luttmer, 2015).

Chapter 1

The Role of Fertility Desires for Family Formation and Economic Outcomes of Women

The Role of Fertility Desires for Family Formation and Economic Outcomes of Women

Sofie Cairo[*]

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Abstract

Matching data from the Danish Longitudinal Survey of Youth with rich administrative records, I conduct an empirical investigation of the direct link between young women's fertility desires and their educational, labor market and family formation outcomes, controlling for a rich set of factors related to individual earnings potential and socio-economic background.

I find that fertility desires translate into earlier childbearing and are associated with significant increases in realized fertility levels. None-the-less half of individuals in my sample experience fertility levels below desires and the likelihood of a fertility deficit increases in educational aspirations. While desired family type is uncorrelated with educational attainment, desiring a large family is associated with significant reductions in cumulative gross income and wage earnings over working career of 7-8% of mean sample income. Wage reductions associated with desires go hand in hand with wage reductions from realized fertility of 3.4% per childbirth. Income reductions reflect occupational choices, delayed entry to the labor market and decreased labor supply over working career. Realized childbirths exacerbate negative labor supply consequences. Surprisingly, having no desire for a family is associated with even larger reductions in gross income over working career of 11% of mean income, which cannot be accounted for by differences in labor supply nor in wage earnings.

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1 Introduction

Bridging two extensive literatures on the role of childbearing for labor market outcomes of women, in this paper I conduct an empirical investigation of the link between fertility desires of young women and their economic outcomes. A mature structural literature on joint fertility and labor supply choices of women holds that economic consequences of childbirth and childrearing are fully foreseen, such that women, based on their joint preferences and earnings ability, make fully informed decisions about labor supply and childbearing trading-off utility from consumption with utility from children. The overarching assumption is that women with higher opportunity costs of childbearing will trade-off extensive childbearing for more intensive work lives with higher earnings (see e.g. Eckstein et al., 2019; Sommer, 2016; Conesa, 2002; Caucutt et al., 2002; Choi et al., 2011). In addition, in the structural model of Adda et al. (2017), fertility preferences influence not only expected childbearing, but also economic choices made in anticipation of childbirth, such as occupational choices. Women base their pre-birth choices on beliefs about occupation-specific childrearing compatibility, e.g. expected career costs arising at childbirth. Hence fertility preferences have an effect on both pre- and and post birth economic outcomes of women. At the same time, a rich applied literature seeks to identify causal post birth effects of childbearing on women's labor market outcomes, (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017; Angelov et al., 2016; Kleven et al., 2018; Rosenbaum, 2019). This literature relies on natural experiments to solve potential endogeneity issues due to women's capacity to (imperfectly) control their fertility. Relying on instrumental variables strategies, it identifies sizeable and long-lasting extensive and intensive margin causal effects of childbearing on women's labor market outcomes (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017). To bridge these two literatures, I conduct an empirical investigation of the direct link between elicited fertility desires of individual women and their later life economic outcomes. Throughout the analysis, I condition on a rich set of observables related to individual earnings potential, including on time preferences, cognitive ability and educational aspiration, to address the key struc-

tural assumption of a negative relationship between an individual's opportunity costs and her childbearing. Specifically, I aim to answer the following research question:

> *Conditional on earnings potential, how is a woman's desired family type linked to her realized family formation, educational attainment, and labor market outcomes?*

In answering this question, my paper contributes to several existing literatures. First, it complements the applied literature on causal effects of childbearing in that it considers both expected and unforeseen effects of motherhood on labor market outcomes of women. Secondly, it contributes to an extensive literature documenting the importance of preferences and personality during adolescence for later life economic outcomes and well-being (see e.g. Golsteyn et al., 2014; Heckman et al., 2006; Epper et al., 2020). Third, it contributes to a rich demographic literature concerned with the transmission of fertility desires into realized family formation and with the link between childbearing and educational attainment (see e.g. Berrington, 2004; Bhrolcháin et al., 2010). [1] Finally, my study contributes to a mature structural literature, which integrates fertility and labor supply choices of women into dynamic life-cycle models. In models of fertility choice, individual women solve an intertemporal utility maximization problem conditional on joint preferences for family formation and consumption, and given structural constraints and uncertainty. Existing models differ on the dimensions of fertility preferences included with some integrating both quality and quantity dimensions in utility arising from children, and others allowing for heterogeneous taste parameters that scale utility contributions from childbirth. However, the common trait is that fertility preferences play a central role for the expected effect of childbirth on future cost and utility trade-offs faced by the individual, hence for choices made prior to and post birth, and for overall long term

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[1]Historically, demographers gathered data on fertility intentions and aggregate fertility to generate population forecasts. The reliance on aggregate data precluded an assessment of the importance of individual intentions for realized fertility, let alone for economic outcomes. More recently, demographers have focused on family planning, particularly the increasing congruence of intentions and fertility outcomes as birth control increased over time (Vlassoff, 1990; Mueller et al., 2019). Finally, elicited fertility intentions of couples have informed the structuring of models of household bargaining, shedding light on the importance of bargaining for the realization of fertility (see e.g. Berrington, 2004; Doepke and Kindermann, 2019; Miller and Pasta, 1995). Miller and Pasta (1995) finds that intra-couple disagreement is common and tends to result in delays.

economic outcomes of women (see e.g. Adda et al., 2017; Eckstein et al., 2019; Sommer, 2016; Conesa, 2002; Caucutt et al., 2002; Choi et al., 2011). The concept of joint or competing preferences constrained by scarce resources (time and savings), exacerbated by uncertainty, are thus key in explaining the educational, occupational and family choices of women, which involve trade-offs between labor market participation and family formation. Typically, fertility preferences, and other preferences remain unobserved in the estimation of models of fertility choice. This implies that unknown parameters of the utility function are backed out from realized outcomes to maximize the fit of data and model.[2] I add to this literature by linking *observed* individual preferences directly to economic outcomes of women conditional on a rich set of observables related to individual earnings potential.

I base my empirical investigation on a number of hypotheses regarding the correlation between fertility preferences on one hand and family formation, educational and labor market outcomes on the other. In formulating these hypotheses, I rely on existing theoretical models of fertility choice, particularly the model presented in Adda et al. (2017) and on the rich applied literature on the causal effects of childbirth on labor supply and earnings of women (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017; Angelov et al., 2016; Kleven et al., 2018; Rosenbaum, 2019). I supplement with insights from the literature on the importance of cognitive ability, personality and preferences for educational choices and economic outcomes of individuals (see e.g. Abbott et al., 2013; Golsteyn et al., 2014; Heckman et al., 2006; Mischel et al., 1988; Epper et al., 2020).

My main hypotheses are that desiring a large family is associated with anticipated childbearing and a higher level of realized fertility, while fertility preferences have hardly any effect on educational achievement, when conditioning on individual earnings potential. Moreover, fertility desires are associated with selecting into specific occupations based on beliefs about child rearing compatibility, postponement of employment entry, and with lower cumulative wage earnings and labor supply over working career. Finally, expected labor market consequences due to selection go hand in hand with unforeseen consequences

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[2]One exception is found in Ejrnæs and Jørgensen (2018), which relies on survey evidence on fertility targets and intentions of married couples for the identification of fertility preferences in a dynamic model of fertility choice.

of childbirth Hence, my research project bridges the idea of fully foreseen economic consequences of childbirth resulting from selection into motherhood, and arising both pre- and post birth, and the idea of unexpected post-birth economic consequences of exogenous increases in childbearing.

I rely on a rich dataset combining an extensive survey panel, the Danish Longitudinal Survey of Youth (DLSY), with detailed administrative registers on education, family formation, and labor market outcomes. The DLSY follows a random sample of 1300 women born around 1954 from adolescence throughout their working lives, and includes measures of individual earnings potential such as cognitive ability scores, educational expectations and time preferences as well as rich information on the socio-economic background of respondents. Moreover, the survey contains fertility preferences elicited around age 22, allowing me to empirically investigate the direct link between fertility desires and outcomes. Inspired by the theoretical framework in Adda et al. (2017), I organize fertility desires into categories of desired family type, specifically *No desire, small family (1-2 children), large family (>3)* and *Don't know*. Arrangement of desires into categories is warranted by the fact that a majority of individuals in my sample have fertility preferences centered on the 2-child societal norm, while remaining individuals primarily deviate from this norm by desiring either no family or a large family. Throughout the empirical analysis, I control for family formation status at elicitation of fertility desires to control for potential bias in results arising from correlations between elicited desires and, e.g., relationship and parental status at elicitation (see e.g. Mueller et al., 2019).[3]

My empirical analysis reveals several interesting results: First, I document that among women in my sample who have not experienced motherhood at elicitation of desires, 10% have no desire for a family, more than half desire a small family of 1-2 children, while 1/3 desire a large family of 3 or more children. Desires for one child are practically inexistent. The variance of fertility desires increases in cognitive ability and educational aspirations in adolescence, while desires are orthogonal to individual time preferences. Women with above median cognitive ability and high educational aspirations are thus more likely to

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[3]At the time of elicitation of fertility desires, 1/3 of women in my sample are already mothers. I include dummies to allow for differential effects for early mothers.

desire either no family or a large family. While greater variance of desires has previously been linked to level of completed education (Testa, 2014), I believe I am the first to link variance of desires to educational aspirations and cognitive ability in adolescence.

For the transmission of desires into family formation, my results confirm that positive family desires translate into a significantly earlier childbearing debut (1.6-2.3 years) relative to individuals with no desire for a family, while desiring a large family is associated with additional anticipation of 1/4-1/2 years relative to individuals desiring a small family. Moreover, age at first birth is strongly and positively associated with individual time preferences and educational aspirations, confirming existing evidence (see e.g. Epper et al., 2020; Geruso and Royer, 2018; Black et al., 2008; Miller, 2011; Herr, 2016). As documented by demographers (see e.g. Mueller et al., 2019; Berrington, 2004; Doepke and Kindermann, 2019; Bhrolcháin et al., 2010; Günther and Harttgen, 2016), I find that stronger fertility desires are associated with higher levels of realized fertility. Particularly, desiring children is associated with an increase of .3 childbirths relative to no desire, while desiring a large family is associated with an additional increase of .5 childbirths. Never the less, among individuals desiring a family, close to half end up with fewer children than desired reflecting both extensive and intensive margin issues. 10-15% of individuals with desire end up childless, while 1/5 end up with no more than one child despite having desires for more. The risk of fertility deficits is unrelated to ability and time preferences, while educational aspirations are associated with a significant increase in the likelihood of experiencing a deficit. The link between fertility deficits and completed education mirrors findings of Heiland et al. (2005). The association between fertility deficits and educational aspirations is an intensive margin result. Neither time preferences, cognitive ability nor educational aspirations influence the likelihood of remaining childless. The association between deficits and aspirations is also robust to controlling for age at first birth, indicating that delayed childbearing cannot explain deficits. The higher likelihood of fertility deficits among women with educational aspirations may be of even greater relevance today, as women continue to pursue higher education, while being less likely to pursue education and family formation in parallel than women in my sample are. The main drivers of deficits appear to be delayed formation of stable relationships, instability of relationships over-

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all, as well as actual level of desires, which is mainly a mechanical result.For educational achievement, my results confirm that completed level of education, both secondary and highest education, are uncorrelated with family formation desires, once the individual's earnings potential is taken into consideration. In line with prior evidence (see e.g. Golsteyn et al., 2014; Epper et al., 2019; Heckman et al., 2006; Abbott et al., 2013), I find that the the main drivers of level of education are own and parental expectations, cognitive ability, time preferences and self-esteem during adolescence. In line with reduced form results in Adda et al. (2017), I find suggestive evidence that individuals desiring a large family are more likely to select into less abstract occupations characterized by greater childrearing compatibility than individuals desiring a small family, while individuals desiring a small family are more likely to select into abstract occupations.

For labor market outcomes, I find that desiring a large family is associated with significant income losses of 2 mill. DKK in 2019-price levels over working career relative to individuals desiring a small family. The cumulative reduction in gross income is equivalent to 8% relative to the sample mean. The result is robust across samples. Once individual labor market outcomes are included as controls, the association practically disappears, indicating that income effects are transmitted via labor market earnings, particularly labor supply. In fact, individuals desiring a large family also experience significant cumulative wage losses over working career equivalent to a 7% reduction relative to the sample mean. The wage losses following from desired family type go hand in hand with wage reductions due to realized fertility of 3.4% per childbirth. This indicates that some share of economic consequences of childbirth are unforeseen, i.e. occur independently of desired family type. Investigating the mechanisms responsible for income and wage reductions, I find that individuals desiring a large family are more likely to delay labor market entry, but more importantly desiring a large family is associated with a lower cumulative labor supply over working career, reflecting both intensive and extensive margin effects, e.g. fewer years in the labor market, fewer years with full time employment and higher cumulative unemployment. Realized fertility exacerbates the negative consequences on labor supply, as realized childbirths are associated with a higher share of time spent in

part time employment and as assisting spouse, and with reductions in overall job stability proxied by average job duration.

My findings confirm the importance of both selection effects and direct unexpected effects of realized childbirth on labor market outcomes of women. The association between realized childbirths and labor market outcomes conditional on desires corroborates findings of Ejrnæs and Jørgensen (2018) that some economic consequences of realized childbirth occur independently of the wantedness of children. This is in line with identification of negative child penalties in the applied literature (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017). However, my results also lend evidence to the role of self-selection following from utility trade-offs between family formation and career as an important driver of career costs related to childbearing, Adda et al. (2017).

Finally, somewhat puzzling, I find that having no desire for a family is associated with significant reductions in cumulative gross income over working career equivalent to 11% of the sample mean. The association remains sizeable and significant at 5% after controlling for completed education and labor market outcomes. In fact, wage earnings of women desiring no family are not significantly different from wage earnings of individuals desiring a small family. Though my analysis of labor market mechanisms do indicate that working lives of individuals with no desire for a family are characterized by less stability, labor market outcomes alone cannot solve the puzzle.

My results confirm that the inclusion of heterogeneous fertility preferences are relevant for the design and estimation of structural models on joint fertility and labor supply choices of women. Particularly, the modelling of high and low preference types is warranted, as their economic outcomes differ significantly. The addition of agents with no desire for children may be warranted as these agents tend to postpone childbearing significantly compared to women with positive desires. Since labor market outcomes are not markedly different for women with no desire, I speculate that their savings behaviors may differ from that of other women. This is plausible, if desiring no family decreases incentives to accumulate buffer-savings in early adulthood and in anticipation of childbirth. However, the issue calls for further investigation.

One caveat of my analysis is that I rely on a one-off measure of fertility desires, which precludes an evaluation of stability of preferences over time. Particularly, I find that more than 60% of individuals with no initial desire for a family end up as mothers. While household bargaining may explain part of this phenomenon (Doepke and Kindermann, 2019; Miller and Pasta, 1995), it appears that a substantial fraction of women develop a taste for fertility later in life. Hence, policy initiatives that extend the biological window of childbearing may be warranted. These may also help the large fraction of women realizing fertility below desires. Potential policy initiatives to achieve this aim are extended access to freezing of eggs and increased access to IVF-treatment for women, who are either childless or have already had their first or second child. However, as negative labor market effects from increased levels of fertility do exist independently of fertility desires, overall labor supply in society and individual earnings are likely to suffer because of increased childbearing. Therefore, initiatives increasing overall gender equality in childrearing are equally important.

The remainder of the paper is organized as follows. Section 2 presents the testable hypotheses on which my empirical analysis relies. Section 3 presents data sources, samples and descriptives on fertility desires and covariates. Section4 presents my outcome measures. Finally, Section 5 presents my empirical model and estimation results, while Section 6 concludes.

2   Empirical strategy

As the applied literature on the direct link between fertility preferences and economic outcomes of women is scarce, I rely on theoretical insights from structural life-cycle models of joint fertility and labor supply choices of women to form testable hypotheses on the direct link between desired family type and outcomes. These are investigated empirically relying on reduced form OLS estimations in Section 5.

2.1   Structural literature on fertility choice

Early structural models on fertility choice and the dependence of fertility choice on the earnings potential of individuals were cast in a static framework. These models accounted

for the observed negative relationship between earnings and completed fertility either by the quantity-quality trade-off or by the analysis of optimal time allocation of households. The quantity-quality trade-off approach was pioneered by Becker (1960) and Becker and Lewis (1973), who modelled preferences in which parents value not only the quantity or number of children, but also the quality of children. In this set-up, higher income was associated with a lower quantity of, but a higher quality of children, and thus higher hourly wages would imply lower completed fertility. An alternative approach taken by Mincer (1963); Becker (1965) and Willis (1973) to explain the observed negative relation between earnings and number of children, was the time-allocation approach that treated childcare as a labor intensive activity - more or less costly depending on the opportunity costs of working, i.e. a mother's outside option characterized by her wage earnings from employment. Thus, women with higher earnings potential would optimally choose to dedicate more time to work and less to childrearing leading to lower realized fertility. Another consequence of the time allocation framework was that no more than one household member would allocate time to both market production and house production (childcare) due to gains from specialization. The assumption of a negative relationship between childbearing and opportunity costs of women is still reflected in many models on fertility choice.

The next generation of structural models on fertility choice were dynamic, but the first intertemporal lifecycle models on fertility choice studied women's choice of labor supply or fertility in isolation, holding one or the other fixed. [4] Some of the earliest papers, which build and estimate life cycle models with joint decisions on female labor supply and fertility are Rosenzweig and Wolpin (1980); Moffitt (1984); Hotz and Miller (1988), while more recent models are found in Gayle et al. (2012); Sheran (2007); Choi et al. (2011); Sommer (2016); Aguero and Marks (2008); Ejrnæs and Jørgensen (2018). The recent models build on the opportunity cost explanation to account for the empirical correlation between earnings potential and completed fertility with elaborate extensions to account

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[4]Endogenous dynamic fertility with exogenous labor supply (see e.g. Wolpin, 1984; Hotz and Miller, 1988, 1993; Heckman and Willis, 1976; Ward and Butz, 1980; Rosenzweig and Schultz, 1985; Cigno and Ermisch, 1989; Blackburn et al., 1993; Heckman and Walker, 1990; Hotz and Miller, 1993; Leung, 1994; Arroyo and Zhang, 1997; Altuğ and Miller, 1998). Endogenous dynamic labor supply with exogenous fertility (see e.g. Heckman and MaCurdy, 1980; Blau and Robins, 1988; Eckstein and Wolpin, 1989; Van der Klaauw, 1996; Hyslop, 1999; Attanasio et al., 2008; Keane and Sauer, 2009; Blundell et al., 2016)

for the role of idiosyncratic income uncertainty and fertility risks for the dynamics of fertility choice. The combination of risk averse agents and uninsurable income risk implies a trade-off between building assets in early career and pursuing maternity, while fecundity is high. Due to the nature of a childbirth, which has similar characteristics as an investment in a durable consumption good, requiring a future stream of time and consumption investments, idiosyncratic uninsurable income risk implies a need for precautionary savings among young households planning to conceive. On a macro-level, this implies that increasing income risk (uncertainty) leads to fertility delay and overall fertility reductions (see e.g. Choi et al., 2011; Sommer, 2016; Ejrnæs and Jørgensen, 2018). While elaborate on the role of income and fertility risk on fertility choices and outcomes, this strand of recent papers is silent on the joint influence of earnings potential and fertility preferences on women's pre- and post birth choices regarding education, occupation and labor supply. They model post birth career costs as proportional wage deductions per child, and are thus uninformative on the role of fertility preferences on women's initial human capital investments and initial income paths prior to the realization of fertility.

To address this issue, the authors of Adda et al. (2017) build a dynamic life-cycle model that illustrates the complex interactions between fertility desires and occupational choices of women. It is central to the model that the utility of pre-birth investments in human capital is not only weighed up against a future earnings stream conditional on ability, but also against the opportunity costs of expected births given fertility desires, including foregone earnings, foregone human capital investments, and skill attrition during child-related intermittencies from work. Occupation specific opportunity costs, as reflected in skill attrition rates during intermittency are thus a central feature of the model, but also the assumption that skill attrition rates differ across career stages is of great importance. Particularly, it matters for determining the individual's optimal timing of birth.Essentially conditional on earnings potential, women face a trade-off between pursuing abstract occupations characterized by higher initial wages and higher wage paths over career combined with higher atrophy rates during work interruptions, and pursuing manual/automatic occupations characterized by lower initial wages and lower wage paths over career combined with lower atrophy rates during work interruptions. Women select-

ing into abstract occupations also face a trade-off between pursuing fertility in their most fertile period and facing specific career-timing attrition costs. Relying on reduced-form evidence Adda et al. (2017) finds that career costs occurring at birth increase until intermediate career in abstract occupations, while they are almost constant in non-abstract occupations. This inspires the modelling assumption of increasing career costs in abstract occupations and constant career costs in less abstract occupations, modelled as constant wage growth deductions. The implication is that individuals selecting into less abstract occupations face no restrictions on optimal timing of birth from their occupational choice. Selecting into a less abstract occupation is then conducive to lower age at first birth.

From the overall model framework in Adda et al. (2017) hypotheses on the association between fertility desires and outcomes are formulated to guide my empirical analysis. These are presented in Subsections 2.2, 2.3 and 2.4. Relevant empirical evidence from the applied literature is included to broaden the perspective.

2.2 Fertility desire and family formation

In this section, I present my hypotheses on the transmission of fertility desires into realized family formation. In structural models of fertility choice, a higher taste for fertility will ceteris paribus result in a higher level of realized fertility at the individual level. The positive relationship between fertility desires and realized fertility is also well established in the empirical demographic literature. Despite a tendency for individuals to overestimate the number of children they will give birth to over their reproductive career, the lifetime fertility desire of an individual is a strong predictor of realized fertility (see e.g. Berrington, 2004; Bhrolcháin et al., 2010; Günther and Harttgen, 2016; Miller and Pasta, 1995; Bongaarts, 2001; Quesnel-Vallée and Morgan, 2003).

A natural first step is therefore to test the hypothesis of a positive correlation between desired family type and realized fertility. I assume that the relation between desires and realized childbirth increases in both number of children desired and in type of family desired, i.e. fertility is increasing across the categories *no family, small family* and *large family*. However, given biological and economic risks, I do not assume a perfect correlation between desired family type and realized childbirths. The transmission from desires to re-

alized births is likely to be sensitive to the establishment of stable relationships, individual infertility or reduced fertility risks, as well as to economic constraints and uncertainty (see e.g. Choi et al., 2011; Ejrnæs and Jørgensen, 2018; Sommer, 2016). The fertility outcomes of women with no desire for family formation have not gained prior attention. Here, I assume that women with no desire for family formation are less likely to end up as parents than women desiring a family. However, I do expect a significant share of women with no desire to end up as parents for reasons ranging from household bargaining and risk of unwanted pregnancies to taste shocks sparked by changes is current circumstances. Thus, the transmission of *no desire* to family formation is also imperfect.

The next aspect to consider is the effect of fertility desires on timing of birth. In most structural models on fertility choice, a woman desiring family formation will ceteris paribus pursue fertility as soon as possible, as utility from desired children then accrues over a longer time period. This implies that the number of children desired will not affect timing of fertility directly for women with positive desires. Observed differences in timing, i.e. fertility delay, are then modelled as arising from credit constraints in early adult life or from uninsurable idiosyncratic income risk. Income risk induces agents to accumulate buffer savings before taking on economic responsibilities of parenthood (Ejrnæs and Jørgensen, 2018; Choi et al., 2011; Sommer, 2016).[5] An alternative mechanism to mitigate income risk is human capital investments as included in the model of Sommer (2016), which results in delay of both buffer-saving and fertility. Finally, matching and fertility delays may arise, if individuals pursue education in order to access matching markets of a higher quality, i.e. to gain access to partners with a higher insurance value as in the model of Abbott et al. (2013). Delays due to human capital investment and accumulation of savings may be exacerbated by increasing age-related biological fertility risks, which prolong the time to conceive.

While most models do not link fertility preferences and timing directly, the model of Adda et al. (2017) allows timing to depend on fertility desires. As women select into occupations, which have either constant career costs of childbirth over time or rising

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[5]In the models of Choi et al. (2011) and Ejrnæs and Jørgensen (2018) risk averse agents build a buffer of wealth prior to pursuing fertility, since investments in children involve future fixed economic commitments, similar to investments in durable goods, which cannot be rewarded if income shocks occur.

career costs until intermediate career, women with strong desires are more likely to select into occupations, which do not constrain their optimal timing, while women with weaker desires are more likely to select into abstract occupations in which delayed timing (past intermediate career) is optimal. The occupational choice resulting from desiring a small family, i.e. into the abstract occupation, is then conducive to starting a family later and having fewer children on average, while the occupational choice resulting from desiring a large family is conducive to pursuing earlier and more extensive childbearing.

On the ability of individuals to realize their fertility desires, existing structural models propose two mechanisms increasing the risk of fertility deficits, namely delayed efforts to conceive and individual earnings ability. Factors that delay the realization of fertility such as idiosyncratic income risk are likely to increase biological fertility risks, resulting in a higher risk of experiencing a fertility deficit. In addition, high opportunity costs of childbearing reflecting individual earnings ability (and exacerbated by completed education) impose a steeper trade-off between career and family formation and may result in fertility below desires. Finding that the risk of experiencing a deficit is linked to individual earnings potential would indicate that women with a higher opportunity cost trade-off lower fertility for higher earnings (Becker, 1965; Becker and Lewis, 1973).

The insights above motivate the following working hypotheses on family formation: *Stronger fertility desires are associated with lower age at first birth and higher levels of realized fertility, and women with a higher earnings potential face a higher risk of realizing fertility below desires.*

2.3   Desires and education

In this section, I formulate my hypotheses on the link between fertility preferences and educational outcomes. I address two issues namely the potential link between fertility desires and level of education and the importance of fertility desires for occupational choices.

The overriding assumption of the model in Adda et al. (2017) is that fertility preferences will influence choices on occupation and career path taking place in anticipation of family formation due to anticipation of occupation-specific childrearing compatibility

and expected opportunity costs of intermittencies, i.e. parental leave in connection with childbirth. Women desiring a large family will then be less likely than women desiring a small family to opt for abstract occupations characterized by delayed optimal timing of birth and higher opportunity costs at intermittency.

At the same time, an extensive empirical and structural literature documents that educational choices and achievements are strongly correlated with individual time preferences, personality and cognitive ability as founded in nature and nurture, i.e. determined by parental traits and investments and general conditions while growing up. The individual's endowment of earnings potential determines both her taste for education (utility from education), as well as her cognitive and non-cognitive costs of making investments in human capital (see e.g. Abbott et al., 2013; Heckman et al., 2006; Golsteyn et al., 2014; Epper et al., 2019).

I reconcile these literatures by assuming that all educational choices are based both on individual earnings potential and on beliefs about future childrearing compatibility of potential working lives in relation to fertility desires. However, beliefs about childrearing compatibility will play a very minor role for decisions on level of education, since educational acchievement is not strongly linked to later work life conditions. Hence, choices on level of secondary and post-secondary education will mainly reflect individual earnings potential and parental expectations. Additionally, they will be driven by a strong path-dependence reflecting the general structure of European educational systems, which tend to split individuals into academic and vocational tracks from early on.[6] Conditional on earlier educational outcomes, expectations on final level of education, joint preferences, including fertility desires, the individual makes her choice of occupation (as proxied by field of study) based on beliefs about occupation-specific childrearing compatibility. This implies that among individual desiring a family, the individual's occupational choice will be strongly fertility desires, while for individuals with no desire for family formation, occupational choices will be only weakly, if at all, correlated with fertility desires.

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[6]In Denmark the completion of Lower Secondary Education was used to pave the way for Upper Secondary Education, followed by post-secondary studies for a Bachelor's degree or higher level university degree.

This motivates the following working hypothesis on the association between desires and education: *Desired family type has no significant impact on educational attainment, but influences choice of occupation due to beliefs about career-specific childrearing compatibility.*

2.4 Desires and labor market outcomes

Considering now the association between desires and labor market outcomes of women, income reductions related to fertility desires arise both prior to and post birth in the theoretic framework of Adda et al. (2017). Anticipatory income effects reflect occupational choices based on beliefs about occupation-specific childrearing compatibility. Particularly, pre-birth occupational choices result in specific job characteristics, e.g., a specific wage path, flexibility of work schedule and potentially a given sector of employment. This has consequences for earnings (and potentially labor supply) prior to and even in the absence of any realized birth. In addition to these anticipatory effects, larger fully foreseen negative effects on labor supply and income occur at childbirth. Post birth effects on income reflect foregone human capital investment and skill attrition during intermittency, which result in lower post birth wage growth (as accumulated work experience is devaluated), while absences per se result in reduced post birth labor supply and lower earnings during work interruptions. Finally, the assumption that occupation specific opportunity costs of work interruptions vary over career stages, imply that occupational choices may affect timing of employment entry and family formation, as delayed birth is optimal for individuals selecting into abstract occupations.

The association between desires and cumulative earnings over working career will then reflect the cumulative effect of desires on pre- and post birth labor market outcomes. If there was no transmission of desires, the association would reflect only pre-birth effects of occupational choice on earnings, while if the transmission was perfect, the association between desires and earnings should be equal to the association between realized fertility and earnings. However, with imperfect transmission of desires into realized fertility, the correlation between desires and earnings is likely to reflect some combination of antici-pated pre and post birth factors on earnings. From the above reasoning, the hypothesis

is that individuals desiring a large family will have significantly lower cumulative wage earnings over working career than individuals desiring a small family, as a reflection of their occupational choice, wage path, labor supply and realized family formation.

Occupational choices of individuals with no desire for children cannot reflect expected motherhood. Hence occupational choices and choices on employment entry must be made without considerations of childrearing compatibility. This implies a positive association between no desire and cumulative wage earnings over working career as individuals with no desire do not face a trade-off. Conditional on having children, women with no initial desire for family formation, should experience (only) unforeseen consequences of childbirth on earnings. Thus the inclusion of a no desire type with some exogenous probability of childbirth very explicitly gives rise to the existence of unforeseen career costs.[7]

The existence of costs from unforeseen childbirth is paramount to the assumption underlying a rich applied literature, which seeks to identify causal post birth effects on women's labor market outcomes (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017; Angelov et al., 2016; Kleven et al., 2018; Rosenbaum, 2019). This literature relies on natural experiments to solve potential endogeneity issues due to women's capacity to (imperfectly) control their fertility. Hence, relying on instrumental variables strategies, it identifies extensive and intensive margin causal effects of childbirth on women's labor market outcomes. The identified effects are generally sizable and long-lasting (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017).

In recent years, the IV-approach has been complemented by quasi-experimental event-studies on the trend of women's earnings around the time of birth (see e.g. Angelov et al., 2016; Kleven et al., 2018; Nix et al., 2019; Rosenbaum, 2019). These studies demonstrate that earnings of women and men follow similar trends until the time of childbirth, at which point earnings of women drop dramatically, and only partially recover over time.

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[7]In the model of Adda et al. (2017) the world consist only of individuals desiring a small or a large family.

In Kleven et al. (2018) the cumulative drop in earnings over the first 10 years is found to be around 10% per child on average. This reflects primarily drops in labor supply. [8]

Evidence that realized fertility gives rise to additional labor market effects, after controlling for fertility desires, could indicate that expected labor market consequences of child birth differ from actual consequences of realized childbirth, generating unforeseen effects. Kuziemko et al. (2018) document that women underestimate the time costs of childrearing, as mothers-to-be overestimate the expected number of hours they will work post-birth, while Kleven et al. (2018) cites an OECD study showing that the percentage of women believing mothers should decrease their working hours, while their children are small, increases with the arrival of children. This suggests some combination of unforeseen time constraints and taste shocks occurring at birth.

My empirical analysis will not offer conclusive evidence on whether negative associations between realized fertility and income are due to unforeseen childbirths reflecting imperfect transmission of desires into realized fertility or whether they reflect unforeseen consequences of childbirth as described above. Hence, I consider unforeseen costs of childbirth to result from both.

Reconciling the structural assumption that career costs are fully foreseen and the empirical literature on unforeseen consequences of realized childbirth, gives rise to the following hypotheses: *Desiring a large family is associated with a greater tendency to delay labor market entry and with lower expected cumulative earnings and labor supply over working career. Moreover, negative labor market consequences of desires and realized fertility go hand in hand reflecting imperfect transmission of desires into fertility as well as unforeseen consequences of childbirth.*

Before testing my hypotheses empirically, I present my dataset and samples in Section 3, as well as outcome measures in Section 4.

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[8]Other mechanisms explored in Kleven et al. (2018) are post-birth selection into public sector employment and family friendly firms, as well as post-birth promotion probabilities.

3   Data

My main data source is the Danish Longitudinal Survey of Youth (DLSY), which is a panel study of 3,151 individuals born around 1954. The respondents were first interviewed in 1968 around age 14, when they attended 152 different seventh grade classes across Denmark. They were sampled to be nationally representative. The full sample was subsequently interviewed in several waves throughout adolescence and adult life with high response rates. Around 70 pct. of the original individuals still participated in the 2001 wave at age 47. In addition, parents of respondents were interviewed in 1969 making it possible to control for respondents' socioeconomic environment during childhood. In my analysis, I focus on the subset of 1300 female respondents, who participated in survey waves from 1968, 1973 and 1976. These waves are crucial to my analysis as they contain detailed data on background characteristics, preferences and cognitive ability of individuals in my sample.I link the survey data to high quality register data administered by

Statistics Denmark. From the registers, I observe respondents' family formation histories, labor market histories, and educational attainment. The registers on educational outcomes include level and type of education as well as field of study. The registers on labor market outcomes and income include entry year, labor supply, participation, as well as income and earnings across working career. Registers on family formation include data on marriages, cohabitations and divorces, as well as on number of births and year of birth. I supplement with survey evidence on relationships and childbirths in early adulthood (1973, 1976) and over full fertile age (2001).

3.1   Population and samples

The original DLSY sample consists of 3,151 respondents, of which 1,563 are women and 1,588 are men. I remove 11 observations with missing birth year or having a birth year prior to 1952. This leaves me with 3,140 observations. I also remove nine observations for teenage parents, i.e. respondents who experienced a birth prior to the age of 17, as these pregnancies have a high probability of being unplanned. This leaves me with 3,131 observations in the full sample of women and men, of which 1,548 respondents are women.

Limiting the sample to individuals for whom survey data from 1968, 1973 and 1976 is available, leaves me with approximately 1,312 female respondents.

Table 1: Survey Waves

Year of survey	1968	1973	1976	1992	2001
Average age in years	14	19	22	38	47
Total respondents	3,131				
Men	1,580	1,396	1,293	1,124	1,059
Women *(Sample 1)*	1,546	1,380	**1,312**	1,196	1,125
Parental status, 1976					
Early mother			381		
Other respondents			931		
Relationship status, 1976					
Cohabiting/Married, 1976			880		
Not Cohabiting, 1976			432		
Desire					
Desire *(Sample 3)*			**1,106**		
No desire			193		
Don't know			13		
Early mother, 1976					
Desiring (additional) children			281		
No (additonal) desire			96		
Don't know			4		
Other respondents, 1976					
Total			931		
Desiring children			825		
No desire			97		
Don't know			9		
All, except early mother w/ no desire					
Total *(Sample 2)*			**1212**		
Early mother, desire			281		
Other respondents			931		

Note: All female respondents in the sample are born between 1952 and 1956. 83 per cent are born in 1954, while 98 pct. are born between 1953 and 1955. 20 respondents with a missing date of birth, a birth in 1951 or who became parents prior to the age of 17 are removed.

From Table 1, we see that almost 1/3 of the 1,312 female respondents were mothers at elicitation of fertility desires in 1976. I refer to this subgroup as *Early mothers*, while women, who were not mothers at elicitation are referred to as *Other respondents*. As discussed below in Subsection 3.2, I cannot rule out that fertility desires correlate with parental status at elicitation. Therefore, my analysis relies on three samples, where sample

1 includes all $1,312$ respondents, who participated in waves 1968, 1973 and 1976. For estimations on sample 1, I include dummies for *Early mother with no (additional) desire*, and *Early mother with additional desire*. Sample 2 consists of all respondents, except *Early mother with no (additional) desire*, as it is unclear, whether these individuals have no additional desire, because their target is already met, or if they did not intend to have children in the first place. Sample 2 consists of $1,212$ respondents. Finally, sample 3 consists of the $1,106$ respondents, who expressed a positive desire for (additional) children, whether or not they were mothers at elicitation. For samples 2 and 3 a dummy for *Early mother* is included in the estimations. Restricted samples risk being less representative, which is why I mainly focus on the full sample in my empirical analysis. From Table 1, we also see that $2/3$ of respondents were already cohabiting or married at the time of elicitation of fertility desires. Almost 90% of early mothers were cohabiting or married, while a little more than half of other respondents were cohabiting or married at elicitation of desires. As desires may also correlate with being in a stable relationship, I include controls for relationship status at elicitation throughout the empirical analysis.

3.2 Taste for fertility

I now present the raw measures of elicited fertility desires and the constructed measure of desired family type inspired by Adda et al. (2017). Additionally, I adress limitations of the survey evidence and how to accommodate potential concerns. I proceed to address potential confounding factors related to the earnings potential of individuals.

Since the first generation of models, architects of structural models on fertility choice have described fertility preferences by some marginal utility contribution arising from each additional child birth and potentially from the quality of children (see e.g. Becker, 1960, 1965; Becker and Lewis, 1973; Abbott et al., 2013; Eckstein et al., 2019; Adda et al., 2017; Ejrnæs and Jørgensen, 2018; Sommer, 2016; Choi et al., 2011). Similar to consumption goods or leisure, utility contributions from childbirth have been assumed concave in the number of children, reflected in high marginal utility of children at zero, but decreasing steeply in number of children. In later models, heterogeneity in fertility preferences is

modelled by including individual specific taste multipliers on utility contributions from children. [9]

In an empirical analysis of the direct link between economic outcomes and fertility preferences, the first concern is then, what measure of fertility preferences should be used to proxy individual specific taste for fertility. One way to think of an individual's taste for fertility is in terms of some ideal level of fertility that the individual would strive to achieve in the absence of any constraints and uncertainty. More broadly, an individual's taste for fertility may be proxied by some ideal number of childbirths. However, as the individual faces budget and time constraints, as well as income and fertility risks, she is forced to trade-off intertemporal utility from fertility with utility from consumption. Given this trade-off, she may choose to delay childbirth, reflecting e.g. infinite marginal utility of consumption at very low levels of income, a need for precautionary savings to meet future economic commitments in light of idiosyncratic income risk or if time-profiles of career costs show a decreasing trend over time as in the model of Adda et al. (2017). The agent might also end up having fewer children than desired, if career costs are very high, biological fertility risks high or institutional constraints binding, e.g., in the absence of childcare.

While taste parameters on fertility remain unobserved in the estimation of most structural models on fertility choice, Ejrnæs and Jørgensen (2018) rely on elicited fertility targets and short-term fertility intentions of married couples in the estimation of their model. This allows for modelling marginal utility from childbirth as dependent on whether childbirth is the result of effort to conceive or occurred by chance. The authors find that

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[9]In the model of Adda et al. (2017) the preferences of female agents are reflected in increasing concave utility contributions taking as inputs taste for fertility, number of children, occupation, leisure and marital status. Similar to consumption goods or leisure, the marginal utility of children is assumed high at zero, but decreasing steeply in number of children. Leisure is interacted with utility of children due to the complementarity between leisure and childrearing. This is essentially equivalent to assuming that child quality resulting from parental time investments increases the utility of children as in the seminal analyis by Becker (1960); Becker and Lewis (1973). The individual's taste for fertility is modelled as a scalar on the utility contribution of child number 1 and child number 2, respectively. Other than affecting the utility of children, the taste for fertility parameter also affects the probability of marriage, which is modelled as an exogenous non-linear function of the age of a woman, and her experience describing her career stage, while the probability of divorce is affected only by the number of children. Finally, marginal costs of child rearing are assumed decreasing in childrearing compatibility of occupation and in sharing the responsibility with a husband.

some economic behaviors differ systematically with wantedness of children. Particularly, couples are found to dissave to smooth consumption for wanted children, while there is no effect on savings for unwanted children. [10] For labor supply, the authors instead find that the negative effects of childbirth are similar across the wantedness of children. Taken at face value, results in Ejrnæs and Jørgensen (2018) lend evidence to the idea that selection based on preferences matter for anticipated or planned economic outcomes, while exogenous or mechanical costs arising from childbirth coexist. These mechanical effects may be closer to the negative causal effects of childbirth as identified in the applied literature (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017).

In order to assess the direct link between desires and economic outcomes empirically, we are interested in a stable measure of the unconstrained lifetime fertility desires of individuals that will proxy the (unobserved) stable individual preferences relied upon in the existing life-cycle models on fertility choice. The ideal measure of fertility preferences would consist of repeated survey elicitations initiated during adolescence and following respondents up until their first birth.

3.3   Desired fertility in DLSY

My measure of individual desired fertility is a one-off measure of desired fertility of individuals elicited in the DLSY survey wave of 1976. The measure was elicited, when respondents were around 22 years old. Respondents answered the following question: *Would you like to have (additional) children?* To which they could answer: *Yes, No* or *Don't know*. Only if they answered *Yes* were they presented with the follow-up question: *How many children would you like to have (including the ones you already have)?* To which they could answer: *None, 1, 2, 3, 4 or more* or *Don't know*. By combining these two questions on fertility preferences and interpreting *No desire* for children, as a desire for zero children, I construct two raw measures of desires, namely *Desire versus no desire* and

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[10]This is in line with insights from the standard consumption-savings buffer-stock models of Deaton Angus (1991) and Carroll et al. (1992); Carroll and Samwick (1997), in which permanent expected income shocks lead to adjustment of savings in order to smooth consumption, while there is no effect on savings from negative unexpected income shocks, implying that consumption must take a larger share of the adjustment.

Number of children desired. From the latter, I arrange desires into three main categories of *Desired family type*, namely *Desires no family, desires a small family of $1-2$ children* and *Desires a large family of* 3 *ormore children*. I also pool individuals answering that they *Don't know* in either the first or second fertility question into a fourth category. The arrangement into categories is inspired by the structural framework in Adda et al. (2017), though their model includes only two types, namely low and high desires. In line with tradition, the theoretical framework in (Adda et al., 2017) does not include a *No desire type* as it relies on unobserved taste for fertility, and as such cannot distinguish between childlessness due to biological fertility risks and childlessness as a result of preferences. However, including this additional type is a natural choice for my analysis given my access to a direct measure of fertility preferences. Moreover, the arrangement into family types is warranted by the insight that the distribution of desires is centered on a 2-child norm with more than half of the sample desiring two children. Women can then deviate from the norm by expressing either desire for a large family or for no family at all. Desire for one child is practically inexistent. The distribution of desires implies that including desires as a continuous measure to find monotonously increasing or decreasing associations with outcomes is less informative.

My main concern with relying on a one-off elicitation of fertility desires is whether preferences are stable over time and to what extent current circumstances, specifically marital and parental status at elicitation, influence individual fertility desires. I address these concerns below and present strategies to accommodate them.

Without repeated elicitations of desires, evaluating the stability of desired fertility is impossible, however earlier studies have found that measures of unconstrained desired fertility are likely to be more stable over time than elicited measures of expected, planned or intended fertility. Measures of *expected fertility* have been found to take into consideration long-term constraints known to the respondent at the time of elicitation, e.g. infertility or other physical constraints to fertility, and measures of *fertility intentions* or *planned fertility* are considered short-term measures influenced by considerations of optimal timing of fertility given the individual's current situation. Individuals and couples have been found to adjust their fertility intentions in response to economic uncertainty, changes in

the institutional framework, but also in line with their perceived ability to have a child or under the influence of broader social norms on reproductive age. Essentially, the more constrained a preference measure is, the more predictive of fertility behavior it is likely to be. However, the opposite is true for stability. Elicitations of desired fertility are found to be the most stable measure of fertility preferences over time, as desires refer to some ideal level of fertility unconditional on constraints, uncertainty and the current state of the individual. Mueller et al. (2019) finds that even presented with hypothetical scenarios of natural disaster or economic distress very few women tend to alter their measure of overall desired fertility downwards. However, Mueller et al. (2019) does find evidence of retrospective adjustments of fertility targets in one particular situation, namely as a result of excess fertility, i.e. realized fertility above an earlier stated fertility target. [11] This implies that changes in parental status may influence elicited desires. Hence, I control for parental status at elicitation of desires in estimations of my empirical model. Survey questions on parental status at elicitation are presented in Appendix A.1.

The next issue is the influence of current relationship status on elicited desires. Particularly, the influence of being in a stable relationship is of concern. 2/3 of women are cohabiting or married at elicitation of desires, and desired family type could be correlated with being in a stable relationship as both are essential elements of family formation, cf. the assumption of Adda et al. (2017) that taste for fertility has an impact on the probability of marriage. One might also speculate that entry into a stable relationship could generate taste shocks resulting in stronger family desires. Finally, elicited desires might be the result of household bargaining expressing desires of the couple rather than of the individual. This is however less of a concern as Doepke and Kindermann (2019) find large discrepancies between elicited desires of husbands and wives. I accommodate these concerns by controlling for relationship status prior to (1973) and at elicitation of desires (i.e. in 1976). I thereby hope to reduce potential bias in the coefficients on desired family

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[11]Particularly, Mueller et al. (2019) finds that individuals increase their fertility desires retrospectively, if they end up having more children than they originally desired, and mothers, who revise their fertility targets retrospectively cannot recall their original desires, nor whether original targets were lower or higher than revised targets.

type from relationship status. Survey questions on relationship status are presented in Appendix A.2.

3.4 Earnings potential

To isolate the effect of desires on family formation and economic outcomes, it is important to control for confounding factors that correlate with fertility desires, while exerting a significant influence on outcomes. The positive correlation between level of education and desires is well documented in the demographic literature,(see e.g. Bongaarts, 2001; Quesnel-Vallée and Morgan, 2003; Testa, 2014; Iacovou and Tavares, 2011). These studies document that women completing relatively higher education intend to have more children than women completing less education, but ultimately end up having fewer children (Testa, 2014; Iacovou and Tavares, 2011). Another strand of the demographic literature documents that the variance of desires increases in level of completed education of individuals, such that desiring no family or a large family is more frequent among individuals with higher education (Heiland et al., 2005).At the same time, it is well documented that individual earnings potential is positively associated with a row of long-term economic outcomes such as educational attaiment, labor market outcomes, health and well-being of the individual, as well as with family formation via human capital investment choices and opportunity costs of childbearing. Especially, non-cognitive and cognitive ability, taste for education and time preferences have been documented to have long lasting effects on economic outcomes of individuals (see e.g. Golsteyn et al., 2014; Heckman et al., 2006; Epper et al., 2020). The potential correlation between individual earnings potential and fertility desires, and the positive association with outcomes implies a risk of biased estimates. This risk is accommodated by controlling for a rich set of covariates on individual earnings potential in all estimations of outcomes on desires. The survey measures of adolescent time preferences, cognitive ability and educational aspirations for secondary education and self-esteem are presented in Appendix A.3.

3.5 Simple descriptives on desires

Having presented my dataset, samples, and measures of fertility desires and potential confounders, I proceed with simple descriptives on the distribution of desires and desired family type, including by earnings potential and relationship status at elicitation.

3.5.1 Distribitions of desires and desired family type

From Figure 1, we see that 1/4 of early mothers do not desire additional children, while 3/4 of early mothers do. For other respondents, i.e. individuals, who are not mothers at elicitation, a little more than 10% have no desire for children, while almost 90% do.

Figure 1: Desires by parental status at eliciation
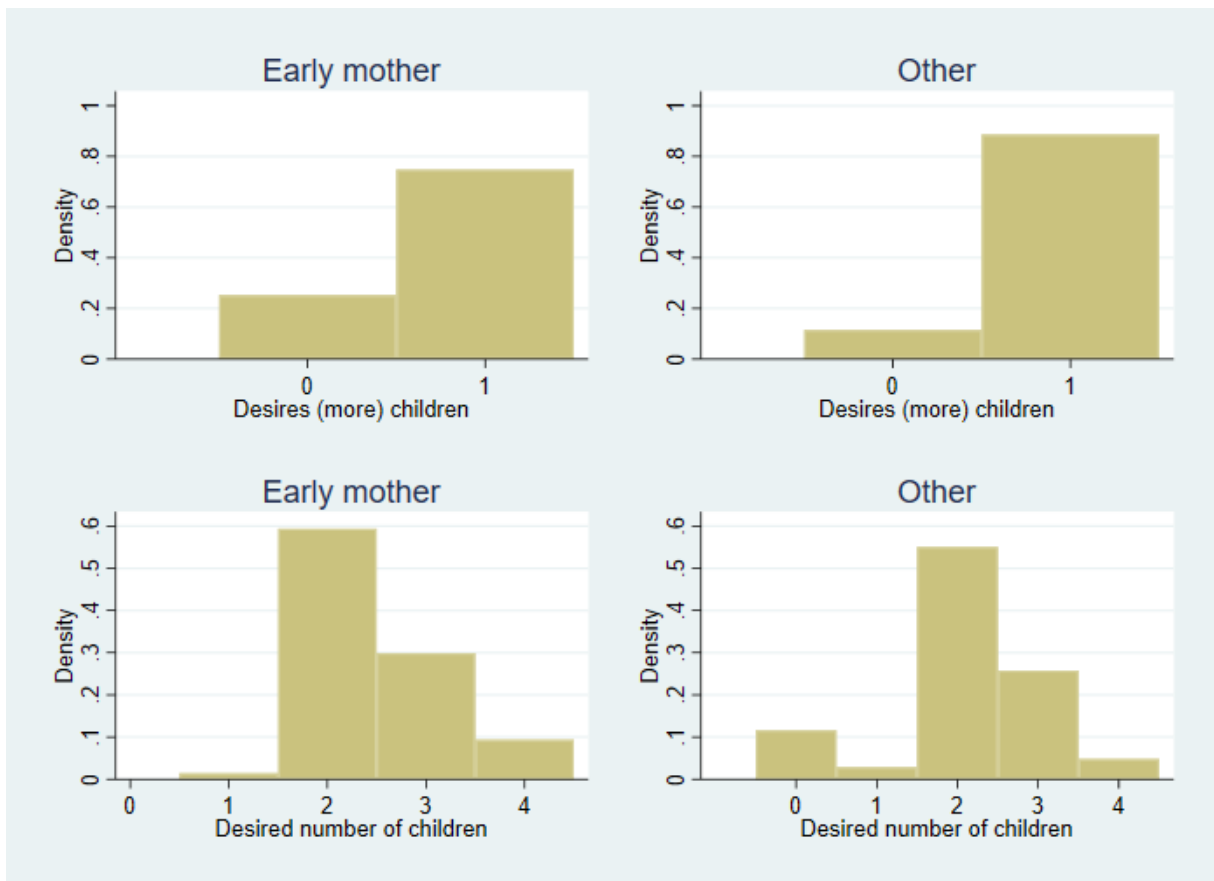
Note: Separate histograms are presented for early mothers ($n = 381$) and other respondents ($n = 931$).

Among early mothers with additional desire for children, more than half of individuals (59%) desire two children demonstrating a strong 2-child norm, while 2/3 desire three children. Finally, less than 10 pct. desire four or more children. The distribution of desires

among other respondents is similar. A little more than half (55%) of individuals desire 2 children, close to 1/4 desire 3, and only 5% desire 4 or more children. It is worth noting that desires for one child are practically inexistent. Early mothers with additional desire on average desire 2.5 children, while other respondents with positive desires, desire 2.4 children on average. [12]

For early mothers, the tendency to desire additional children correlates with the number of children they have at elicitation, cf. Figure 2. On average, early mothers desiring additional children have 1.1 children at elicitation, while early mothers with no additional desire have 1.6 children at elicitation. One extra child at elicitation is associated with a 37% lower probability of desiring additional children lending evidence to the interpretation that the answer *No (additional) desire* implies that these individuals have already reached their fertility target. [13]

Among early mothers with additional desire, the probability of having (more) children after elicitation is 83%, while the probability is only 17% among early mothers with no additional desire. In contrast, among other respondents with no desire, the probability of parenthood is close to 65%, while other respondents with desire for children have a probability of 85% of becoming parents. This implies three things. First, early mothers and other respondents with (additional) desire follow similar patterns of realizing fertility after elicitation of desires. Hence, the subgroups can potentially be grouped. Second, early mothers with no desire and other respondents with no desire differ significantly in their realization of fertility. Finally, fertility preferences of other respondents with no desire may be less stable than preferences of other subgroups. This could reflect a range of factors, such as taste shocks, unexpected pregnancies or household bargaining.

In Figure 3 desires are organized into categories of desired family type. Category 1 represents *No (addtional) desire*, category 2 represents *Desire for a small family*, and category 3 represents *Desire for a large family*. Category 4 consists of women, who answered that they *Don't know* if they desire (additional) children or how many they desire. Among other respondents, 10% have no desire for a family, while more than 1/2 desire a small

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[12]If women, who do not desire children at elicitation are included, the average number of children desired is 2.1 among other respondents.

[13]Based on bivariate LPM regression of the dummy *Desire* on realized fertility in 1976 for early mothers.

Figure 2: Number of children at elicitation
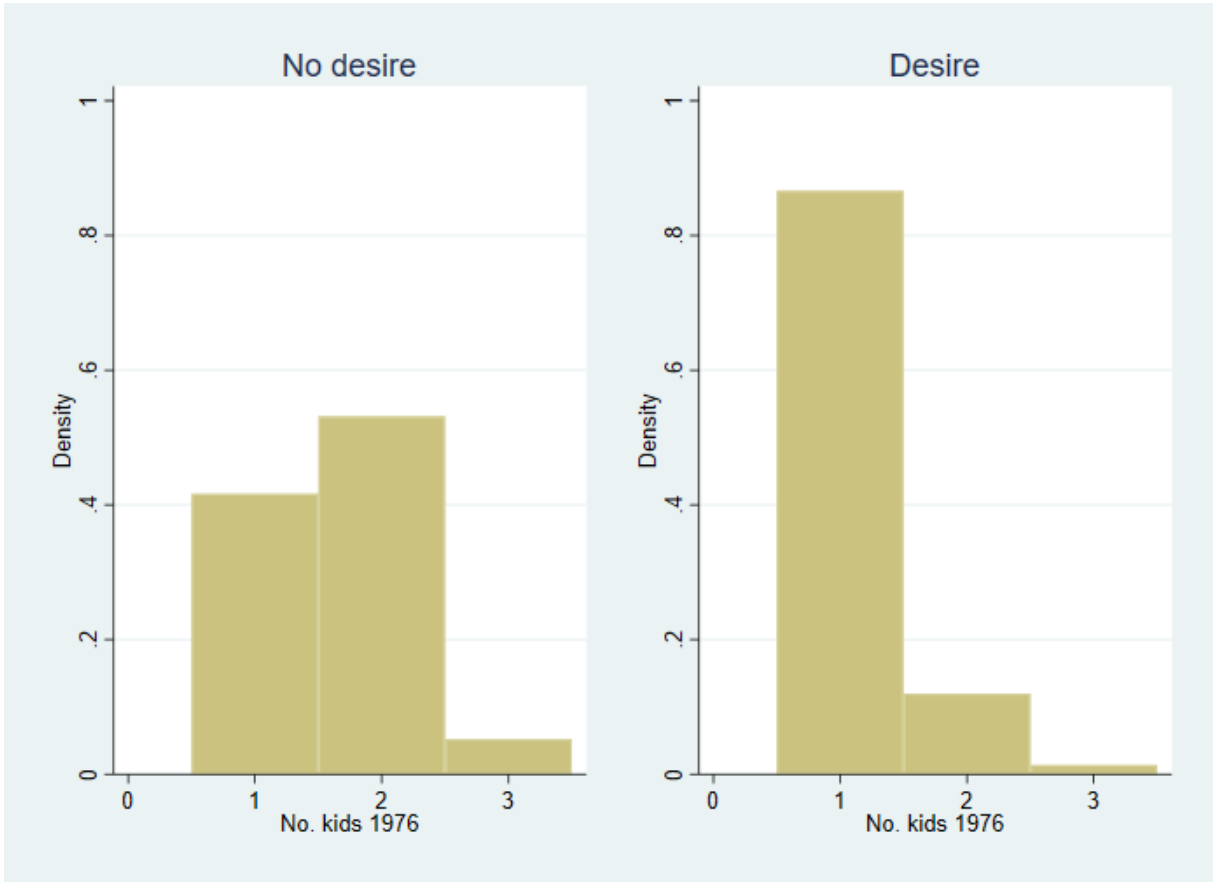
Note: Separate histograms are represented for early mothers with (285) and without (additional) desire (97).

family. Finally, 1/3 desire a large family. Among early mothers, 1/4 have no additional desire, a little less than 1/2 desire a small family and 1/3 desire a large family.

3.5.2 Distribution of desired family type and earnings potential

Considering next, how desired family type varies with individual earnings potential, Figure 4 shows the distribution of desired family type by ability and patience, where individual time preferences are split into two categories of high and low patience, and ability based on cognitive skills in adolescence is split by the median into high and low ability. Figure 4 is based on sample 2, excluding early mother with no desire, such that category 1 has a clean interpretation as no desire for a family.

From Figure 4 we see a strong 2-child norm dominating desires for all four combinations of patience and ability. Patience appears to be orthogonal to desired family type

Figure 3: Desired family type

as distributions comparing across are similar. Distributions of desired family type among individuals with higher ability are more spread out than distributions of individuals with lower ability. This is reflected in a higher percentage of women desiring either no family or a large family among individuals with above median ability.

Figure 5 illustrates the distribution of desired family type by expected level of secondary education. The distributions among individuals expecting to complete Lower or Upper Secondary Educations are more spread out similar to the pattern found for high ability individuals. The desire for 2-children is largest among individuals with lower educational expectations.

Finally, I consider the link between completed education and desired family type. From Figure 6, which shows the distribution of desired family type by level of education, we see that distributions of desired family type demonstrate a greater spread for individuals

Figure 4: Desired family type by ability and patience
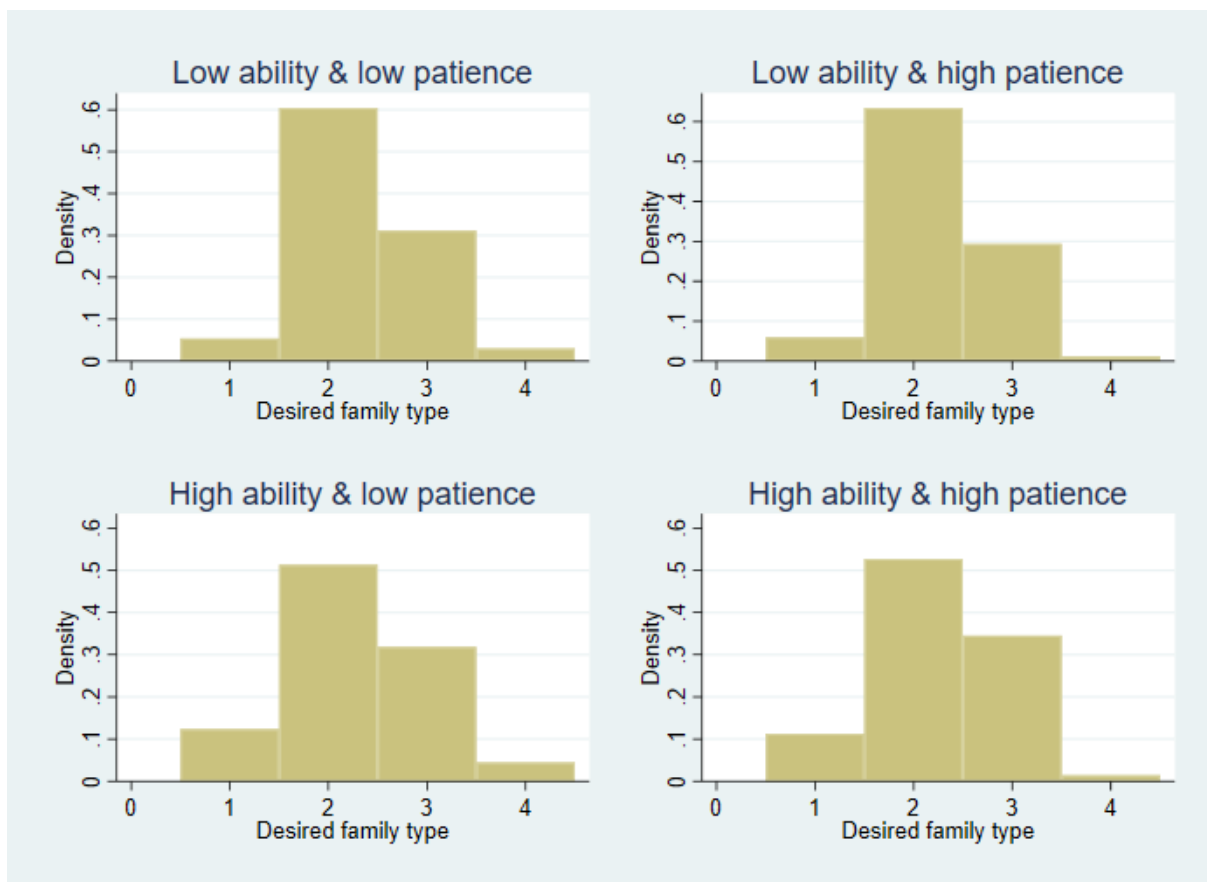
completing a Bachelor's or Master's degree, mirroring results for cognitive ability and educational aspirations. The percentage of individuals desiring no family is thus higher among individuals completing a university degree, particularly those completing a Master's degree (or above), relative to individuals with less education. Similarly, less educated women are relatively more likely to desire a small family in line with the two-child norm. Across all educational levels, the desire for a large family is most frequent among women completing a Bachelor's degree. Finally, women completing a Master's are more likely to answer that they *Don't know*, when asked about their fertility desires.

Increasing variation of desires in level of education, such that more educated women are relatively more likely to prefer both no family and a family of three or more children over a family of two children, is in line with findings of Heiland et al. (2005). The question is whether greater variation alone is responsible for earlier findings that individuals com-

Figure 5: Desired family type by expected education
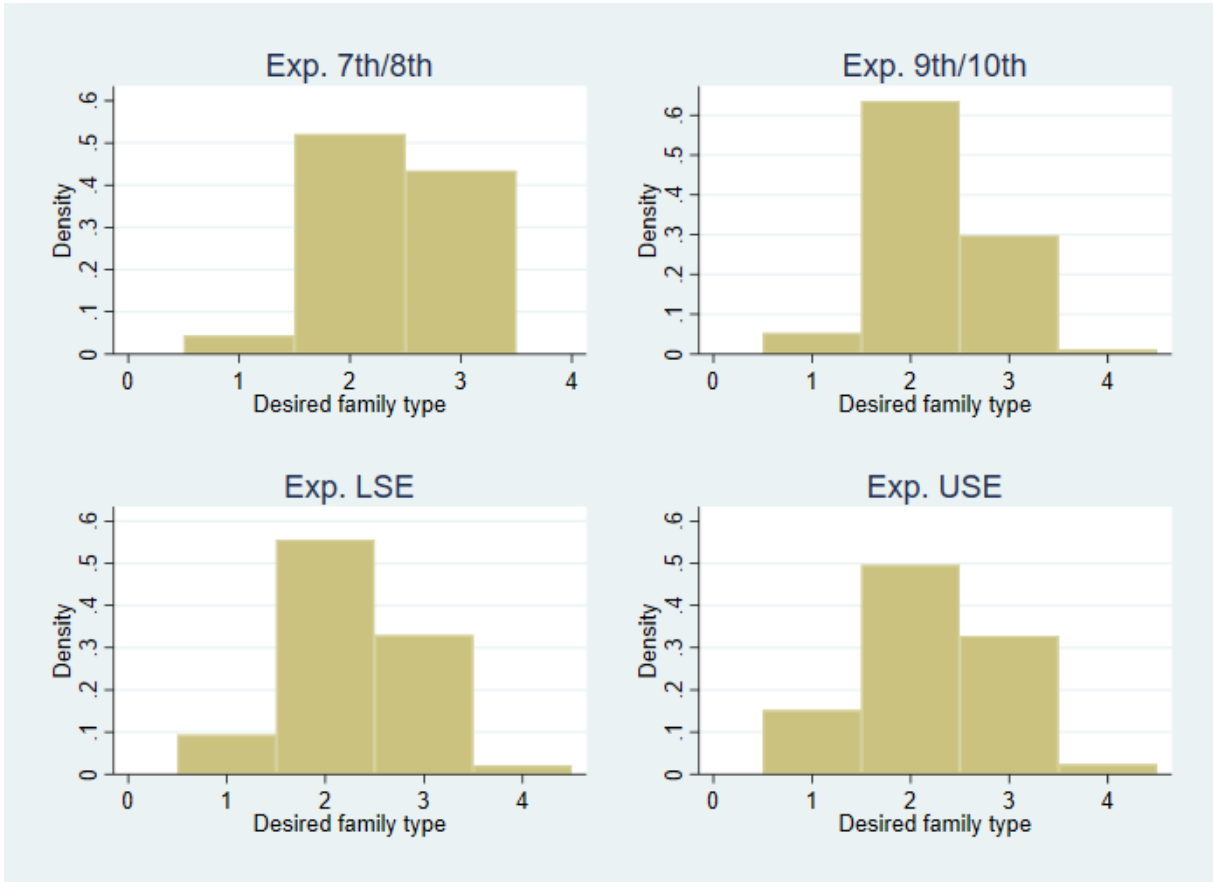
pleting relatively more education (Testa (2014); Iacovou and Tavares (2011)) are more likely to miss their fertility targets and ultimately have fewer children than less educated women.

Despite the fact that my measure of desires is elicited at age 22 after most respondents have finished their education, the evidence that measures of ability and educational expectations in adolescence correlate with greater variance in desired family type, lends evidence to the hypothesis that fertility desires are rather shaped by innate traits and preferences than by education.

3.5.3 Desired family status and relationship status at elicitation

As described in Section 3.2, desires for children may go hand in hand with a desire for being in a stable relationship such as marriage or living with a partner. It is therefore

Figure 6: Desired family type by completed level of education
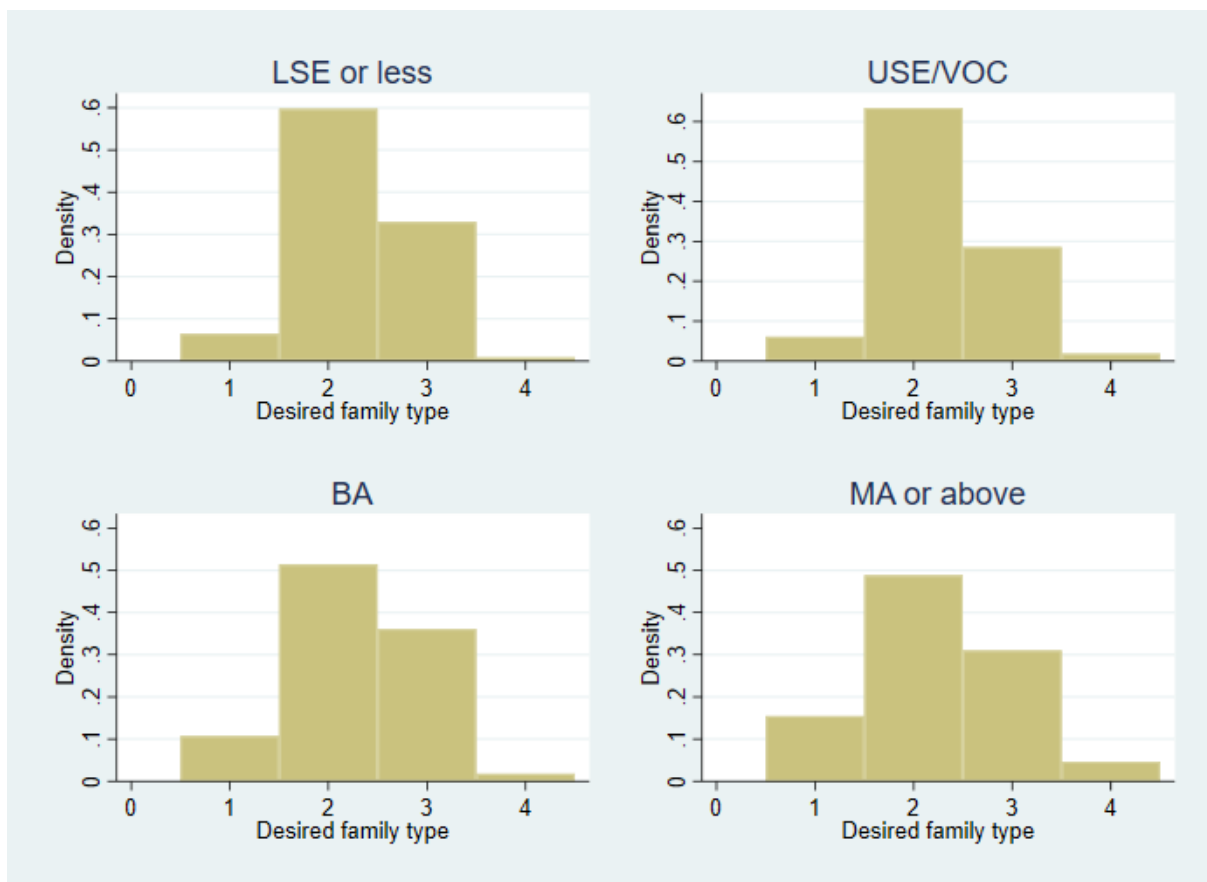
possible that fertility desires correlate with relationship status at elicitation. In Appendix A.4 distributions of desired family type by relationship status 3 years prior to elicitation (in 1973) for early mothers and other respondents demonstrate that distributions of desired family type are not increasing in stability of relationships 3 years prior to elicitation.

In Tables 3 and 2 distributions of desired family type by relationship status at the time of elicitation (1976) are shown for early mothers and other respondents, respectively.

By 1976, almost 90% of early mothers were married or cohabiting, while 6% had no partner. Among other respondents, 60% were married or cohabiting, 15% had a partner, while almost 1/4 had no partner. Table 3 shows that among early mothers, individuals with no partner are relatively more likely to have no additional desire, while married individuals are relatively more likely to desire a large family. Finally, cohabiting couples

Table 2: Desires and relationship of other respondents, 1976

Desired family type	No desire	Small family	Large family	Don't know
No partner	17.43	52.29	27.52	2.75
No wish to move in	23.53	58.82	17.65	0.00
Not consider move in	15.15	48.48	33.33	3.03
Consider move in	10.53	47.37	42.11	0.00
Consider marriage	6.98	54.65	37.21	1.16
Cohabiting	9.54	60.76	28.61	1.09
Married	6.40	58.72	33.72	1.16
Total	11.30	57.05	30.14	1.51

Note: Relationshipstatus was elicited in the same survey wave as fertility desires at age 22. 23% had no partner, 4% had no wish to move in with a partner, 4% had not considered to move in, 2% had considered to move in, 9% had considered marriage, 39% were cohabiting, and 18% were married.

Table 3: Desires and relationship of early mothers, 1976

Desired family type	No desire	Small family	Large family	Don't know
No partner	45.45	31.82	22.73	0.00
No wish to move in	20.00	40.00	40.00	0.00
Not consider move in	0.00	75.00	25.00	0.00
Consider move in	0.00	50.00	50.00	0.00
Consider marriage	42.86	42.86	14.29	0.00
Cohabiting	28.57	48.98	21.43	1.02
Married	22.22	43.21	32.10	2.47
Total	25.20	44.36	28.61	1.84

Note: Relationship status was elicited in the same survey wave as fertility desires at age 22. 6% had no partner, 1% had no wish to move in with a partner, 1% had not considered to move in, 1% had considered to move in, 2% had considered marriage, 26% were cohabiting, and 64% were married.

are relatively more likely to desire a small family. Desires are thus positively correlated with stability of relationships for early mothers at the time of elicitation.

For other respondents, Table 2 shows that individuals with no partner or with no wish to move in are relatively more likely to have no desire for a family, while the opposite is true for individuals, who are married, cohabiting or wishing to be so. Married and cohabiting individuals are relatively more likely to desire a small family, i.e. adhering to societal norms, while individuals desiring to establish a stable relationship are relatively more likely to desire a large family. This implies that overall desire for a family is positively correlated with being in or desiring to establish a stable relationship, while stability of relationships is negatively correlated with desired family size.

The systematic patterns between desired family type and relationship status at elicitation, combined with the fact that relationship status may directly affect the ability of individuals to realize family formation, including their timing of fertility, independently of desires, confirms relationship status at elicitation as a potential confounder. This calls for adding it among controls in the empirical investigation.

3.6   Descriptive statistics

I proceed with descriptive statistics on the socio-economic background of individuals in my sample, as well as on earnings potential and relationship status over adult life. I present descriptive statistics for the full sample and separately for early mothers and other respondents. The descriptives demonstrate that the subgroups differ significantly on socio-economic background and earnings potential.

3.6.1   Socio-economic background

From Table 4, we see that close to 85% of individuals are born in 1954, while almost all are born between 1953 and 1955. This implies that their age is between 13 and 15 years at the first DLSY wave in 1968. By 2017, 10% have died. Regarding childhood conditions, more than 85% of individuals have had a stable childhood growing up with both parents. 66% are first or second born- On average individuals have 2.2 siblings, though 1/3 have 3 or more. Half of individuals come from a family with an annual breadwinner income of 20.000 to 39.000 DKK in 1967, one year prior to the first survey wave in 1968, while close to 1/4 of families had a lower income and 1/4 had a higher income. Close to 5% of respondents come from a family with no breadwinner income in the past year.

From Table 5, we see that almost all individuals have working fathers, primarily working as unskilled workers, skilled workers, white collar employees and farmers. 1/4 of fathers have subordinates. Around 2/5 of individuals have working mothers, close to half have stay-at-home mothers, while 7% have mothers that are employed as assisting spouses. Finally, 5% of individuals have no mother. Working mothers typically hold unskilled jobs or white collar jobs, while around 17 % of mothers have subordinates. From Table 5 we see that a majority of respondents' parents have completed only obligatory schooling (up to 8th grade). This is true for 70-75% of parents, while 13-14% have taken a middle

Table 4: Descriptives, Sample 1

Variable	Mean	Std.	N
Birth year			
Born 1952	0.002	0.039	1312
Born 1953	0.097	0.296	1312
Born 1954	0.841	0.365	1312
Born 1955	0.059	0.237	1312
Born 1956	0.001	0.028	1312
Respondent died	0.095	0.293	1312
Stable upbringing*	0.864	0.343	1312
Siblings			
No. of siblings	2.245	1.418	1258
0-2 sibl.	0.614	0.487	1312
3-6 sibl.	0.345	0.476	1312
No resp.	0.041	0.199	1312
Parity			
Parity	2.195	1.329	1256
1st/2nd born	0.659	0.474	1312
Born 3rd or later	0.341	0.474	1312
Household income (1967)			
0 DKK	0.046	0.211	1312
1-9.000 DKK	0.020	0.139	1312
10-19.000 DKK	0.147	0.354	1312
20-29.000 DKK	0.271	0.445	1312
30-39.000 DKK	0.239	0.427	1312
40-49.000 DKK	0.144	0.351	1312
50-59.000 DKK	0.046	0.211	1312
60-69.000 DKK	0.031	0.174	1312
70-79.000 DKK	0.019	0.137	1312
80-89.000 DKK	0.008	0.091	1312
90-99.000 DKK	0.004	0.062	1312
No response	0.023	0.150	1312

Note: *Living with both parents in 1968.

school exam or completed either LSE or USE. Regarding post-secondary education, 60% of fathers have no vocational education, 1/4 of fathers have (some) vocational education, and only 7% have completed either USE or hold a University degree. Half of mothers have no vocational education, 1/3 have (some) vocational education, while only 7% have completed USE or hold a University degree.

Splitting the sample into early mothers and other respondents, reveals large differences in socio-economic background. From Table 6 we see that early mothers are significantly less likely to have had a stable upbringing than other respondents are. They come from larger families, are typically born later into their families, and their families are less likely

Table 5: Descriptives on Parents, Sample 1

Variable	Mean	Std.	N
Father's occ.			
NLF	0.014	0.120	1312
Unskill.	0.202	0.402	1312
Skilled	0.104	0.306	1312
White collar	0.289	0.453	1312
S.e. farmer	0.231	0.422	1312
S.e. crafts	0.059	0.237	1312
S.e. busi./proff.	0.075	0.264	1312
No resp.	0.024	0.154	1312
Father has subord.	0.562	0.496	1312
Father employed	0.986	0.120	1312
Father's educ.			
7/8th grade	0.620	0.485	1312
Mid. school	0.101	0.301	1312
LSE/USE	0.128	0.334	1312
Miss./no resp.	0.151	0.358	1312
Father's voc. educ.			
None	0.601	0.490	1156
Voc. educ.	0.312	0.464	1156
USE/degree	0.067	0.249	1156
No resp.	0.020	0.140	1156
Mother's empl.			
Not empl.	0.456	0.498	1312
Empl.	0.418	0.493	1312
Ass. spouse	0.069	0.254	1312
Dead/no resp.	0.057	0.232	1312
Mother's occ.			
NLF	0.498	0.500	1312
Unskill.	0.261	0.440	1312
Skilled	0.003	0.055	1312
White collar	0.209	0.407	1312
S.e. busi/proff	0.007	0.083	1312
S.e. crafts	0.003	0.055	1312
Dead/No resp.	0.019	0.137	1312
Mother has subord.	0.173	0.378	1312
Mother's educ.			
7/8th grade	0.651	0.477	1312
Mid. school	0.108	0.311	1312
LSE/USE	0.136	0.343	1312
Miss./no resp.	0.105	0.307	1312
Mother's voc. educ.			
None	0.502	0.500	1156
Voc. educ	0.339	0.474	1156
USE/degree	0.067	0.251	1156
No resp.	0.092	0.289	1156

to have had annual incomes above 50.000 DKK in the past year. From Table 7 we see

Table 6: Descriptives, early mother versus other

Variable	Mean	Std.	N	Mean	Std.	N	Diff.	P-value
Birth year								
Born 1952	0.001	0.033	931	0.003	0.051	381	-0.002	(0.585)
Born 1953	0.077	0.267	931	0.144	0.352	381	-0.067***	(0.001)
Born 1954	0.851	0.357	931	0.819	0.386	381	0.032	(0.166)
Born 1955	0.070	0.255	931	0.034	0.182	381	0.036**	(0.004)
Born 1956	0.001	0.033	931	0.000	0.000	381	0.001	(0.318)
Respondent died	0.088	0.284	931	0.110	0.314	381	-0.022	(0.233)
Stable upbring.*	0.876	0.329	931	0.832	0.374	381	0.044*	(0.044)
Siblings								
No. of sibl.	2.154	1.351	896	2.470	1.551	362	-0.316***	(0.001)
0-2 sibl.	0.644	0.479	931	0.538	0.499	381	0.106***	(0.000)
3-6 sibl.	0.318	0.466	931	0.412	0.493	381	-0.094**	(0.001)
No resp.	0.038	0.190	931	0.050	0.218	381	-0.012	(0.338)
Parity								
Parity	2.156	1.292	893	2.292	1.413	363	-0.136	(0.113)
1st/2nd born	0.677	0.468	931	0.617	0.487	381	0.060*	(0.041)
3rd or later	0.323	0.468	931	0.383	0.487	381	-0.060*	(0.041)
Household income(1967)								
0 DKK	0.052	0.221	931	0.034	0.182	381	0.017	(0.140)
1-9.000 DKK	0.023	0.149	931	0.013	0.114	381	0.009	(0.215)
10-19.000 DKK	0.134	0.341	931	0.178	0.383	381	-0.044	(0.051)
20-29.000 DKK	0.253	0.435	931	0.315	0.465	381	-0.061*	(0.027)
30-39.000 DKK	0.230	0.421	931	0.262	0.441	381	-0.033	(0.218)
40-49.000 DKK	0.148	0.356	931	0.134	0.341	381	0.014	(0.494)
50-59.000 DKK	0.054	0.226	931	0.029	0.168	381	0.025*	(0.029)
60-69.000 DKK	0.038	0.190	931	0.016	0.125	381	0.022*	(0.015)
70-79.000 DKK	0.024	0.152	931	0.008	0.089	381	0.016*	(0.019)
80-89.000 DKK	0.012	0.108	931	0.000	0.000	381	0.012***	(0.001)
90-99.000 DKK	0.004	0.065	931	0.003	0.051	381	0.002	(0.622)
No response	0.029	0.168	931	0.008	0.089	381	0.021**	(0.003)
Observations	931			381			1312	

Note: *Living with both parents in 1968. ***/**/* indicate statistical difference at the 1%/5%/10%-level. P-values in parentheses.

that parents of early mothers are significantly less educated with 70% having completed only obligatory schooling, while parents of other respondents are significantly more likely to have completed LSE, USE, VOC or to hold a University degree. In addition, early mothers are significantly more likely to have had working mothers (48% vs. 39%) and parents employed as unskilled labor (unskilled father 30% vs. 16%). Fathers of early mothers are significantly less likely to be self-employed farmers, business professionals, and white-collar employees. In addition, fathers of early mothers are less likely to have subordinates.

Table 7: Descriptives on Parents, early mother versus other

Variable	Mean	Std.	N	Mean	Std.	N	Diff.	P-value
Father's occ.								
NLF	0.013	0.113	931	0.018	0.134	381	-0.005	(0.483)
Unskill.	0.159	0.366	931	0.307	0.462	381	-0.148***	(0.000)
Skilled	0.092	0.290	931	0.134	0.341	381	-0.041*	(0.037)
White collar	0.328	0.470	931	0.194	0.396	381	0.133***	(0.000)
S.e. farmer	0.236	0.425	931	0.218	0.413	381	0.018	(0.467)
S.e. crafts	0.059	0.236	931	0.060	0.238	381	-0.001	(0.929)
S.e. busi./proff.	0.090	0.287	931	0.039	0.195	381	0.051***	(0.000)
No resp.	0.023	0.149	931	0.029	0.168	381	-0.006	(0.523)
Father has subord.	0.575	0.495	931	0.530	0.500	381	0.044	(0.143)
Father employed	0.987	0.113	931	0.982	0.134	381	0.005	(0.483)
Father's educ.								
7/8th grade	0.591	0.492	931	0.693	0.462	381	-0.102***	(0.000)
Mid. school	0.113	0.316	931	0.071	0.257	381	0.042*	(0.013)
LSE/USE	0.158	0.365	931	0.055	0.229	381	0.103***	(0.000)
Miss./no resp.	0.139	0.346	931	0.181	0.386	381	-0.043	(0.062)
Father's voc. edu.								
None	0.560	0.497	836	0.709	0.455	320	-0.150***	(0.000)
Voc. educ	0.333	0.471	836	0.259	0.439	320	0.073*	(0.013)
USE/degree	0.084	0.277	836	0.022	0.147	320	0.062***	(0.000)
No resp.	0.024	0.153	836	0.009	0.097	320	0.015	(0.054)
Mother's empl.								
Not empl.	0.476	0.500	931	0.407	0.492	381	0.069*	(0.022)
Empl.	0.391	0.488	931	0.483	0.500	381	-0.092**	(0.002)
Ass. spouse	0.078	0.269	931	0.047	0.212	381	0.031*	(0.026)
Dead/no resp.	0.055	0.228	931	0.063	0.243	381	-0.008	(0.572)
Mother's occ.								
NLF	0.516	0.500	931	0.454	0.499	381	0.062*	(0.043)
Unskill.	0.230	0.421	931	0.339	0.474	381	-0.109***	(0.000)
Skilled	0.002	0.046	931	0.005	0.072	381	-0.003	(0.439)
White collar	0.224	0.417	931	0.171	0.377	381	0.054*	(0.023)
S.e. busi/proff	0.008	0.086	931	0.005	0.072	381	0.002	(0.627)
S.e. crafts	0.001	0.033	931	0.008	0.089	381	-0.007	(0.145)
Dead/No resp.	0.019	0.138	931	0.018	0.134	381	0.001	(0.907)
Mother has subord.	0.172	0.377	931	0.176	0.381	381	-0.004	(0.863)
Mother's educ.								
7/8th grade	0.629	0.483	931	0.703	0.457	381	-0.074**	(0.009)
Mid. school	0.114	0.318	931	0.094	0.293	381	0.019	(0.289)
LSE/USE	0.166	0.373	931	0.060	0.238	381	0.106***	(0.000)
Miss./no resp.	0.090	0.287	931	0.142	0.349	381	-0.052*	(0.011)
Mother's voc. educ.								
None	0.462	0.499	836	0.606	0.489	320	-0.145***	(0.000)
Voc. educ	0.373	0.484	836	0.250	0.434	320	0.123***	(0.000)
USE/degree	0.084	0.277	836	0.025	0.156	320	0.059***	(0.000)
No resp.	0.081	0.274	836	0.119	0.324	320	-0.037	(0.068)
Observations	931			381			1312	

Note: ***/**/* indicate statistical difference at the 1%/5%/10%-level. P-values in parentheses.

The descriptives above demonstrate that the socio-economic backgrounds of early mothers and other respondents differ quite dramatically.

3.6.2 Individual earnings potential

Table 8 presents descriptives on factors related to the earnings potential of individuals. From Table 8 we see that the distribution of respondents' own expectations regarding attainment of secondary education is very similar to the distribution of parental expectations of their children's educational attainment elicited one year later. This indicates a high degree of alignment among individuals' educational aspirations and parental expectations. Almost 5% of respondents expect to complete less than obligatory schooling achieving only 7th or 8th grade, 37% expect to complete obligatory schooling (achieving 9th or 10th grade), 35% expect to complete LSE, and 1/5 expect to complete USE. Finally, 4% answer that they do not know. As parental expectations are available for a reduced selection of individuals (1156 vs. 1312 individuals), I mainly rely on own educational expectations in the empirical analysis. Complementing educational expectations, individuals in my sample completed three ability tests on inductive, verbal and spatial ability, respectively. The score on the inductive test has been found to correlate significantly with later life economic outcomes, such as income in Epper et al. (2020), and as such is a proxy for individual cognitive ability. The average score on the inductive test is 23, while the standardized test score is 0.045. The average verbal test score is 37, while the standardized score is 0.061. Finally, the average spatial score is 22, while the standardized score is -0.06. Regarding individual time preferences elicited at age 19, 1/4 of individuals in the sample are categorized as non-patient, 1/2 are classified as patient, and finally 1/4 are classified as very patient. In addition, around age 19, individuals completed Rosenberg's self-esteem test. The standardized score for individuals in my sample is -0.19.

Comparing early mothers and other respondents, from Table 9 we see that early mothers expect to complete significantly less secondary education than other respondents, and this is mirrored by lower parental expectations with regard to completion of secondary education. Almost 2/3 of early mothers expect to complete 10th grade or less secondary

Table 8: Earnings potential, Sample 1

Variable	Mean	Std.	N
Own expectations			
Exp. 7th/8th	0.046	0.211	1312
Exp. 9th/10th	0.367	0.482	1312
Exp. LSE	0.348	0.477	1312
Exp. USE	0.195	0.396	1312
No resp.	0.043	0.202	1312
Cognitive ability			
Inductive score	23.029	8.112	1275
Std. cog. skills	0.045	0.956	1275
Verbal score	37.291	7.547	1274
Std. verb. skills	0.061	0.932	1274
Spatial score	22.436	6.872	1274
Std. spa. skills	-0.060	0.934	1274
Time preferences			
Not patient*	0.248	0.432	1312
Patient**	0.507	0.500	1312
Very patient***	0.226	0.419	1312
Std. rosen score	-0.193	1.025	1284
Parental expectations			
Left school	0.031	0.174	1156
Expect 8th	0.030	0.171	1156
Expect 9th	0.159	0.366	1156
Expect 10th	0.199	0.399	1156
Exp. LSE	0.369	0.483	1156
Exp. USE	0.204	0.403	1156

*Note: *Not patient=What offer would you choose: Average salary from the start, **Patient=What offer would you choose: Low salary first 2 years, then higher, ***Very patient=What offer would you choose: Very low salary first 4 years, then very high.****Rosenbergs standardized score.*

education, while this is only true for 1/3 of other respondents. Moreover, early mothers score significantly lower on all ability tests and on Rosenberg's self-esteem test. Finally, they are significantly more likely to be impatient. To sum up, individuals with early childbearing are characterized by lower innate ability, patience and educational aspirations mirroring theirs parent's expectations than other respondents. This is in line with finding in Rosenbaum (2015) and Epper et al. (2020).

3.6.3   Relationship Factors

Regarding relationship factors, I focus on the full sample as differences between early mothers and other respondents were described in Section 3.5.3. From Table 10 we see that overall, 1/3 of individuals have no partner in 1973, 1/2 of individuals have a partner,

Table 9: Earnings potential, early mother versus other

Variable	Mean	Std.	N	Mean	Std.	N	Diff.	P-value
Own expectations								
Exp. 7th/8th	0.032	0.177	931	0.081	0.274	381	-0.049**	(0.001)
Exp. 9th/10th	0.311	0.463	931	0.504	0.501	381	-0.192***	(0.000)
Exp. LSE	0.381	0.486	931	0.268	0.443	381	0.114***	(0.000)
Exp. USE	0.236	0.425	931	0.094	0.293	381	0.142***	(0.000)
No resp.	0.039	0.193	931	0.052	0.223	381	-0.014	(0.291)
Cognitive ability								
Inductive score	24.038	7.870	911	20.503	8.170	364	3.536***	(0.000)
Std. cog. skills	0.164	0.927	911	-0.253	0.963	364	0.417***	(0.000)
Verbal score	38.262	7.232	909	34.874	7.780	365	3.388***	(0.000)
Std. verb. skills	0.181	0.893	909	-0.238	0.961	365	0.418***	(0.000)
Spatial score	22.968	6.675	910	21.104	7.178	364	1.864***	(0.000)
Std. spa. skills	0.012	0.907	910	-0.241	0.975	364	0.253***	(0.000)
Time preferences								
Not patient*	0.212	0.409	931	0.339	0.474	381	-0.127***	(0.000)
Patient**	0.524	0.500	931	0.465	0.499	381	0.060	(0.050)
Very patient***	0.244	0.430	931	0.184	0.388	381	0.060*	(0.014)
Std. rosen score	-0.144	1.020	915	-0.315	1.029	369	0.171**	(0.007)
Parental expectations								
Left school	0.012	0.109	836	0.081	0.274	320	-0.069***	(0.000)
Expect 8th	0.012	0.109	836	0.078	0.269	320	-0.066***	(0.000)
Expect 9th	0.136	0.343	836	0.219	0.414	320	-0.082**	(0.002)
Expect 10th	0.189	0.392	836	0.225	0.418	320	-0.036	(0.183)
Exp. LSE	0.400	0.490	836	0.291	0.455	320	0.109***	(0.000)
Exp. USE	0.244	0.430	836	0.100	0.300	320	0.144***	(0.000)
Observations	931			381			1312	

Note: ***/**/* indicate statistical difference at the 1%/5%/10%-level. P-values in parentheses. *Not patient=What offer would you choose: Average salary from the start, **Patient=What offer would you choose: Low salary first 2 years, then higher, ***Very patient=What offer would you choose: Very low salary first 4 years, then very high.****Rosenbergs standardized score.

and 15% of respondents are already married. Three years later at elicitation of desires, 1/5 still have no partner, 15% have a partner, and more than 2/3 are married or cohabiting. 1/3 of individuals with a partner have no plans to move in.

Over adult life, practically all individuals enter stable relationships. 93% end up marrying, while only 1% never enter a stable relationship. Among individuals with one or more stable relationships, almost 2/3 have just one stable relationship over adult life, 1/4 have two stable relationships, and 12% have three or more. On average 2/5 of individuals experience a divorce or split up a cohabitation over adult life. Age at first cohabitation or marriage is 21 years, while age at first marriage is 23.5 years on average. Average age at

first break-up is almost 32 years, while average age at first divorce is 36 years. Particular ages of matching and breaking-up are likely to be specific to the cohort in question.

Table 10: Relationship status, Sample 1

Variable	Mean	Std.	N
Relationship, 1973			
Marr./cohab	0.153	0.360	1312
Partner	0.501	0.500	1312
No partner	0.343	0.475	1312
Relationship status I, 1976			
No relationship	0.183	0.387	1312
Going steady	0.123	0.328	1312
Engaged	0.022	0.147	1312
Live with a partner	0.354	0.479	1312
Married	0.316	0.465	1312
Relationship status II, 1976			
No partner	0.183	0.387	1312
No wish move-in	0.030	0.170	1312
No consid. move-in	0.028	0.166	1312
Consid. move-in	0.016	0.126	1312
Consid. marr.	0.071	0.257	1312
Cohab.	0.354	0.479	1312
Marr.	0.316	0.465	1312
No. of relationships			
Never rela.	0.011	0.104	1277
1 ever	0.606	0.489	1124
2 ever	0.260	0.439	1124
3+ ever	0.116	0.320	1124
Total number of marr.	1.015	0.582	1124
Total number of cohab.	0.502	0.805	1124
Ever marr./cohab.	0.975	0.157	1312
Ever married	0.932	0.253	1271
Ever divorce/separ.	0.377	0.485	1312
Ever broke up cohab.	0.213	0.409	1312
Ever widow	0.028	0.166	1312
Timing			
Age at first marriage	23.556	7.345	1169
Age of first cohab or marriage	21.553	4.721	1238
Age of first divorce	36.227	10.616	494
Age 1st breakup (marr./cohab)	31.778	10.009	662

Note: *Splitting up includes divorces and separations.

In this section, I presented descriptive statistics on socioeconomic background, individual earnings potential and relationship factors. I proceed with descriptives on my outcomes measures.

4 Outcomes

I now present my outcome measures, including descriptive statistics for the full sample and split by early mothers and other respondents. I document that outcomes of early mothers and other respondents differ significantly. This reflects differences in socio-economic background and earnings potential as demonstrated in Section 3.6.

4.1 Family Formation

I wish to test two hypotheses on family formation, namely that stronger fertility desires are associated with lower age at first birth and increased levels of realized fertility and that women with a higher earnings potential face a higher risk of experiencing fertility deficits, i.e. of realizing fertility below desires. I therefore consider the following outcomes in my empirical analysis: Age at first birth, realized fertility, i.e. the number of children a woman gave birth to, and dummies for experiencing a fertility deficit and for remaining childless. The fertility outcomes are based on the Medical Birth Register, which starts in 1973, when respondents were 19 years old on average. For earlier births, I rely on survey evidence. I present descriptives on family formation outcomes comparing early mothers and other respondents, keeping in mind that family formation of early mothers takes place earlier than that of other respondents per definition. In fact from Table 11, we see that there are significant differences on most parameters of realized fertility, when comparing the two subgroups.

Early mothers have their first birth at age 20 and their last at age 27 on average, while other respondents, conditional on motherhood, have their first birth at age 27 and their last at age 31 on average. This reflects both tighter spacing of childbearing among other respondents, as well as intensive margin effects as early mothers end up having more children than other respondents on average. Early mothers give birth to 2.4 children on average, while other respondents give birth to 1.6 children on average. 42 pct. of early mothers graduate from their highest completed education after having had their first child. This is true for 1/5 of other respondents, implying that pursuing family and education in parallel is quite common. 28% of early mothers have ended their childbearing prior to final graduation, while this is true for 17% of other respondents. In addition, 3/4 of

early mothers enter the labor market after initiating childbearing and 1/4 have completed childbearing prior to labor market entry. Thus, labor market entry and childbearing take place in parallel. Only 5% of other respondents enter the labor market after giving birth to their first child and only 2% have completed childbearing prior to entry. This indicates that other respondents predominantly follow a sequential pattern of education, entry to the labor market and then family formation.

Overall, 83% of other respondents end up as parents, while 17.3% end up childless. Despite these high probabilities of parenthood, almost half of other respondents (47%) end up with fewer children than desired, and if considering only individuals desiring a family, more than half (52%) end up with fewer children than desired. The high share of individuals experiencing a deficit among other respondents reflects both extensive margin effects, i.e. 15% end up childless despite having positive desires, and intensive margin effects, i.e. many women have fewer children than desired. Most noticeable, despite practically no one desiring just one child, almost 20% of other respondents end up having just one child. Only 1/3 of other respondents have as many children as they desired (37% of women desiring children), while 1/6 end up with more children than desired (10.1% of women desiring children). Among early mothers with desire, 1/3 of individuals realize fertility below desires, 2/5 have as many children as they desired, while 1/4 have more children than desired.

4.2   Education

Regarding education, I am interested in testing the hypothesis that desired family type has no significant impact on educational attainment, but influences the individual's choice of occupation as proxied by field of study due to beliefs about career-specific childrearing compatibility. In order to evaluate this hypothesis, I consider four outcomes, namely level of completed secondary education (SE), a dummy for completion of Upper Secondary Education (USE), level of highest completed education and field of study for highest completed education.

I base level of secondary education on *Type of secondary education* from the administrative registers on completed education (UDDA). The variable is treated as a continuous

Table 11: Family formation, early mother versus other

Variable	Mean	Std.	N	Mean	Std.	N	Diff.	P-value
Desire for children								
No desire	0.114	0.318	931	0.252	0.435	381	-0.138***	(0.000)
Desire	0.886	0.318	931	0.748	0.435	381	0.138***	(0.000)
Don't know	0.010	0.098	931	0.010	0.102	381	-0.001	(0.892)
Desires								
1 child	0.028	0.165	931	0.010	0.102	381	0.017*	(0.021)
2 children	0.541	0.499	931	0.433	0.496	381	0.108***	(0.000)
3 children	0.253	0.435	931	0.218	0.413	381	0.036	(0.163)
4 or more	0.047	0.212	931	0.068	0.252	381	-0.021	(0.154)
Don't know	0.026	0.159	931	0.018	0.134	381	0.007	(0.391)
Desired family type								
No desire	0.114	0.318	931	0.252	0.435	381	-0.138***	(0.000)
Small family	0.569	0.495	931	0.444	0.497	381	0.126***	(0.000)
Large family	0.301	0.459	931	0.286	0.453	381	0.015	(0.596)
Don't know	0.026	0.159	931	0.018	0.134	381	0.007	(0.391)
Fertility								
Parent in 1973	0.000	0.000	931	0.286	0.453	381	-0.286***	(0.000)
Ever parent	0.827	0.378	931	1.000	0.000	381	-0.173***	(0.000)
No. of childr.	1.648	1.030	931	2.417	1.024	381	-0.770***	(0.000)
0 kids	0.173	0.378	931	0.000	0.000	381	0.173***	(0.000)
1 kid	0.203	0.402	931	0.142	0.349	381	0.061**	(0.006)
2 kids	0.466	0.499	931	0.478	0.500	381	-0.012	(0.705)
3 kids	0.126	0.332	931	0.249	0.433	381	-0.124***	(0.000)
4+ kids	0.032	0.177	931	0.131	0.338	381	-0.099***	(0.000)
Surplus/gaps								
Fertility>desire	0.162	0.369	931	0.192	0.394	381	-0.029	(0.212)
Fertility=desire	0.364	0.481	931	0.430	0.496	381	-0.066*	(0.027)
Fertility<desire	0.474	0.500	931	0.378	0.486	381	0.096**	(0.001)
Timing								
Age 1st birth	27.359	4.366	765	20.087	1.375	381	7.273***	(0.000)
Age at last birth	31.303	4.704	765	26.966	5.501	381	4.337***	(0.000)
Year of first birth	1981	4.380	765	1974	1.457	381	7.035***	(0.000)
Year of last birth	1985	4.722	765	1980	5.480	381	4.446***	(0.000)
Marr./cohab pre SEgrad	0.028	0.165	931	0.042	0.201	381	-0.014	(0.227)
Marr./cohab pre grad	0.192	0.394	901	0.288	0.454	364	-0.096***	(0.000)
SE-grad>B1	0.021	0.145	931	0.063	0.243	381	-0.042**	(0.002)
Final grad.>B1	0.207	0.406	931	0.423	0.495	381	-0.215***	(0.000)
Final grad.>Blast	0.169	0.375	931	0.281	0.450	381	-0.112***	(0.000)
Entry*>B1	0.047	0.212	931	0.769	0.422	381	-0.722***	(0.000)
Entry*>Blast	0.019	0.138	931	0.276	0.447	381	-0.256***	(0.000)
Observations	931			381			1312	

Note: ***/**/* indicate statistical difference at the 1%/5%/10%-level. P-values in parentheses. *Entry refers to entry to the labor market after first or last birth.

variable and it includes seven steps, *7th, 8th, 9th, 10th, Higher Preparatory Exam (HPE), USE (Languages), USE (Math)*. The dummy on completion of USE takes the value one for individuals, who completed an HPE exam or USE exam. Level of highest completed

education is also taken from the education register. It is treated as a continuous variable and includes five steps, namely *Less than USE, Vocational education (or USE), Short Tertiary Education (STE), BA, MA and above.* Field of study for highest completed education is taken from the registers and contains 19 fields, when the category *missing* is included.[14]

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[14]Fields with less than 0.5% of the sample have been omitted from Tables 12 and 14.

Table 12: Descriptives on Education, Sample 1

Variable	Mean	Std.	N
DLSY			
Secondary educ. 1976			
Still in school	0.016	0.126	1312
7th/8th	0.069	0.254	1312
9th	0.147	0.354	1312
10th	0.215	0.411	1312
LSE	0.300	0.458	1312
USE/HPE no exam	0.012	0.110	1312
USE/HPE exam	0.238	0.426	1312
Miss./other	0.004	0.062	1312
Att. post-SE, 1976			
Not att. post-SE	0.716	0.451	1312
Att. post-SE	0.284	0.451	1312
Compl. post-SE	0.471	0.499	1312
Registers			
Timing			
Grad year, SE	1971.556	2.180	1292
Grad year, post-SE	1982.685	10.234	854
Final grad. year	1978.721	10.054	1292
Dummy for USE	0.241	0.428	1312
Level			
Less than USE	0.312	0.463	1292
USE or VOC	0.325	0.469	1292
Short tertiary	0.037	0.189	1292
BA or similar	0.263	0.441	1292
Master or above	0.063	0.243	1292
Miss.	0.015	0.123	1312
Type, SE			
7th/8th	0.108	0.311	1312
9th/10th	0.636	0.481	1312
HPE	0.064	0.245	1312
STX/languages	0.125	0.331	1312
USE/math	0.052	0.222	1312
Miss.	0.015	0.123	1312
Field of study*			
Agri/fores./fishing	0.005	0.067	1312
Arts	0.010	0.099	1312
Tech/media prod.	0.005	0.067	1312
Build./civil engin.	0.012	0.110	1312
Bus./admin./law	0.189	0.392	1312
Engin./techn.	0.011	0.106	1312
Human sciences	0.029	0.168	1312
Primary school	0.307	0.461	1312
Services	0.011	0.106	1312
Social and health	0.297	0.457	1312
Social sciences	0.017	0.128	1312
Teach. and learn.	0.053	0.225	1312
USE	0.027	0.163	1312
Missing	0.015	0.123	1312

Note: * Fields with less than .5% have been left out.

Table 12 contains descriptives on educational outcomes for the full sample based on survey data and register data. At age 22, individuals were asked, which secondary education they had completed. According to the survey data only 1.6 % of individuals were still attending some secondary education, while 2/5 had completed 7th-10th grade, 1/3 had completed Lower Secondary Education (LSE), and 24% had completed Upper Secondary Education (USE). Around 1/4% of individual were still attending some post-secondary education at age 22, while 1/2 of individuals had already completed their post-secondary education.

From the registers, I find that 1/10 of individuals left school after 7th or 8th grade, while 63.6% completed 9th or 10th grade, 6.4% completed Higher Preparatory Exam (HPE), 12.5% completed a USE in Social sciences and Languages, while 5.2% completed a Scientific or Mathematical USE. This is reflected in 1/4 having completed USE in total. Regarding level of highest completed education, almost 1/3 completed less schooling than USE, almost 1/3 completed vocational education or USE, 1/4 completed a Bachelor's degree, and only 6.3% completed a Master's degree or a higher level of education. Respondents graduated from secondary education around 1971 at age 17 (median), while respondents, who went on to post-secondary education graduated around 1979 (median graduation year) at age 25.

Table 13: Descriptives on Education, early mother versus other

Variable	Mean	Std.	N	Mean	Std.	N	Diff.	P-value
DLSY								
Secondary educ. 1976								
Still in school	0.014	0.117	931	0.021	0.144	381	-0.007	(0.357)
7th/8th	0.033	0.180	931	0.157	0.365	381	-0.124***	(0.000)
9th	0.111	0.314	931	0.236	0.425	381	-0.126***	(0.000)
10th	0.204	0.403	931	0.241	0.429	381	-0.037	(0.135)
LSE	0.314	0.464	931	0.265	0.442	381	0.049	(0.081)
USE no exam	0.014	0.117	931	0.008	0.089	381	0.006	(0.362)
USE	0.308	0.462	931	0.066	0.248	381	0.243***	(0.000)
Miss./other	0.002	0.046	931	0.008	0.089	381	-0.006	(0.127)
Att. post-SE, 1976								
Not att. post-SE	0.640	0.480	931	0.900	0.300	381	-0.260***	(0.000)
Att. post-SE	0.360	0.480	931	0.100	0.300	381	0.260***	(0.000)
Compl. post-SE	0.505	0.500	931	0.388	0.488	381	0.116***	(0.000)
Registers								
Timing								
Grad year, SE	1971	2.229	916	1970	1.913	376	0.893***	(0.000)
Grad year, post-SE	1981	9.371	656	1985	12.287	198	-3.704***	(0.000)
Dummy for USE	0.306	0.461	931	0.081	0.274	381	0.225***	(0.000)
Level								
Less than HS	0.251	0.434	916	0.460	0.499	376	-0.209***	(0.000)
HS or similar	0.302	0.460	916	0.380	0.486	376	-0.078**	(0.007)
Short tertiary	0.049	0.216	916	0.008	0.089	376	0.041***	(0.000)
BA or similar	0.314	0.465	916	0.138	0.346	376	0.176***	(0.000)
Master or above	0.083	0.276	916	0.013	0.115	376	0.070***	(0.000)
Miss.	0.016	0.126	931	0.013	0.114	381	0.003	(0.689)
Type, SE								
7th/8th	0.069	0.253	931	0.205	0.404	381	-0.136***	(0.000)
9th/10th	0.609	0.488	931	0.701	0.459	381	-0.092**	(0.002)
HPE	0.075	0.264	931	0.037	0.188	381	0.038**	(0.010)
STX/lang.	0.164	0.371	931	0.029	0.168	381	0.135***	(0.000)
USE/math	0.067	0.249	931	0.016	0.125	381	0.051***	(0.000)
Miss.	0.016	0.126	931	0.013	0.114	381	0.003	(0.689)
Field of study*								
Missing	0.016	0.126	931	0.013	0.114	381	0.003	(0.689)
Agri/fores./fishing	0.005	0.073	931	0.003	0.051	381	0.003	(0.504)
Arts	0.013	0.113	931	0.003	0.051	381	0.010	(0.089)
Tech/media prod.	0.004	0.065	931	0.005	0.072	381	-0.001	(0.817)
Build./civil engin.	0.015	0.122	931	0.005	0.072	381	0.010	(0.143)
Bus./admin./law	0.191	0.393	931	0.184	0.388	381	0.007	(0.754)
Engin./techn.	0.016	0.126	931	0.000	0.000	381	0.016*	(0.013)
Human sciences	0.040	0.195	931	0.003	0.051	381	0.037***	(0.000)
Primary school	0.247	0.432	931	0.454	0.499	381	-0.207***	(0.000)
Services	0.005	0.073	931	0.026	0.160	381	-0.021**	(0.001)
Social and health	0.315	0.465	931	0.255	0.436	381	0.060*	(0.031)
Social sciences	0.019	0.138	931	0.010	0.102	381	0.009	(0.258)
Teach. and learn.	0.068	0.251	931	0.018	0.134	381	0.049***	(0.000)
USE	0.033	0.180	931	0.013	0.114	381	0.020*	(0.042)
Missing	0.016	0.126	931	0.013	0.114	381	0.003	(0.689)
Observations	931			381			1312	

Note: *Fields with less than .5% have been left out.

For field of study, I find that individuals in my sample are concentrated in a handful of fields of study reflecting strong gender divides among the cohort in question. As seen from Table 12 the main fields of study for individuals in my sample are *Business Economics, Administration and Law, Primary School Teaching, Social Services and Health* and *Teaching and Learning*. 1/5 of individuals studied within the field of *Business Economics, Administration and Law*, 1/3 studied within *Primary School Teaching*, 1/3 studied within *Social Services and Health*. Finally, 5% of individuals studied within *Teaching and Learning*. The field most likely to proxy an occupation with abstract tasks and lower childrearing compatibility, leading to a higher degree of human capital depreciation during maternity leave, and as such involving higher career costs from childbirth according to Adda et al. (2017) is the field of *Business*. Hence, in order to test the hypothesis that desiring a small family is associated with a higher probability of selecting into abstract occupation, I regress a dummy for having *Business* as field of study on desired family type. Apart from proxying an abstract occupation, I also consider this field of study to be more likely to result in private sector employment. Similarly, I rely on a dummy for having *Teaching and Learning* as field of study to test the hypothesis that individuals desiring a large family are more likely to select into non-abstract occupations characterized by higher childrearing compatibility. I also consider the field *Teaching and Learning* to be more likely to result in public sector employment.

Table 14: Descriptives on Education, early mother versus other

Variable	Mean	Std.	N	Mean	Std.	N	Diff.	P-value
DLSY								
Secondary educ. 1976								
Still in school	0.014	0.117	931	0.021	0.144	381	-0.007	(0.357)
7th/8th	0.033	0.180	931	0.157	0.365	381	-0.124***	(0.000)
9th	0.111	0.314	931	0.236	0.425	381	-0.126***	(0.000)
10th	0.204	0.403	931	0.241	0.429	381	-0.037	(0.135)
LSE	0.314	0.464	931	0.265	0.442	381	0.049	(0.081)
USE no exam	0.014	0.117	931	0.008	0.089	381	0.006	(0.362)
USE	0.308	0.462	931	0.066	0.248	381	0.243***	(0.000)
Miss./other	0.002	0.046	931	0.008	0.089	381	-0.006	(0.127)
Att. post-SE, 1976								
Not att. post-SE	0.640	0.480	931	0.900	0.300	381	-0.260***	(0.000)
Att. post-SE	0.360	0.480	931	0.100	0.300	381	0.260***	(0.000)
Compl. post-SE	0.505	0.500	931	0.388	0.488	381	0.116***	(0.000)
Registers								
Timing								
Grad year, SE	1971	2.229	916	1970	1.913	376	0.893***	(0.000)
Grad year, post-SE	1981	9.371	656	1985	12.287	198	-3.704***	(0.000)
Dummy for USE	0.306	0.461	931	0.081	0.274	381	0.225***	(0.000)
Level								
Less than HS	0.251	0.434	916	0.460	0.499	376	-0.209***	(0.000)
HS or similar	0.302	0.460	916	0.380	0.486	376	-0.078**	(0.007)
Short tertiary	0.049	0.216	916	0.008	0.089	376	0.041***	(0.000)
BA or similar	0.314	0.465	916	0.138	0.346	376	0.176***	(0.000)
Master or above	0.083	0.276	916	0.013	0.115	376	0.070***	(0.000)
Miss.	0.016	0.126	931	0.013	0.114	381	0.003	(0.689)
Type, SE								
7th/8th	0.069	0.253	931	0.205	0.404	381	-0.136***	(0.000)
9th/10th	0.609	0.488	931	0.701	0.459	381	-0.092**	(0.002)
HPE	0.075	0.264	931	0.037	0.188	381	0.038**	(0.010)
STX/lang.	0.164	0.371	931	0.029	0.168	381	0.135***	(0.000)
USE/math	0.067	0.249	931	0.016	0.125	381	0.051***	(0.000)
Miss.	0.016	0.126	931	0.013	0.114	381	0.003	(0.689)
Field of study*								
Missing	0.016	0.126	931	0.013	0.114	381	0.003	(0.689)
Agri/fores./fishing	0.005	0.073	931	0.003	0.051	381	0.003	(0.504)
Arts	0.013	0.113	931	0.003	0.051	381	0.010	(0.089)
Tech/media prod.	0.004	0.065	931	0.005	0.072	381	-0.001	(0.817)
Build./civil engin.	0.015	0.122	931	0.005	0.072	381	0.010	(0.143)
Bus./admin./law	0.191	0.393	931	0.184	0.388	381	0.007	(0.754)
Engin./techn.	0.016	0.126	931	0.000	0.000	381	0.016*	(0.013)
Human sciences	0.040	0.195	931	0.003	0.051	381	0.037***	(0.000)
Primary school	0.247	0.432	931	0.454	0.499	381	-0.207***	(0.000)
Services	0.005	0.073	931	0.026	0.160	381	-0.021**	(0.001)
Social and health	0.315	0.465	931	0.255	0.436	381	0.060*	(0.031)
Social sciences	0.019	0.138	931	0.010	0.102	381	0.009	(0.258)
Teach. and learn.	0.068	0.251	931	0.018	0.134	381	0.049***	(0.000)
USE	0.033	0.180	931	0.013	0.114	381	0.020*	(0.042)
Missing	0.016	0.126	931	0.013	0.114	381	0.003	(0.689)
Observations	931			381			1312	

Note: *Fields with less than .5% have been left out.

Comparing early mothers and other respondents, we see from Table 14 that early mothers completed significantly less education than other respondents did. Early mothers are significantly more likely to have left school after 7th, 8th and 9th grade. Early mothers are thus significantly less likely to have completed USE than other respondents are. 1/3 of other respondents completed USE, while only 6.6% of early mothers had done so by 1976.

From the registers, we see that other respondents are significantly more likely to complete a University degree than early mothers are with more than 40% of other respondents completing some degree. Only 1/6 of early mothers achieve a University degree, while 38% complete vocational education. Regarding field of study, early mothers are significantly overrepresented in *Primary School Teaching* and *Services*, while other respondents are overrepresented in the fields of *Human Sciences, Social and Health, Teaching and Learning* and *USE Teaching*.

4.3   Labor Market

For labor market outcomes, I wish to test the hypotheses that desiring a large family is associated with lower cumulative labor supply and wage earnings over working career and that realization of fertility is associated with reduced earnings independently of desires. This requires outcome measures of both income and labor supply. From the administrative registers, I have access to several series of annual income data. The broadest income measure is annual gross income, which includes capital income, labor market earnings and income from public transfers and pensions. The gross income measure is also available net of taxes. However, my main interest lies with the narrower income measures of annual labor market earnings and wage earnings from paid employment as these reflect individual labor market performance more directly. To construct income over working career, I first correct annual income for price developments over time, and then cumulate annual income over the period from 1980 to 2017. The cumulative income measures are all expressed in millions DKK at 2019-price levels. Unfortunately, I lack income data for the early adulthood of individuals in my sample. By 1980, the starting year of existing income registers, a majority of mothers-to-be in my sample have already had their first child, and

potentially taken their first leave from the labor market. Hence, ideally my cumulative measure would include also income for the years 1972-79 around ages 18 to 25. All other things equal, this data constraint may reduce estimated associations between income and desired family type (and realized childbirths).[15] Descriptives on income measures are found in Table 15 and show that individuals in my sample had a cumulative gross income of 25.3 mill. DKK measured in 2019-price levels over the period from 1980 to 2017. This is equivalent to an average annual income of 0.68 mill. DKK over the 38-year period. Considering labor market earnings and wages respectively, the average annual income is reduced to 0.55 mill. DKK and 0.24 mill. DKK, respectively. Average annual transfers amount to 46.000 DKK annually, and as such cannot explain the large gap between gross income and wage earnings. This indicates that capital gains (including rental value of owned housing) and earnings from private enterprise on average play a large role for the total gross income of individuals in my sample. Annual after-tax income on average amounts to 0.2 million DKK.

Table 15: Income 1980-2017, Sample 1

Variable	Mean	Std.	N
Cum. gross inc. (mio. DKK)	25.315	18.258	1294
Avg. gross inc. (mio. DKK)	0.676	0.475	1294
Cum. earnings (mio. DKK)	20.617	11.619	1294
Avg. earnings (mio. DKK)	0.550	0.302	1294
Cum. wage earnings (mio. DKK)	8.980	4.850	1312
Avg. wage earnings (mio. DKK)	0.244	0.124	1294
Cum. transfers inc. (mio. DKK)	1.659	1.574	1312
Avg. transfers inc. (mio. DKK)	0.046	0.043	1294
Cum. disp. inc. (mio. DKK)	7.523	4.227	1312
Avg. disp. inc. (mio. DKK)	0.205	0.107	1294

Note: ***/**/* indicate statistical difference at the 1%/5%/10%-level. P-values in parentheses. Income is in millions DKK at 2019-price levels.

Comparing early mothers and other respondents, from Table 16 we see that the average annual income of early mothers amounts to 80% of the average annual income of other respondents. This is true for gross income, labor market earnings and wage earnings, while public transfers of early mothers are 36% higher than those of other respondents. Due to

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[15]I intend to extend the analysis, once historical income data for the period 1970 to 1979 becomes available. This would allow me to construct wage growth over full career from age 18 to 63.

higher transfers and equalizing tax measures, average annual disposable income of early mothers amounts to 85% of the average annual disposable income of other respondents.

Table 16: Income 1980-2017, Other vs. early mother

Variable	Mean	Std.	N	Mean	Std.	N	Diff.	P-value
Cum. gross inc. (mio. DKK)	26.687	20.806	916	21.991	8.773	378	4.696***	(0.000)
Avg. gross inc. (mio. DKK)	0.713	0.543	916	0.588	0.220	378	0.124***	(0.000)
Cum. earnings (mio. DKK)	22.057	11.914	916	17.126	10.061	378	4.932***	(0.000)
Avg. earnings (mio. DKK)	0.588	0.309	916	0.456	0.262	378	0.132***	(0.000)
Cum. wage earnings (mio. DKK)	9.521	4.869	931	7.658	4.546	381	1.863***	(0.000)
Avg. wage earnings (mio. DKK)	0.259	0.123	916	0.206	0.117	378	0.053***	(0.000)
Cum. transfers inc. (mio. DKK)	1.503	1.507	931	2.040	1.667	381	-0.537***	(0.000)
Avg. transfers inc. (mio. DKK)	0.041	0.041	916	0.056	0.046	378	-0.015***	(0.000)
Cum. disp. inc. (mio. DKK)	7.868	4.779	931	6.681	2.180	381	1.187***	(0.000)
Avg. disp. inc. (mio. DKK)	0.214	0.122	916	0.181	0.050	378	0.033***	(0.000)
No. of observations	931			381			1312	

Note: ***/**/* indicate statistical difference at the 1%/5%/10%-level. P-values in parentheses. Income is in millions DKK and 2019-price levels.

Considering now the association between desired family type and labor supply and participation, I have access to a broad range of employment outcomes for the period 1976 to 2017 from the registers, including age at employment entry, labor supply over career, years in different occupational categories, as well as cumulative unemployment degree. These allow me to investigate how labor market outcomes are associated with both desires and realized fertility.

My main focus is labor supply over working career. I construct annual employment degrees from annual obligatory contributions to the public pension's fund, ATP, for the period 1976-2007. The annual contributions reflect how many hours the individual was working over the year. Annual employment degrees are then constructed by dividing ATP contributions by the maximum yearly contribution, which is considered equivalent to full time employment over the year.[16] For the period 2008 to 2017, the registers contain actual annual working hours. They are combined with earlier employment degrees by dividing annual working hours by full time employment (52 weeks times 37 hours). Cumulating annual employment degrees gives me cumulative labor supply over the period 1976-2017 (age 22 to 63). The ATP contributions also allow me to construct measures of cumulative

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[16]A contribution of 1166 DKK is equivalent to full time over the year.

full time employment ($>$ 30 hours per week), part-time employment ($10-30$ hours per week), and no employment ($<$ 10 hours per week). A part from these measures of labor supply, I have access to data on main occupation over the year allowing me to construct cumulative years as employee, independent, assisting spouse a.o. over working career.

For unemployment, the registers contain a measure of the cumulative unemployment degree over the period 1980-2017, as well as cumulative weeks with unemployment insurance benefit receipt. Additionally, access to data on employers allows me to construct measures of cumulative number of jobs and average job duration. Finally, I consider a dummy for ever having been a manager from the DLSY survey. In the empirical analysis I focus mainly on entry year, cumulative labor supply and unemployment, as well as on job stability as reflected in number of jobs and average job duration.

From Table 17, we see that the average age at entry to employment is 23 years, though the median age of entry is closer to 22 years. By 2017, close to 9% of individuals have retired. [17] On average, individuals have been in some employment for more than 35 of the 38 data years, with a cumulative employment degree of 27.8 equivalent to 73% of potential working time. They have spent 1/5 of their time in either no employment (0-9 hours/week), close to 1/3 of their time in part-time employment (10-30 hours/week) and half of their time in full time employment (>30 hours/week). Their average working week had 25 hours. Regarding cumulative unemployment, on average individuals have been unemployed with benefits for 2.5 years (136 weeks) from 1980 to 2017. They have held seven jobs on average and worked in each job for approximately 5 years. Their dominating occupational category is employment. Half of respondents held a position with managerial responsibilities.

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[17]The official retirement age for individuals in my sample ranges from 65 to 67 years.

Table 17: Labor market outcomes, Sample 1

Variable	Mean	Std.	N
Registers, 1976-2017			
Age at entry to empl.	22.965	9.581	1312
Retired ($<=2017$)	0.089	0.285	1312
Years (some) empl.	35.283	9.111	1294
Years Full time	20.257	12.110	1294
Years Part time	10.765	7.496	1294
Years No empl.	8.114	8.990	1312
Share of period			
Some empl.	0.882	0.195	1294
Full time	0.502	0.286	1294
Part time	0.278	0.189	1294
No empl.	0.204	0.229	1312
Cum. labor supply			
Cum. empl. degr.	27.761	11.607	1312
Avg. labor supply			
Avg. hours/week	25.596	9.641	1312
Year in occ.			
Independent	1.095	4.274	1312
Ass. spouse	0.430	2.734	1312
Empl. w/ company	0.429	2.058	1312
Empl.	27.746	11.094	1312
Empl. w/ UI	0.477	0.990	1312
Retired w/ company	0.053	0.837	1312
Retired	1.845	5.486	1312
Other	1.499	3.817	1312
Early retirem.	0.748	1.210	1312
Unempl.	1.123	2.201	1312
Unempl. w/ UI	0.474	1.068	1312
Unempl. w/ SA	0.449	1.800	1312
Registers, 1980-2017			
Cum unempl. degree	96.797	213.559	1293
Unempl. in weeks	136.814	180.672	1293
No. of jobs	7.087	4.319	1281
Avg. job dur.(years)	5.623	4.128	1281
DLSY, 2001			
Ever manager	0.450	0.498	1124

Note: xxx

Table 18: Labor market outcomes, early mother versus other

Variable	Mean	Std.	N	Mean	Std.	N	Diff.	P-value
Registers, 1976-2017								
Age at entry to empl.	22.773	10.099	931	23.432	8.169	381	-0.659	(0.217)
Retired (<=2017)	0.070	0.255	931	0.136	0.344	381	-0.067***	(0.001)
1976-2017								
Years (some) empl.	36.287	8.374	916	32.849	10.301	378	3.438***	(0.000)
Years full time	21.788	11.909	916	16.548	11.801	378	5.241***	(0.000)
Years part time	10.371	7.470	916	11.720	7.481	378	-1.348**	(0.003)
Years no empl.	6.954	8.117	931	10.948	10.309	381	-3.994***	(0.000)
Share of period								
Some empl.	0.904	0.173	916	0.828	0.231	378	0.077***	(0.000)
Full time	0.540	0.277	916	0.410	0.284	378	0.130***	(0.000)
Part time	0.268	0.189	916	0.300	0.187	378	-0.032**	(0.005)
No empl.	0.175	0.208	931	0.275	0.263	381	-0.100***	(0.000)
Cum. labor supply								
Cum. empl. degr.	28.885	11.261	931	25.014	11.992	381	3.871***	(0.000)
Avg. labor supply								
Avg. hours/week	26.637	9.192	931	23.052	10.234	381	3.585***	(0.000)
Year in occ.								
Independent	1.127	4.193	931	1.018	4.469	381	0.108	(0.685)
Ass. spouse	0.461	2.909	931	0.354	2.251	381	0.106	(0.477)
Empl. w/ company	0.511	2.281	931	0.228	1.349	381	0.283**	(0.006)
Empl.	28.521	10.835	931	25.853	11.498	381	2.668***	(0.000)
Empl. w/ UI	0.403	0.905	931	0.659	1.154	381	-0.256***	(0.000)
Retired w/ company	0.037	0.713	931	0.092	1.081	381	-0.055	(0.357)
Retired	1.463	4.849	931	2.777	6.711	381	-1.314***	(0.001)
Other	1.354	3.712	931	1.853	4.046	381	-0.499*	(0.038)
Early retirem.	0.701	1.173	931	0.861	1.291	381	-0.159*	(0.037)
Unempl.	1.044	2.220	931	1.315	2.142	381	-0.271*	(0.040)
Unempl. w/ UI	0.383	0.941	931	0.696	1.303	381	-0.312***	(0.000)
Unempl. w/ SA	0.306	1.361	931	0.798	2.543	381	-0.492***	(0.000)
Registers, 1980-2017								
Cum unempl. degree	74.628	181.305	916	150.660	269.473	377	-76.033***	(0.000)
Unempl. in weeks	120.330	174.218	916	176.865	189.823	377	-56.535***	(0.000)
No. of jobs	6.980	4.171	910	7.350	4.658	371	-0.370	(0.184)
Avg. job dur.(years)	5.929	4.371	910	4.872	3.349	371	1.057***	(0.000)
DLSY, 2001								
Ever manager	0.466	0.499	792	0.413	0.493	332	0.053	(0.100)
Observations	931			381			1312	

From Table 18 we see that working lives of early mothers are characterized by reduced labor supply relative to other respondents. Early mothers have spent significantly less time in full-time employment and significantly more time in either part-time or no employment. Their cumulative unemployment degree is twice as large as that of other respondents, reflected also in a higher number of cumulative weeks with unemployment benefits. Finally, they spent more time in occupational categories characterized by public transfers, such as early retirement and welfare benefits, than other respondents. Having presented my outcomes measures, in the next section I review my empirical results.

5   Results

In the following section, I test my hypotheses on the association between desired family type and outcomes as proposed in Section 2. I start by presenting my empirical model. Based on estimations of the model, I then present my empirical results on the associations between fertility desires on one hand, and family formation, completed education and labor market outcomes, including income, on the other.

5.1   Estimation Strategy

In order to test my hypotheses, I rely on reduced-form estimations of the relationship between outcomes and desired family type according to the following empirical model:

$$y_i = \beta_0 + \beta_1 Nodes_i + \beta_2 Large_i + \beta_3 Nodes \times Mom_i + \beta_4 Des \times Mom_i$$
$$+ \beta_5 earn_i + \beta_6 fam_i + \beta_7 X_i + \varepsilon_i$$

where $Nodes_i$ indicates no family desired (i.e. dummy for having no desire for a family), $Large$ indicates large family desired (i.e. dummy for desiring a large family). $Nodes \times Mom_i$ indicates early mother with no additional desire (dummy) and $Des \times Mom_i$ indicates early mother with additional desire (dummy). This structure implies that the reference category of the estimation is *Other respondent, Small family desired*. For sample 2, the dummy $Nodes \times Mom_i$ is left out. Finally, for sample 3 both $Nodes_i$ and $Nodes \times Mom_i$ are left out. $earn_i$ is a vector of factors related to individual earnings

potential, fam_i is a vector containing early relationship factors, specifically relationship status in 1973 and 1976 at elicitation of fertility desires. X_i is a vector of socio-economic characteristics and fixed effects for birth years $1952 - 1956$. Finally, y_i denotes the different outcome variables of interest. The model estimates the correlation between outcomes and desired family type, controlling for a broad set of covariates and fixed effects. The model is flexible enough to accommodate non-monotonous effects of desired family type on outcomes. However, coefficients on desired family type cannot be interpreted as causal effects due to risk of confounding factors, i.e. unobservables in the error term ε_i correlating with desires.

For family formation outcomes, i.e. age at first birth and realized fertility, I complement the main empirical model based on desired family type with a model specification that integrates desires as a continuous variable:

$$y_i = \beta_0 + \beta_1 desires_i + \beta_3 Nodes \times Mom_i + \beta_4 Des \times Mom_i$$
$$+ \beta_5 earn_i + \beta_6 fam_i + \beta_7 X_i + \varepsilon_i$$

where $desires_i$ is number of children desired. The two specifications relying on desired family type and raw desires, respectively, complement each other.

For educational attainment, I complement the main empirical model with a model that allows for differential effects of desiring a family versus having no desire for a family:

$$y_i = \beta_0 + \beta_1 Des_i + \beta_3 Nodes \times Mom_i + \beta_4 Des \times Mom_i$$
$$+ \beta_5 earn_i + \beta_6 fam_i + \beta_7 X_i + \varepsilon_i$$

where Des_i indicates family desired, such that *No desire* is the reference category. Finally, for estimations of income and labor market outcomes on desired family type, I expand the vector fam_i with long-term family formation outcomes, including a dummy for being married at all, a dummy for having had more than one stable relationship during fertile age, and with realized fertility, i.e. number of experienced childbirths. Realized fertility is included to allow for separate effects of desired family type and realized fertility on labor market outcomes and income over working career. I also include completed education

among explanatory variables. In my final specification, I include realized labor market outcomes in estimations on income to illustrate the transmission from fertility desires to income via, e.g., labor supply. Hence, I end up estimating the following model:

$$y_i = \beta_0 + \beta_1 desires_i + \beta_3 Nodes \times Mom_i + \beta_4 Des \times Mom_i$$
$$+ \beta_5 earn_i + \beta_6 fam_i + \beta_7 X_i + \beta_8 educ_i + \beta_9 labor_i + \varepsilon_i$$

where $educ_i$ is a vector of educational level categories and $labor_i$ is a vector of labor market outcomes, including labor supply.

For all empirical estimations, I rely on robust standard errors for inference.

5.2 Results on family formation:

I start my empirical analysis by establishing a direct association between desired family type and family formation. Hence, I present my empirical results related to the hypothesis that higher fertility desires are associated with a lower age at first birth and higher levels of realized fertility. In addition, I examine the hypothesis that women with a stronger earnings potential have a higher risk of experiencing a fertility deficit, i.e. of realizing fertility below desires.

5.2.1 Results on age at first birth and gaps

Results from empirical estimations of age at first birth on desires and desired family type are found in Tables 19 and 20. For age at first birth, the sample is naturally reduced to women, who experienced motherhood. The results in column (1) of Table 19 indicate that an individual's age at first birth is negatively and significantly correlated with her desired number of children. For each desired child expected age at first birth is reduced by half a year. From results in column (2), we see that taste for fertility continues to influence timing of family formation significantly (by .5 years), when controlling for earnings potential, including educational expectations, time preferences and ability. However, an individual's timing of first birth is also strongly correlated with her educational expectations and time preferences. Expecting to complete USE is associated with a delay in age at first birth of 1.7 years, while individual time preferences are associated with significant drops in age

at first birth of almost half a year (for each unit increase on the patience scale ranging from 1 to 3). Results in column (3) indicate that educational expectations are central to early relationship formation, as the inclusion of early relationship outcomes renders the coefficient on adolescent educational expectations insignificant. This is in line with findings of Geruso and Royer (2018) and Black et al. (2008) that timing of graduation is correlated with entry into stable relationships, e.g., cohabitation or marriage. The negative association between timing of birth and fertility desires is robust to the inclusion of early relationship outcomes. The same is true for the positive association between timing of birth and time preferences.Being an early mom with additional desire for fertility is correlated

with reductions in age at first birth of 5-7 years, while being an early mother with no additional desire is correlated with reductions in age at first birth of 7-9 years. This is mainly a mechanical reflection of these subgroups having a mean age at first birth of 20 years, while other respondents have a mean age at first birth of 27 years. Taking adolescent preferences and ability, as well as early family formation into account, the model accounts for 58% of the variation in age at first birth.

Considering now results on the association between age at first birth and desired family type, results in column (1) of Table 20 show that having no desire for a family is associated with a delay in age at first birth of more than 2 years, while desiring a large family is associated with an insignificant anticipation by one quarter of a year compared to the reference group desiring a small family. This indicates that timing of birth is more sensitive to the individual's desire for having children than to the actual family size desired. Once individual earnings potential and early relationship outcomes are controlled for, the coefficient on desiring a large family increases numerically to 0.4 years and turns statistically significant (at the 10%-level). This indicates that desiring a large family is associated with anticipation of childbearing by almost half a year compared to the reference group desiring a small family. The conclusion that timing of birth is more sensitive to having no desire than to desires remains valid, though the delay is reduced to 1.6 years once earnings potential and early relationships are included as controls. The coefficient on having no desire remain statistically significant at the 1%-level. Surprisingly, women, who answered that they do not know if they want children, or how many they

Table 19: Age at first birth on desires, Sample 1

Variable	(1) Coef.	(1) SE	(2) Coef.	(2) SE	(3) Coef.	(3) SE
Taste for fertility						
Desires	-0.5136***	(0.1632)	-0.5447***	(0.1577)	-0.4166***	(0.1570)
Mom: Desire	-6.8503***	(0.1780)	-6.3962***	(0.1763)	-5.3181***	(0.2311)
Mom: No Desire	-9.2086***	(0.4274)	-8.7103***	(0.4308)	-7.4021***	(0.4894)
Earnings potential						
Time prefs.			0.5616***	(0.1609)	0.5387***	(0.1627)
Rosen score*			0.0566	(0.1111)	0.1797*	(0.1085)
Own exp.						
(Ref. 7th(/8th/Miss.))						
Exp. 9th/10th			-0.1210	(0.3146)	-0.2071	(0.4026)
Exp. LSE			0.7038*	(0.3794)	0.2302	(0.4664)
Exp. USE			1.7441***	(0.4834)	0.7498	(0.5764)
Ability**						
Cognitive			-0.0543	(0.1538)	-0.0376	(0.1462)
Spatial			0.2492*	(0.1324)	0.2423*	(0.1279)
Verbal			0.0492	(0.1605)	0.0337	(0.1628)
Early relationships						
(1973)						
Marr./cohab					-0.9777***	(0.3307)
Partner					-0.7745***	(0.2655)
(1976)						
Consid. move-in					-2.8403***	(0.7070)
Consid. marr.					-0.7212	(0.5780)
Cohab.					-2.0835***	(0.3419)
Marr.					-2.8043***	(0.3586)
Constant	28.4899***	(0.3985)	26.8009***	(0.5504)	28.8748***	(1.1394)
Controls					X	
Fix					X	
No. of obs.	1128		1067		1017	
R^2	0.48		0.51		0.58	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Standardized Rosenberg's self-esteem score from 1973. **Ability is measured by standardized test scores from 1968. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

want, tend to anticipate motherhood by close to 1.5 years. In line with findings above, time preferences (+.6 years) and educational expectations (+1.5 years) are positively and significantly associated with age at first birth, though the coefficient on educational expectations drops, once early relationship outcomes are included as controls, cf. results in column (3). Forming stable relationships in early adulthood is associated with reductions in age at first birth of 1-3 years. Taking adolescent preferences and ability, as well as early

family formation into account, the model accounts for 56% of the variation in age at first birth.

Table 20: Age at first birth on desired family type, Sample 1

Variable	(1) Coef.	SE	(2) Coef.	SE	(3) Coef.	SE
Desired family type						
(Ref. Small family)						
No desire	2.3368***	(0.6538)	2.0733***	(0.6386)	1.6294***	(0.6268)
Large family	-0.2440	(0.2446)	-0.3884	(0.2453)	-0.4079*	(0.2311)
Don't know	-1.6088*	(0.9567)	-1.6384*	(0.9218)	-1.4244*	(0.8321)
Mom: Desire	-6.7850***	(0.1759)	-6.3549***	(0.1755)	-5.3271***	(0.2166)
Mom: No Desire	-10.3011***	(0.6462)	-9.5719***	(0.6409)	-8.2281***	(0.6524)
Earnings potential						
Time prefs.			0.6175***	(0.1610)	0.5817***	(0.1548)
Rosen score*			0.0649	(0.1095)	0.1351	(0.1040)
Own exp.						
(Ref. 7th(/8th/Miss.)						
Exp. 9th/10th			-0.2302	(0.3287)	-0.2373	(0.3309)
Exp. LSE			0.5809	(0.3944)	0.3519	(0.3902)
Exp. USE			1.5399***	(0.4952)	1.1761**	(0.4836)
Ability**						
Cognitive			-0.0885	(0.1551)	-0.0564	(0.1461)
Spatial			0.2727**	(0.1328)	0.2478*	(0.1267)
Verbal			0.0618	(0.1601)	0.0838	(0.1505)
Early relationships						
(1973)						
Marr./cohab					-0.9150***	(0.3245)
Partner					-0.7444***	(0.2572)
(1976)						
Consid. move-in					-3.2427***	(0.6566)
Consid. marr.					-0.5671	(0.5535)
Cohab.					-1.9255***	(0.3347)
Marr.					-2.8225***	(0.3450)
Constant	27.2455***	(0.1823)	25.5791***	(0.4463)	27.8161***	(0.5339)
Controls					X	
Fix					X	
No. of obs.	1146		1085		1085	
R^2	0.48		0.51		0.56	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Standardized Rosenberg's self-esteem score from 1973. **Ability is measured by standardized test scores from 1968. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

To sum up results on the association between timing of childbearing and fertility desires, I find that desired family type and fertility desires do influence timing of first birth

significantly in line with my hypothesis. However, timing is more sensitive to having desire versus having no desire than to the number of children desired. Particularly having no desire for a family delays timing of first birth by 1.5-2 years, while desiring a large family is associated with anticipations of childbearing by 1/4-1/2 years relative to individuals desiring a small family. Moreover, I find that anticipation due to fertility desires is exacerbated by early entry or plans to enter a stable relationship, while high patience and educational aspirations in adolescence tend to delay timing of first birth.

5.2.2 Completed fertility

Considering the direct association between fertility desires and realized fertility levels, we see, from results in column (1) of Table 21, that an individual's desired number of children is positively and significantly associated with her realized fertility level. Each additional child desired is associated with an expected increase in realized fertility of 1/3 of a child. The coefficient is statistically significant at the 1%-level. The additional association of being an early mother with desire is .56 children, resulting in a total effect of .8 children, while the additional association of being an early mother with no desire is 1.05, resulting in a total effect of 1.29 children. This may reflect that early mothers with no additional desire have more children at elicitation of desires.

As seen from column (2) of Table 21 controlling for individual earnings potential and relationship status in early adulthood, reduces the association between desires and realized fertility slightly to 1/4, but the coefficient remains statistically significant at the 1%-level. Relationship status in early adulthood is associated with significant increases in realized fertility of 1/6-1/4 children. Finally, from column (3) in Table 21, we see that the association between desires and fertility remains practically unchanged and statistically significant after controlling for later relationship outcomes. Regarding long term relationship outcomes, the association between ever being married and realized fertility is larger than that of fertility desires and realized fertility with a statistically significant coefficient of 1.1, while having had multiple stable relationships is associated with a statistically significant reduction in realized fertility of 1/6. Taking into account adolescent preferences

and ability, as well as early family formation and later relationship outcomes, the model accounts for 24% of the variation in realized fertility.

Table 21: Number of children on desires, Sample 1

	(1)		(2)		(3)	
Variable	Coef.	SE	Coef.	SE	Coef.	SE
Taste for fertility						
Desires	0.2925***	(0.0351)	0.2575***	(0.0368)	0.2354***	(0.0364)
Mom: Desire	0.6760***	(0.0688)	0.5540***	(0.0767)	0.5319***	(0.0773)
Mom: No Desire	1.2793***	(0.1285)	1.0683***	(0.1454)	0.9980***	(0.1486)
Early relationships						
(1976)						
Consid. move-in			0.2383	(0.2582)	0.3002	(0.2077)
Consid. marr.			0.2521**	(0.1258)	0.2511**	(0.1211)
Cohab.			0.1548*	(0.0797)	0.0438	(0.0791)
Marr.			0.2923***	(0.0887)	0.1027	(0.0893)
Later relationships						
Ever married					1.0525***	(0.1254)
>1 cohab/marriage					-0.1685***	(0.0581)
Constant	1.0436***	(0.0814)	0.9226***	(0.3289)	0.3833	(0.3178)
Earnings potential			X		X	
Controls			X		X	
Fix			X		X	
No. of obs.	1290		1160		1121	
R^2	0.16		0.19		0.24	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Earnings potential* consists of standardized inductive, spatial and verbal ability scores, standardized score on Rosenberg's self-esteem test, expectations for secondary expectations and time preferences. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

Considering the association between desired family type and realized fertility, we see from results in column (1) of Table 22 that having no desire for a family is associated with a significant reduction of 1/3 in realized fertility, while desiring a large family is associated with a significant increase of 1/2 compared to the reference group desiring a small family. Additional large associations are found for early mothers (.7-1.0). Controlling for early and late relationship factors reduces the coefficient on no desire to 1/5 and the coefficient on desiring a large family to .4. The coefficient on desiring a large family remains statistically significant at the 1%-level. From column (2), we see that early relationships are associated with increases in realized fertility in line with findings above. Similarly, from column (3), we see that the association between ever being married and realized fertility is large

and significant with a coefficient of 1.1, while having had multiple stable relationships is associated with a significant reduction in realized fertility of 1/6 as above. Taking into account preferences and ability, as well as early family formation and later relationship outcomes, the model accounts for 25% of the variation in realized fertility.

Table 22: Number of children on desired family type, Sample 1

Variable	(1) Coef.	SE	(2) Coef.	SE	(3) Coef.	SE
Desired family type						
(Ref. Small family)						
No desire	-0.3366***	(0.1168)	-0.2774**	(0.1280)	-0.2041*	(0.1233)
Large family	0.5031***	(0.0659)	0.4402***	(0.0678)	0.4370***	(0.0653)
Don't know	-0.0942	(0.1911)	-0.0939	(0.1998)	0.0977	(0.1713)
Mom: Desire	0.7080***	(0.0682)	0.5595***	(0.0761)	0.5392***	(0.0766)
Mom: No Desire	1.1168***	(0.1490)	0.8915***	(0.1702)	0.8040***	(0.1705)
Early relationships						
(1976)						
Consid. move-in			0.2563	(0.2603)	0.2944	(0.2149)
Consid. marr.			0.2662**	(0.1247)	0.2599**	(0.1205)
Cohab.			0.1848**	(0.0795)	0.0654	(0.0787)
Marr.			0.3425***	(0.0883)	0.1402	(0.0885)
Later relationships						
Ever married					1.0897***	(0.1244)
>1 cohab/marriage					-0.1770***	(0.0574)
Constant	1.5427***	(0.0393)	1.3677***	(0.3188)	0.7545**	(0.3103)
Earnings potential			X		X	
Controls			X		X	
Fix			X		X	
No. of obs.	1312		1179		1140	
R^2	0.16		0.19		0.25	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Earnings potential* consists of standardized inductive, spatial and verbal ability scores, standardized score on Rosenberg's self-esteem test, expectations for secondary expectations and time preferences. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

To sum up results on the association between realized fertility and fertility desires, my hypothesis that stronger fertility desires are associated with higher levels of realized fertility is confirmed, independently of individual earnings potential. The results also confirm a direct link between desired family type and realized fertility levels among individuals in my sample. The difference in realized fertility between women desiring a small family and women desiring a large family of 1/2 child is larger than the difference between women de-

siring a small family and women desiring no family. However, the link between desires and realized fertility is not one-to-one reflecting that the transmission from desires to realized fertility is far from perfect. Both early and later relationship outcomes tend to exacerbate the effect of fertility desires on realized fertility, particularly early considerations of marriage and early as well as late marriage are associated with increases in realized fertility. Instead changing partners tends to reduce overall levels of realized fertility.

5.2.3 Fertility deficits

The final hypothesis regarding family formation is, whether women with higher earnings potential face higher risks of experiencing a fertility deficit, i.e. whether they have a higher probability of experiencing realized fertility below their desired level.

I conduct the analysis on the subset of individuals desiring (additional) children, i.e. on sample 3. To learn more about the importance of extensive and intensive margin effects, I first consider a dummy for having experienced any fertility deficit, which could either be the result of remaining childless despite desiring a family or of having fewer children than desired. Next, I consider a dummy for remaining childless over fertile age to evaluate extensive margin effects in isolation. To account for individual earnings potential, I focus on the impact of cognitive ability, educational aspirations and time preferences.

Results on the probability of experiencing a fertility deficit are reported in Table 23. From results in column (1), we see that desiring a large family is associated with an increased probability of experiencing a fertility deficit irrespective of having a cognitive ability below the 25th-percentile or above the 75th quartile compared to individuals with medium ability levels and a positive desire for a family. The increase in the probability of experiencing a fertility deficit following from desiring a large family of 25 percentage points is sizeable and statistically significant. This suggests that having high desires per se increases the probability of experiencing a fertility deficit, while it lends no evidence to the hypothesis that high ability individuals have a higher risk of experiencing a fertility deficit than lower ability individuals. However, when including a broader set of factors related to individual earnings potential, we see from results in column (2) that expecting to complete USE is associated with a significant increase of 9 percentage-points in the

probability of experiencing a fertility deficit. This suggests that individuals with educational aspirations do have a higher likelihood of experiencing a fertility deficit relative to individuals with lower educational ambitions. Given the overall sample probability of experiencing a fertility deficit of 50%, this is a sizeable increase of almost 1/5 for women desiring both children and an education. Moreover, the inclusion of early relationship outcomes renders the coefficient on educational expectations insignificant as seen from results in column (3) of Table 23. This indicates that the influence of educational aspirations on the formation of stable relationships drives the higher risk of experiencing a fertility deficit. In fact, having no partner at elicitation of desires is correlated with an increased risk of 16 percentage points of experiencing a fertility deficit. Instead, early marriage or considerations of marriage lower the probability by 10 percentage points. Finally, from column (4) we see that having more than one stable relationship over adult life is associated with an increase of 12 percentage points in the probability of experiencing a deficit, while marriage over all reduces the probability by 1/3.

Considering now extensive margin effects, from results in column (1) of Table 24 we see that desiring a large family has no significant effect on the probability of remaining childless regardless of being high or low ability compared to individuals with medium ability levels and a positive desire for a family. Across specifications, I find that desiring a small family and having ability below the 25th percentile is associated with an increased probability of remaining childless by 5-6 percentage points. Thus, weaker desires combined with low ability is associated with a higher risk of remaining childless. In addition, having no partner in early adulthood (1976) is associated with an increase in the probability of remaining childless of 10 percentage points, while getting married during adult life is associated with a sizeable and significant reduction in the probability of remaining childless of 40 percentage points.

To sum up, educational aspirations are found to be associated with a lower probability of fulfilling one's fertility desires among women desiring a family. My results indicate that this follows from the influence of higher educational expectations on the formation of stable relationships in early adulthood. The result reflects intensive margin effects rather than extensive margin effects, as educational expectations are not associated with a higher

Table 23: Risk of fertility below desires, Sample 3

Variable	(1) Coef.	SE	(2) Coef.	SE	(3) Coef.	SE	(4) Coef.	SE
Family type, ability								
(Ref. desire, medium*)								
Small, high	-0.0665	(0.0442)	-0.1128**	(0.0495)	-0.1203**	(0.0488)	-0.1006**	(0.0485)
Large, high	0.2547***	(0.0484)	0.2100***	(0.0541)	0.2186***	(0.0532)	0.2650***	(0.0545)
Small, low	-0.1271***	(0.0420)	-0.0992**	(0.0470)	-0.1109**	(0.0451)	-0.1492***	(0.0449)
Large, low	0.2582***	(0.0575)	0.2745***	(0.0614)	0.2773***	(0.0611)	0.2898***	(0.0653)
Earnings pot.								
Exp. USE			0.0890**	(0.0420)	0.0825**	(0.0415)	0.0659	(0.0448)
Rosen score*			-0.0290*	(0.0148)	-0.0266*	(0.0146)	-0.0070	(0.0147)
Time prefs.			-0.0093	(0.0224)	-0.0152	(0.0221)	-0.0058	(0.0221)
Ability**								
Spatial			0.0205	(0.0186)	0.0153	(0.0183)	0.0081	(0.0182)
Verbal			0.0071	(0.0217)	0.0048	(0.0211)	-0.0081	(0.0217)
Early rela.								
(1976)								
No relationship					0.1593***	(0.0417)	0.1160***	(0.0435)
Consid./married					-0.1011***	(0.0323)	-0.0765**	(0.0328)
Late rela.								
Ever married							-0.3534***	(0.0485)
>1 cohab/marriage							0.1283***	(0.0308)
Constant	0.4665***	(0.0200)	0.4668***	(0.0499)	0.4945***	(0.0523)	0.9271***	(0.1330)
Controls							X	
Fix							X	
No. of obs.	1119		1064		1064		1012	
R^2	0.05		0.06		0.09		0.17	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

probability of remaining childless. Hence, educational expectations may result in delayed family formation and as a result in lower fertility than desired. Time preferences and cognitive ability are not associated with a higher risk of missing one's fertility target. On both the extensive and the intensive margin, early and later relationship outcomes are associated with sizeable and significant decreases in the risk of missing one's target and of remaining childless. Specifically, marrying is correlated with a lower probability of childlessness and of missing one's fertility target, while having no partner in early adulthood increases the probability of remaining childless and as result the probability of fertility below desires. Finally, having had more than one stable relationship during adult

Table 24: Risk of childlessness, Sample 3

Variable	(1) Coef.	(1) SE	(2) Coef.	(2) SE	(3) Coef.	(3) SE	(4) Coef.	(4) SE
Family type, ability								
(Ref. desire, medium*)								
Small, high	0.0526*	(0.0315)	0.0269	(0.0360)	0.0199	(0.0358)	0.0440	(0.0338)
Large, high	-0.0157	(0.0302)	-0.0382	(0.0331)	-0.0317	(0.0319)	0.0168	(0.0291)
Small, low	0.0553*	(0.0309)	0.0769**	(0.0341)	0.0681**	(0.0321)	0.0576*	(0.0307)
Large, low	-0.0443	(0.0307)	-0.0214	(0.0348)	-0.0212	(0.0326)	-0.0025	(0.0269)
Earnings pot.								
Exp. USE			0.0075	(0.0288)	0.0009	(0.0285)	-0.0330	(0.0242)
Time prefs.			-0.0117	(0.0143)	-0.0165	(0.0140)	-0.0058	(0.0128)
Rosen score*			-0.0183**	(0.0089)	-0.0158*	(0.0087)	-0.0059	(0.0073)
Ability**								
Spatial			0.0216*	(0.0118)	0.0185	(0.0117)	0.0150	(0.0107)
Verbal			0.0137	(0.0142)	0.0121	(0.0135)	0.0137	(0.0126)
Early rela.								
(1976)								
No relationship					0.1631***	(0.0359)	0.1059***	(0.0325)
Consid./married					-0.0521***	(0.0180)	-0.0280*	(0.0164)
Late rela.								
Ever married							-0.4335***	(0.0600)
>1 cohab/marriage							0.0300*	(0.0179)
Constant	0.1022***	(0.0121)	0.1210***	(0.0324)	0.1264***	(0.0340)	0.5648***	(0.1143)
Controls							X	
Fix							X	
No. of obs.	1119		1064		1064		1012	
R^2	0.01		0.02		0.07		0.23	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birthyear fixed effects.

life only affects only the risk of missing one's target, not the risk of remaining childless. My results suggest that it is rather matching risks than biological risks driving fertility deficits among women with a strong taste for education.

Having established a direct link between fertility desires and family formation outcomes, including timing of childbearing, realized fertility and the risk of missing one's fertility target, I continue with an analysis of the association between fertility desires and economic outcomes, including education and labor market outcomes.

5.3 Results on education

In this section, I review my results on the link between educational attainment and desires, particularly I test the hypothesis that completed levels of secondary and highest education are orthogonal to desired family type, once factors related to the individual's earnings potential, such as time preferences, ability and socio-economic background, are taken into consideration. The empirical results on secondary education and desired family type are found in Tables 25 and 27, while results on highest completed education are reported in Tables 28 and 29. Finally, empirical results from the estimation of a dummy for USE completion on desired family type can be found in Appendix A.5. In a second step, I test the hypothesis that choice of occupation as proxied by field of study is linked to desired family type due to beliefs on occupation-specific childrearing compatibility. Results on field of study are found in Tables 30 and 31.

5.3.1 Secondary Education

I aim to test the hypothesis that completed level of secondary education is unrelated to desired family type conditional on individual earnings potential. Descriptives on the distribution of desired family type illustrated that both desire for no family and desire for a large family are more common among individuals with a high cognitive ability test and a high taste for secondary education. Since cognitive ability and educational expectations are likely to be positively correlated with level of completed secondary education, I expect both no desire and desire for a large family to correlate positively with completed level of secondary education, as long as earnings potential is included in the error term . Results in column (1) of Table 25 confirm this intuition, but the associations between desired family type and completed level of secondary education are not statistically significant compared to the reference group desiring a small family. In a second step, I test whether desiring a small or a large family relative to having no desire for a family is negatively correlated with level of secondary education. From results in column (2) of Table 25, we see that again, the intuition is confirmed, but coefficients are insignificant. Finally, I test whether desiring a family versus having no desire is linked with completed level of secondary education. Results in column (3) of Table 25 are confirmative of a negative

association, and the coefficient of .28 units is statistically significant at the 10%-level. Given that level of education is measured on a 7-step scale with a sample mean of 3.8, the coefficient is equivalent to a significant reduction in level of secondary education of 7%.

Table 25: Level of Secondary Education, Sample 1

Variable	(1) Coef.	(1) SE	(2) Coef.	(2) SE	(3) Coef.	(3) SE
Desired family type						
(Ref. small family)						
No desire	0.2340	(0.1613)				
Large family	0.0458	(0.0898)				
Don't know	1.1028***	(0.2849)				
Mom: No Desire	-1.2624***	(0.1915)				
Mom: Desire	-0.7611***	(0.0889)				
Desired family type						
(Ref. No desire)						
Small family			-0.1626	(0.1665)		
Large family			-0.1248	(0.1741)		
Don't know			0.9932***	(0.3247)		
Mom: No Desire			-1.2012***	(0.1950)		
Mom: Desire			-0.7687***	(0.0888)		
Taste for fertility						
Desire					-0.2794*	(0.1643)
Mom: No Desire					-1.3489***	(0.1955)
Mom: Desire					-0.7723***	(0.0884)
Constant	3.9442***	(0.0603)	4.1170***	(0.1552)	4.2647***	(0.1558)
No. of obs.	1292		1292		1292	
R^2	0.09		0.09		0.08	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level.

However, as seen from results in column (2) in Table 27, the negative association between desiring a family and level of secondary education is not robust to controlling for factors related to individual earnings potential. Once time preferences, taste for education, measures of ability and Rosenberg's self-esteem score (standardized) are controlled for, the sign of the coefficient on desire changes to positive and is no longer statistically significant. Hence desiring a family is orthogonal to level of secondary education, when controlling for individual earnings potential.

Moreover, the results in column (2) of Table 27 demonstrate that the individual's earnings potential is the main driver of educational accomplishments. The individual's own educational expectations have a strong positive association with completed level of

Table 26: Level of Secondary Education, Sample 1

	(1)		(2)		(3)	
Variable	Coef.	SE	Coef.	SE	Coef.	SE
Taste for fertility						
Desire	-0.2794*	(0.1643)	-0.0404	(0.1570)	0.0919	(0.1690)
Mom: No Desire	-1.3489***	(0.1955)	-0.6268***	(0.1961)	-0.0961	(0.2112)
Mom: Desire	-0.7723***	(0.0884)	-0.4595***	(0.0807)	-0.2830***	(0.0882)
Earnings potential						
Time prefs.			0.1284**	(0.0544)	0.0999*	(0.0553)
Rosen score**			-0.0856**	(0.0342)	-0.0779**	(0.0353)
Own educ. exp.						
(Ref. 7-8th/Miss.)						
Exp. 9th/10th			0.5616***	(0.1480)	0.1739	(0.1526)
Exp. LSE			0.6276***	(0.1626)	0.2524	(0.1891)
Exp. USE			1.5983***	(0.1921)	0.5311**	(0.2313)
Ability*						
Cognitive			0.1962***	(0.0510)	0.1558***	(0.0499)
Spatial			0.0431	(0.0442)	0.0187	(0.0454)
Verbal			0.1904***	(0.0555)	0.1314**	(0.0569)
Par. exp.						
(Ref. <LSE)						
Exp. 9th					0.9446***	(0.1390)
Exp. 10th					1.3448***	(0.1415)
Exp. LSE					1.0697***	(0.1729)
Exp. USE					2.1153***	(0.2183)
Early relationships						
(1973)						
Marr./cohab					-0.2436**	(0.1192)
Partner					-0.1815**	(0.0816)
Constant	4.2647***	(0.1558)	2.9193***	(0.2369)	2.1758***	(0.3234)
Controls					X	
Fix					X	
No. of obs.	1292		1225		1061	
R^2	0.08		0.30		0.40	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Ability is measured by standardized test scores from 1968. **Standardized Rosenberg's self-esteem score from 1973. Controls are Father's education, mother's education, father's occupation, dummy for working mother, siblings, parity, dummy for growing up with both parents, and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Fixed effects for birth year.

secondary education. Specifically, expecting to complete USE is associated with an increase of 1.6 units equivalent to a 40% increase in completed level of secondary education relative to the sample mean. In addition, time preferences, as well as both cognitive and verbal ability are positively and significantly associated with completed level of secondary education. Finally, the individual's score on Rosenberg's self-esteem test is significantly

and negatively correlated with completed level of secondary education in line with expectations. From results in column (3) of Table 27, we see that parental expectations are even more important for completed level of secondary education than the respondent's own educational expectations. This may just be an artifact of parental expectations being elicited one year later than own expectations, i.e. after secondary education has been initiated. However, it appears that the expectations of individuals and parents are substitutes, as coefficients on own expectations drop, when parental expectations are included as controls. The results in column (3) of Table 27 also demonstrate that completed level of secondary education is negatively correlated with early family formation outcomes such as being married or having found a partner by age 19. This confirms earlier findings that time of graduation is strongly correlated with formation of relationships (Geruso and Royer, 2018; Black et al., 2008). Moreover, I find a large negative association between being an early mother with additional desire and level of secondary education across specifications, while controlling for earnings potential renders the negative association between level of education and being an early mother with no additional desire insignificant. This indicates that a lower earnings potential among early mothers with no additional desire is responsible for the negative association with completed level of education.

5.3.2 Highest Completed Education

Considering now the link between level of highest completed education and desired family type, we see from results in column (1) of Table 28 that having no desire for a family is positively associated with completed level of education relative to the reference group desiring a small family. This finding is in line with descriptive results showing that having no desire is more prevalent among women with higher educational aspirations and higher ability. Keeping in mind that highest completed education is measured on a 5-level scale ranging from *Less than USE* to *MA or above*, we see that the statistically significant coefficient of 1/3 of a unit is equivalent to an increase of 12% compared to the sample mean of 2.44. Desiring a large family is also positively and significantly associated with completed level of education relative to the reference group desiring a small family, though the coefficient is half-sized. The association, which is equivalent to an increase of 6%, is statistically

Table 27: Level of Secondary Education, Sample 1

Variable	(1) Coef.	(1) SE	(2) Coef.	(2) SE	(3) Coef.	(3) SE
Taste for fertility						
Desire	-0.2794*	(0.1643)	-0.0404	(0.1570)	0.0919	(0.1690)
Mom: No Desire	-1.3489***	(0.1955)	-0.6268***	(0.1961)	-0.0961	(0.2112)
Mom: Desire	-0.7723***	(0.0884)	-0.4595***	(0.0807)	-0.2830***	(0.0882)
Earnings potential						
Time prefs.			0.1284**	(0.0544)	0.0999*	(0.0553)
Rosen score**			-0.0856**	(0.0342)	-0.0779**	(0.0353)
Own educ. exp.						
(Ref. 7-8th/Miss.)						
Exp. 9th/10th			0.5616***	(0.1480)	0.1739	(0.1526)
Exp. LSE			0.6276***	(0.1626)	0.2524	(0.1891)
Exp. USE			1.5983***	(0.1921)	0.5311**	(0.2313)
Ability*						
Cognitive			0.1962***	(0.0510)	0.1558***	(0.0499)
Spatial			0.0431	(0.0442)	0.0187	(0.0454)
Verbal			0.1904***	(0.0555)	0.1314**	(0.0569)
Par. exp.						
(Ref. <LSE)						
Exp. 9th					0.9446***	(0.1390)
Exp. 10th					1.3448***	(0.1415)
Exp. LSE					1.0697***	(0.1729)
Exp. USE					2.1153***	(0.2183)
Early relationships						
(1973)						
Marr./cohab					-0.2436**	(0.1192)
Partner					-0.1815**	(0.0816)
Constant	4.2647***	(0.1558)	2.9193***	(0.2369)	2.1758***	(0.3234)
Controls					X	
Fix					X	
No. of obs.	1292		1225		1061	
R^2	0.08		0.30		0.40	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Ability is measured by standardized test scores from 1968. **Standardized Rosenberg's self-esteem score from 1973. Controls are Father's education, mother's education, father's occupation, dummy for working mother, siblings, parity, dummy for growing up with both parents, and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Fixed effects for birth year.

significant at the 5%-level. From results in column (2), we see that the difference in coefficients on desired family type is not significant, when comparing individuals with no desire and individuals desiring a large family. Instead desiring a small family is associated with a reduction in level of education of 1/3 relative to individuals with no desire. This result is unsurprising given symmetry of results in columns (1) and (2). Finally, from results

in column (3) of Table 28 we see that desiring a family is associated with a reduction in completed level of education of 1/3, which is likely to be driven by individuals desiring a small family. Being an early mother is associated with large and significant reductions in completed level of education across the specifications presented in Table 28, while not knowing one's desires is associated with large and significant increases in completed level of education, cf. results in columns (1) and (2).

Table 28: Level of Completed Education, Sample 1

Variable	(1) Coef.	(1) SE	(2) Coef.	(2) SE	(3) Coef.	(3) SE
Desired family type						
(Ref. small)						
No desire	0.3278**	(0.1510)				
Large family	0.1636**	(0.0823)				
Don't know	0.6843***	(0.2224)				
Mom: Desire	-0.7454***	(0.0815)				
Mom: No Desire	-1.1499***	(0.1709)				
Desired family type						
(Ref. No desire)						
Small family			-0.3108*	(0.1591)		
Large family			-0.1491	(0.1656)		
Don't know			0.4585*	(0.2645)		
Mom: Desire			-0.7482***	(0.0816)		
Mom: No Desire			-1.1356***	(0.1765)		
Taste for fertility						
Desire					-0.3104**	(0.1488)
Mom: Desire					-0.7453***	(0.0813)
Mom: No Desire					-1.2036***	(0.1698)
Constant	2.5695***	(0.0549)	2.8830***	(0.1492)	2.9510***	(0.1412)
No. of obs.	1292		1292		1292	
R^2	0.09		0.09		0.08	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level.

Relying on the specification in column (1) of Table 28 and including factors related to the individual's earnings potential, results in column (1) of Table 29 confirm that once individual earnings potential is controlled for, desired family type is no longer significantly correlated with completed education. Though coefficients on *no desire* and *desiring a large family* remain positive, they are small and no longer statistically significant. Instead, educational expectations, as well as cognitive and verbal ability are identified as key drivers

of educational attainment. Since elicited expectations concern secondary education, the strong association with highest completed education could reflect that many individuals graduate with secondary education or less or that aspirations for secondary education pave the way for a continued educational path.Likewise parental expectations for secondary education are strongly and positively associated with completed level of highest education as seen from results in column (3) of Table 29. Time preferences, which were positively associated with level of secondary education, are only weakly associated with final level of education across specifications. This suggests that above a certain level of education, time preferences gain less importance. As seen from results in column (3), early cohabitation and particularly early marriage are strongly associated with reductions in completed level of education. Finally, the completion of USE is positively correlated with final level of education with a coefficient of 1.3, equivalent to an increase of 50% relative to the sample mean. Including the outcome USE graduation as a control, naturally reduces coefficients on own and parental expectations for secondary education.

To sum up, results on the completion of education demonstrate that levels of secondary and final education are practically uncorrelated with family formation desires, once the individual's earnings potential is taken into consideration, eliminating the bias arising from correlation between desires and individual earnings potential. Completion of education is primarily driven by educational aspirations, parental expectations, cognitive and verbal ability as well as time preferences and self-esteem of the individual, in line with previous evidence (Epper et al., 2019; Golsteyn et al., 2014; Heckman et al., 2006). The completion of education, particularly obligatory schooling, is however closely linked with early relationship and family formation outcomes, including early marriage and early motherhood in line with findings of Geruso and Royer (2018) and Black et al. (2008).

5.3.3 Field of study

Assuming that field of study can be used as a proxy for an individual's occupational choice, a regression of field of study on desired family type is used to test my final hypothesis related to education as inspired by Adda et al. (2017), namely that individuals desiring

Table 29: Level of Completed Education, Sample 1

	(1)		(2)		(3)		(4)	
Variable	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Desired family type								
(Ref. Small family)								
No desire	0.0924	(0.1396)	0.0396	(0.1466)	-0.0068	(0.1303)	-0.0390	(0.1406)
Large family	0.0463	(0.0756)	0.0987	(0.0789)	0.0787	(0.0716)	0.0659	(0.0747)
Don't know	0.4146**	(0.2092)	0.3626*	(0.2031)	0.1114	(0.1791)	0.0206	(0.1873)
Mom: Desire	-0.4795***	(0.0793)	-0.4776***	(0.0866)	-0.1898**	(0.0868)	-0.1624*	(0.0864)
Mom: No Desire	-0.5263***	(0.1677)	-0.3959**	(0.1812)	-0.1338	(0.1693)	-0.0725	(0.1770)
Eanings potential								
Time prefs.	0.0541	(0.0476)	0.0573	(0.0500)	0.0305	(0.0450)	0.0195	(0.0449)
Rosen score*	-0.0880***	(0.0324)	-0.0842**	(0.0336)	-0.0525*	(0.0310)	-0.0588*	(0.0313)
Own educ. exp.								
(Ref. <LSE/Miss.)								
Exp. LSE	0.3903***	(0.0832)	0.1528	(0.1345)	0.0731	(0.1235)	0.0725	(0.1339)
Exp. USE	0.9922***	(0.1150)	0.4388***	(0.1674)	0.1866	(0.1496)	0.1357	(0.1635)
Ability**								
Cognitive	0.1499***	(0.0474)	0.0939*	(0.0490)	0.0321	(0.0442)	0.0485	(0.0449)
Spatial	-0.0083	(0.0419)	0.0196	(0.0440)	0.0311	(0.0400)	0.0373	(0.0404)
Verbal	0.2099***	(0.0495)	0.1617***	(0.0536)	0.1339***	(0.0491)	0.1032**	(0.0493)
Par. exp.								
(Ref. <LSE)								
Exp. LSE			0.3407**	(0.1358)	0.1957	(0.1262)	0.2033	(0.1328)
Exp. USE			0.9566***	(0.1661)	0.3784**	(0.1583)	0.3092*	(0.1646)
Rela., 1976								
Cohab.					-0.1978**	(0.0790)	-0.1956**	(0.0798)
Marr.					-0.4098***	(0.0879)	-0.3938***	(0.0877)
USE status								
USE grad.					1.2715***	(0.0975)	1.2756***	(0.1004)
Constant	2.0667***	(0.1087)	1.9465***	(0.1133)	2.0667***	(0.1137)	1.8784***	(0.3013)
Controls							X	
Fix							X	
No. of obs.	1225		1086		1086		1061	
R^2	0.28		0.31		0.44		0.46	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Standardized Rosenberg's self-esteem score from 1973. **Ability is measured by standardized test scores from 1968. Controls are Father's education, mother's education, father's occupation, dummy for working mother, siblings, parity, dummy for growing up with both parents, and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Fixed effects for birth year.

a small family are more likely to select into more abstract occupations, while individuals desiring a large family are more likely to select into less abstract occupations.

As described in Section 4.2 individuals in my sample are concentrated in a handful of fields of study, reflecting gender divides prevalent for the cohort in question as well as a higher frequency of women in public sector occupations. To test the hypothesis that

individuals desiring a small family are more likely to select into abstract occupations, I regress a dummy of completing an education in the field *Business* on desired family type. From column (1) in Table 30, we see that desiring a large family is associated with a significantly lower probability of studying within *Business*. Individuals desiring a large family are 6 percentage points less likely to study within *Business* than individuals in the reference group desiring a small family. Surprisingly, the association with having no desire for a family is also negative and significant. The subgroup with no desire for a family is on average 10 percentage points less likely to study within *Business* than individuals desiring a small family. By symmetry, individuals desiring a small family are overall 6.6 percentage points more likely to study within the field of *Business* compared to other individuals in the sample as seen from results in column (2) of Table 30. Given that only 1/5 of all respondents study Business these effects are fairly large.

Considering now *Teaching and learning* as an example of a field, which is representative of occupations with greater childrearing compatibility, from column (3) of Table 30 we see that both desiring no family and desiring a large family are associated with increases of 4 percentage points in the probability of studying within *Teaching* compared to the reference group desiring a small family. Individuals with unknown desires are 7 percentage points more likely to study within *Teaching* than the reference group. By symmetry, the association is similar in size and statistical significance (-4.6 percentage points), though with the opposite sign, for individuals desiring a small family compared to other individuals. In order to assess the robustness of my results, I repeat the previous estimations controlling for individual earnings potential, completed level of education and socio-economic background variables.

As seen from Table 31, the associations between field of study and desired family type are robust to controlling for earnings potential, socio-economic background and level of education. As seen from columns (1) and (3) in Table 31 desiring a large family is associated with a decrease of 5 percentage points in the probability of studying within *Business* and an increase of 3 percentage points in the probability of studying within *Teaching*, compared to the reference group desiring a small family. By symmetry, the effects are of similar size and signficance, but with the opposite sign for individuals desiring

Table 30: Field of study, Sample 1

Variable	Business, econ. and law				Teaching and learning			
	(1)		(2)		(3)		(4)	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Family type desired								
(Ref. Small family)								
No family desired	-0.0980**	(0.0413)			0.0389*	(0.0236)		
Large family	-0.0627**	(0.0247)			0.0385***	(0.0141)		
Don't know	-0.0323	(0.0719)			0.0731*	(0.0411)		
Mom: Desire	-0.0136	(0.0270)			-0.0472***	(0.0154)		
Mom: No Desire	0.0413	(0.0554)			-0.0673**	(0.0316)		
Family type desired								
(Ref. Not small family)								
Small family			0.0663***	(0.0227)			-0.0462***	(0.0129)
Mom: Desire			-0.0098	(0.0266)			-0.0480***	(0.0152)
Mom: No Desire			0.0117	(0.0438)			-0.0727***	(0.0250)
Constant	0.2234***	(0.0161)	0.1549***	(0.0181)	0.0492***	(0.0092)	0.0936***	(0.0103)
No. of obs.	1312		1312		1312		1312	
R^2	0.01		0.01		0.02		0.02	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level.

a small family, cf. results in columns (2) and (4). Individuals with no desire for a family are 9.4 percentage points less likely to study within *Business* once controls are included. However, the association between studying within *Teaching* and having no desire for a family is reduced and no longer statistically significant, when controls are included. Thus, it would appear that the initial coefficient was upward biased, reflecting a positive correlation between earnings potential (or level of education) and studying within the field of *Teaching*. To sum up, I find suggestive evidence that individuals desiring a large family are more likely to select into less abstract (and public sector) occupations, while individuals desiring a small family are more likely to select into more abstract (and private sector) occupations. Surprisingly, individuals with no desire for a family are even less likely to study within the field of *Business*, which contradicts my theoretical prediction that fertility desires are uncorrelated with fertility desires for individuals with no desire. [18]

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[18]I find no significant results for the fields of *Social Services and Health* nor for *Primary School Teaching*. This may reflect the composition within fields, e.g., the relative share of early mothers in the field of primary school is much higher than that of other respondents.

Table 31: Field of study, Sample 1

| | Business, econ. and law | | | | Teaching and learning | | | |
| | (1) | | (2) | | (3) | | (4) | |
Variable	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Family type desired								
(Ref. Small family)								
No family	-0.0940**	(0.0457)			0.0223	(0.0253)		
Large family	-0.0502*	(0.0268)			0.0312**	(0.0149)		
Don't know	-0.0049	(0.0775)			0.0499	(0.0430)		
Mom: Desire	-0.0481	(0.0300)			-0.0017	(0.0167)		
Mom: No Desire	0.0061	(0.0618)			0.0034	(0.0343)		
Family type desired								
(Ref. Not small family)								
Small family			0.0544**	(0.0247)			-0.0364***	(0.0137)
Mom: Desire			-0.0441	(0.0297)			-0.0012	(0.0164)
Mom: No Desire			-0.0318	(0.0488)			-0.0083	(0.0270)
Level of education	-0.0178*	(0.0104)	-0.0175*	(0.0104)	0.0437***	(0.0058)	0.0438***	(0.0058)
Constant	0.1536	(0.1109)	0.0974	(0.1113)	-0.0870	(0.0615)	-0.0517	(0.0617)
Earnings potential*	X		X		X		X	
Controls	X		X		X		X	
Fix	X		X		X		X	
No. of obs.	1197		1197		1197		1197	
R^2	0.04		0.04		0.13		0.13	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. Controls, SES are mother's education, parity, siblings, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Controls, other are patience, educational expectations, ability (cognitive, verbal and spatial), Rosenberg's self-esteem score. Fix is birth year fixed effects.

5.4 Results on income

The next results I present concern the association between desired family type and individual labor market outcomes. I test the hypothesis that cumulative earnings over working career are decreasing in fertility desires, such that desiring a large family is associated with lower cumulative income over working career relative to desiring a small family. I also examine if realized fertility generates income reductions over and above those originating from desired family type. I consider two measures of income over working career, namely cumulative gross income and cumulative wage income as presented in Subsection 4.3. The first measure combines labor market earnings, capital gains and public transfers, while wage earnings is a direct measure of income from paid employment. In the Appendix A.5, I supplement with results on after-tax (net) income and labor earnings more broadly. The results on after-tax income are slightly weaker due to equalizing measures in the tax and

transfer system, but still broadly in line with results found for gross income, while results on labor market earnings are similar to those found for wage earnings.

5.4.1 Gross income

Regarding the association between desired family type and gross income, we see from results in column (1) of Table 32 that women desiring a large family suffer a cumulative income loss of 2 mill. DKK in 2019-price levels over working career compared to the reference group of women desiring a small family. The income reduction is statistically significant at the 5%-level and is equivalent to a reduction of 8% in cumulative gross income compared to the sample mean of 25.3 mill. DKK over working career. Surprisingly, women with no desire for a family fare even worse. Having no desire for a family is associated with a reduction in cumulative gross income over working career of 2.8 mill. DKK equivalent to 11% of the sample mean. The association is statistically significant at the 5%-level. Early mothers suffer additional income reductions of 1.4 mill. DKK for early mothers with no additional desire and 2.5 mill. DKK for early mothers with additional desire. Only for early mothers with additional desire is the extra income reduction statistically significant. Moreover, results in column (1) in Table 32 document that time preferences, cognitive skills and educational expectations in adolescence are associated with sizeable and significant increases in cumulative gross income across working career. This is in line with findings of Golsteyn et al. (2014) and Epper et al. (2020) on the link between preferences in adolescence and long term economic outcomes. As seen from results in column (2) of Table 32, the negative associations between desired family type and income remain sizeable and statistically significant at 5% (large family) and 8% (no desire) of sample mean, when controls for family formation and completed education are included.

Table 32: Cumulative gross income, 1980-2017 in mill. DKK 2019-levels, Sample 1

Variable	(1) Coef.	(1) SE	(2) Coef.	(2) SE	(3) Coef.	(3) SE
Family type						
(Ref. Small family)						
No desire	-2.7643**	(1.4088)	-1.8518*	(1.1117)	-1.0057	(0.7540)
Large family	-2.0212**	(1.0251)	-1.3790**	(0.6655)	-0.4651	(0.4828)
Don't know	1.7029	(2.0063)	1.5852	(1.8444)	0.5203	(1.8750)
Mom: No Desire	-1.4464	(1.2709)	-0.4218	(1.3087)	0.6325	(0.9392)
Mom: Desire	-2.4707***	(0.8137)	-0.7297	(0.5931)	-0.4924	(0.5036)
Earning pot.						
Time prefs.	1.0782***	(0.4083)	0.8132**	(0.3765)	0.2453	(0.2954)
Std. cog. skills	1.4374***	(0.3749)	1.0063***	(0.3330)	0.7159***	(0.2389)
Own educ. exp.						
(Ref. <LSE/Miss.)						
Exp. LSE	3.7430***	(1.4008)	1.3923**	(0.5931)	0.7992**	(0.4042)
Exp. USE	5.7709***	(1.0123)	2.7246***	(0.9954)	2.7178***	(0.7957)
Family formation						
Consid./married			-1.2198**	(0.5512)	-0.3357	(0.3742)
Ever married			3.2445**	(1.3055)	2.1404**	(0.9545)
No. of childr.			-0.1223	(0.2767)	0.0634	(0.2174)
Completed educ.						
ref. 7-8th/miss.						
LSE			2.9043**	(1.4446)	-1.8715	(3.5607)
USE/VOC			4.5996***	(1.4490)	-1.3704	(3.5451)
Short cycle			5.3996***	(1.9217)	-1.6077	(3.6945)
BA			7.4743***	(1.4379)	-0.5636	(3.5418)
Master or above			18.2184***	(3.3478)	12.0797***	(4.3461)
Labor market, 1976-2017						
Age at entry					0.3039**	(0.1264)
Avg. empl. degr.					19.1590***	(6.1710)
No. of jobs					0.1370**	(0.0621)
Share w/ part time					-8.2503***	(1.9927)
Share w/ no empl.					3.0562	(7.5302)
Occupation, 1976-2017, years						
(Ref. Missing)						
Independent					1.1844***	(0.1783)
Ass. spouse					0.8903***	(0.1238)
Employed with company					0.6618***	(0.1357)
Employed					0.9205***	(0.0974)
Employed with UI					0.6385***	(0.1874)
Retired with company					0.8199***	(0.1598)
Retired					0.9090***	(0.1234)
Other					0.5122***	(0.1383)
Early retirement					0.3106**	(0.1580)
Unemployed					0.7786***	(0.1650)
Unemployed with UI					0.5263***	(0.1971)
Unemployed with SA					0.9411***	(0.1610)
DLSY, 2001						
Ever manager					1.2958***	(0.3678)
Constant	22.3356***	(1.0415)	12.6555***	(2.7484)	-31.5082***	(10.2686)
Controls			X		X	
Fix			X		X	
No. of obs.	1257		1197		1038	
R^2	0.05		0.26		0.61	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Relation status in 1976: Married or has partner and considers marriage. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

As seen from Table 33, the results are robust across samples 1,2, and 3, though the estimated reduction in cumulative gross income for women desiring a large family is slightly larger, when reducing the sample to individuals desiring a family (sample 3). As seen from column (2) of Table 32, early considerations of marriage and early marriage as such are associated with reduced cumulative gross income, while marriage overall is associated with increases in gross income. A negative association between realized fertility and gross income is found, but the association is small and statistically insignificant. In isolation, the significant associations between desired family type and income imply that income reductions related to childbearing are (almost) fully foreseen. Level of completed education is found to have large and positive effects on cumulative gross income. This reflects that higher education is correlated with stronger labor market outcomes.In column (3) of Table 32 labor market outcomes are included as controls. Once outcomes such as age at employment entry, average labor supply, share of career with part-time employment and with no employment are included as controls, the income reduction associated with desiring a large family is reduced significantly to -0.5 mill. DKK and the estimate is no longer statistically significant. This suggests that the effect of desired family type on income is transmitted through labor market outcomes across working career. Surprisingly, the negative association between gross income and having no desire for a family remains sizeable (8% of sample mean) and significant even after controlling for a broad set of labor market factors. This suggests that income reductions associated with having no desire for a family are not the result of significantly different labor market outcomes relative to the reference group desiring a small family. Hence, other unobserved factors must play a role.

5.4.2 Wage earnings

Considering now the association between desired family type and cumulative wage earnings over career, from results in column (1) of Table 34 we see that desiring a large family is associated with a sizeable reduction in cumulative wage income of 0.66 mill. DKK in 2019-price levels over working career compared to the reference group desiring a small family. This is equivalent to a 7% reduction compared to the sample mean wage earnings

Table 33: Cumulative gross income 1980-2017 in mill. DKK, 2019 price-level)

	(1) Sample 1		(2) Sample 2		(3) Sample 3	
Variable	Coef.	SE	Coef.	SE	Coef.	SE
Family type						
(Ref. Small)						
No desire	-1.8518*	(1.1117)	-1.9056*	(1.1149)		
Large family	-1.3790**	(0.6655)	-1.4099**	(0.6729)	-1.4753**	(0.6731)
Don't know	1.5852	(1.8444)	1.5892	(1.8506)		
Mom: No Desire	-0.4218	(1.3087)				
Mom: Desire	-0.7297	(0.5931)	-0.6973	(0.6015)	-0.5241	(0.6025)
Earning pot.						
Time prefs.	0.8132**	(0.3765)	0.7801*	(0.4060)	0.6816	(0.4241)
Std. cog. skills	1.0063***	(0.3330)	1.1761***	(0.3604)	1.2726***	(0.3654)
Own. SE exp.						
(Ref. <LSE/Miss.)						
Exp. LSE	1.3923**	(0.5931)	1.2408**	(0.6167)	1.0409	(0.6397)
Exp. USE	2.7246***	(0.9954)	2.4858**	(1.0373)	3.1106***	(1.0948)
Family formation						
Consid./married	-1.2198**	(0.5512)	-1.4489**	(0.5858)	-1.5080**	(0.6030)
Ever married	3.2445**	(1.3055)	3.2213**	(1.3075)	3.1058**	(1.3849)
No. of childr.	-0.1223	(0.2767)	-0.0764	(0.2958)	-0.0360	(0.3063)
Completed educ.						
ref. 7-8th/miss.						
LSE	2.9043**	(1.4446)	3.6711**	(1.6220)	3.7700**	(1.6231)
USE/VOC	4.5996***	(1.4490)	5.1404***	(1.6403)	5.4690***	(1.6427)
Short cycle	5.3996***	(1.9217)	6.1349***	(2.0997)	6.8687***	(2.0979)
BA	7.4743***	(1.4379)	8.1786***	(1.6039)	8.5828***	(1.6318)
MA or above	18.2184***	(3.3478)	18.8763***	(3.4720)	20.3328***	(3.7342)
Constant	12.6555***	(2.7484)	12.3380***	(2.8668)	12.7258***	(2.8708)
Controls	X		X		X	
Fix	X		X		X	
No. of obs.	1197		1107		1025	
R^2	0.26		0.26		0.28	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level.*Relation status in 1976: Married or has partner and considers marriage. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birthyear fixed effects.

of 9.0 mill. DKK over working career. The negative association is statistically significant at the 5%-level. The relative reduction in wage earnings is similar in size to the reduction found for gross income above. The results suggest that lower cumulative wage earnings over career can, at least partially, explain the reduction in cumulative gross income found above.

Table 34: Cumulative wage earnings, 1980-2017 in mill. DKK 2019-levels, Sample 1

Variable	(1) Coef.	(1) SE	(2) Coef.	(2) SE	(3) Coef.	(3) SE
Family type						
(Ref. Small family)						
No desire	-0.5925	(0.5565)	-0.1950	(0.5323)	-0.0024	(0.2754)
Large family	-0.6622**	(0.3033)	-0.6081**	(0.2965)	-0.0492	(0.1419)
Don't know	0.1818	(1.0165)	0.4300	(0.9056)	0.5237	(0.5806)
Mom: No Desire	-1.1543*	(0.6719)	-0.9392	(0.6908)	0.3518	(0.3560)
Mom: Desire	-0.9000***	(0.3070)	-0.3733	(0.3123)	0.0236	(0.1672)
Own educ. exp.						
(Ref. <LSE/Miss.)						
Time prefs.	0.3928**	(0.1978)	0.2519	(0.1884)	0.0008	(0.0971)
Std. cog. skills	0.5287***	(0.1559)	0.3127**	(0.1544)	0.2236***	(0.0729)
Earning pot.						
Exp. LSE	1.5541***	(0.2945)	0.8752***	(0.2954)	0.2546**	(0.1279)
Exp. USE	1.9718***	(0.4421)	0.7529*	(0.4521)	0.5811***	(0.2233)
Family formation						
Consid./married			-0.1668	(0.2663)	0.0441	(0.1228)
Ever married			1.6589***	(0.5911)	0.4990*	(0.2641)
No. of childr.			-0.3068**	(0.1356)	-0.0350	(0.0684)
Completed educ.						
ref. 7-8th/miss.						
LSE			6.8588***	(0.7481)	-0.9856	(0.9818)
USE/VOC			7.7848***	(0.7494)	-0.8549	(0.9774)
Short cycle			8.2869***	(0.8803)	-1.1050	(1.0249)
BA			9.4841***	(0.7270)	-0.3054	(0.9928)
MA or above			11.0422***	(1.1410)	2.6975**	(1.1653)
Labor market, 1976-2017						
Age at entry					0.1331***	(0.0420)
Avg. empl. degr.					4.2900***	(0.9284)
No. of jobs					0.0657***	(0.0173)
Share w/ part time					-5.0877***	(0.4583)
Share w/ no empl.					-6.0459***	(1.6302)
Occupation, 1976-2017, years						
(Ref. Missing)						
Independent					0.0096	(0.0342)
Ass. spouse					0.0554	(0.0375)
Employed with company					0.2852***	(0.0378)
Employed					0.3009***	(0.0287)
Employed with UI					0.1137*	(0.0649)
Retired with company					0.2130***	(0.0722)
Retired					0.1834***	(0.0379)
Other					0.1904***	(0.0423)
Early retirement					-0.0625	(0.0517)
Unemployed					0.1558***	(0.0501)
Unemployed with UI					-0.0465	(0.0587)
Unemployed with SA					0.1977***	(0.0509)
DLSY, 2001						
Ever manager					0.4532***	(0.1203)
Constant	7.8277***	(0.4421)	-2.2461*	(1.1838)	-4.4816**	(2.0973)
Controls			X		X	
Fix			X		X	
No. of obs.	1275		1207		1038	
R^2	0.10		0.21		0.83	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Relation status in 1976: Married or has partner and considers marriage. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

As seen from results in column (2) of Table 34, the reduction in wage income following from desires is robust to controlling for family formation and completed level of education. It amounts to 0.61 mill. DKK (6.7% of sample mean) and remains statistically significant at the 5%-level. At the same time, the results in column (2) demonstrate that realized fertility is associated with income reductions of 0.31 mill. DKK per childbirth. This significant coefficient on realized fertility indicates that the consequences of childbirth on wage income are not fully predictable from individual fertility desires. Instead, individuals on average experience unforeseen wage reductions equivalent to 3.4% of mean wage income per realized childbirth. Given that my income data does not cover initial childbearing years, the estimated reduction in wage earnings following from realized childbirth is likely to be a lower bound.[19] From Table 35 we see that both wage reductions following from desired family type and from realized fertility are statistically robust across samples 1 and 2. However, for women with positive desires for a family (sample 3), the negative association between desiring a large family and wage earnings is slightly larger at 0.68 mill. DKK, while the association between realized fertility and wage earnings is lower at -.2 equivalent to 2.3% of sample mean income, and it is no longer statistically significant. This suggests that the unforeseen negative consequences on wage earnings following from realized childbirth are somewhat larger among individuals with no desire for a family, i.e. among individuals, who are not expecting to have a family.From results in column (2) of Table 34, we see that completed educational level and marriage are associated with large and statistically significant increases in cumulative wage income. Finally, from column (3) in Table 34, we see that both wage reductions following from desired family type and from realized fertility are almost entirely eliminated once labor market outcomes such as age at employment entry, average labor supply, share of career with part-time employment and with no employment are included as controls. Hence, reduced wage income over career associated with desired family type and realized fertility is primarily driven by differences in labor market outcomes over career. The association between cumulative wage income over career and having no desire for a family is sizable and negative at 6.5%

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[19]More than half of individuals in sample 1, who realize fertility, have their first birth prior to 1980, where my income data starts.

of sample mean wage income as seen from Tables 34 and 35. However, the association remains insignificant across specifications indicating that employment outcomes of women with no desire for a family are not significantly different from those of women desiring a small family. Overall, I find that individuals desiring a large family experience large

Table 35: Cumulative Wage Earnings 1980-2017 in mill. DKK, 2017 price-level)

	(1) Sample 1		(2) Sample 2		(3) Sample 3	
Variable	Coef.	SE	Coef.	SE	Coef.	SE
Family type						
(Ref. Small)						
No desire	-0.1950	(0.5323)	-0.1941	(0.5326)		
Large family	-0.6081**	(0.2965)	-0.6280**	(0.2981)	-0.6765**	(0.2998)
Don't know	0.4300	(0.9056)	0.4332	(0.9075)		
Mom: No Desire	-0.9392	(0.6908)				
Mom: Desire	-0.3733	(0.3123)	-0.4022	(0.3160)	-0.3859	(0.3177)
Earning pot.						
Time prefs.	0.2519	(0.1884)	0.2229	(0.2007)	0.1882	(0.2056)
Std. cog. skills	0.3127**	(0.1544)	0.3776**	(0.1627)	0.4235**	(0.1654)
Own. SE exp.						
(Ref. <LSE/Miss.)						
Exp. LSE	0.8752***	(0.2954)	0.8028***	(0.3029)	0.7472**	(0.3098)
Exp. USE	0.7529*	(0.4521)	0.5681	(0.4668)	0.8021*	(0.4802)
Family formation						
Consid./married	-0.1668	(0.2663)	-0.2289	(0.2799)	-0.2280	(0.2838)
Ever married	1.6589***	(0.5911)	1.6252***	(0.5914)	1.6719***	(0.6232)
No. of childr.	-0.3068**	(0.1356)	-0.2604*	(0.1443)	-0.2056	(0.1463)
Completed educ.						
ref. 7-8th/miss.						
LSE	6.8588***	(0.7481)	6.9268***	(0.7444)	6.7512***	(0.7691)
USE/VOC	7.7848***	(0.7494)	7.7470***	(0.7474)	7.6466***	(0.7740)
Short cycle	8.2869***	(0.8803)	8.3207***	(0.8780)	8.3562***	(0.9176)
BA	9.4841***	(0.7270)	9.5512***	(0.7210)	9.5738***	(0.7464)
MA or above	11.0422***	(1.1410)	11.1119***	(1.1347)	11.2303***	(1.2043)
Constant	-2.2461*	(1.1838)	-1.6170	(1.2268)	-1.2589	(1.2488)
Controls	X		X		X	
Fix	X		X		X	
No. of obs.	1207		1117		1034	
R^2	0.21		0.21		0.22	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Relation status in 1976: Married or has partner and considers marriage. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

and significant income and wage reductions over working career in the range of 7-8% of mean sample income compared to individuals desiring a small family. For cumulative

wage earnings, income reductions following from desired family type go hand in hand with additional income reductions following from realized childbirth. The unforeseen negative consequences on wage earnings of realized childbirth are stronger among individuals with no desire for a family, i.e. for individuals to whom family formation may be unexpected. The estimated cumulative income reductions from realized childbirth are similar in size to the annual causal effects found by, e.g., Lundborg et al. (2017) for the 2-5 years horizon, however their sample consists entirely of women with a positive desire for children, i.e. women that have self-selected into IVF-treatment. Combining the reduction due to desired family type of 7% and income reductions from realized fertility of 3.4% per child, I find a cumulative reduction for the first child of 10% for women desiring a large family, while it is only 3.4% for women desiring a small family. The cumulative reduction for women desiring a large family is similar as cumulative effects over the first 10 years after childbirth found in Kleven et al. (2018). My results, however, indicate that the income reduction from the second child would only be 3% of cumulative income, which is below effects found in Kleven et al. (2018) for the 10 year horizon. For individuals with no desire for a family, I find income reductions of 11% on cumulative gross income over working career compared to women desiring a small family. The association remains sizeable and significant after the inclusion of labor market factors suggesting that income reductions associated with having no desire for a family are not the result of significantly different labor market outcomes relative to the reference group desiring a small family. Hence, other factors remaining unobserved must be responsible. I speculate that the subgroup of women with no desire for a family may differ with regard to their wealth and savings behavior across working career. This is plausible as desiring no family may decrease incentives to accumulate buffer-savings in early adulthood. However, I cannot rule out that the income reductions are due to other unobserved factors.

The results overall confirm my hypothesis that desiring a large family is associated with lower cumulative wage earnings over working career relative to desiring a small family. However, women with no desire for a family experience even greater income reductions, which is surprising to me and unaccounted for in existing theoretical models of fertility choice. Below, I investigate potential labor market channels acting as transmitters for the

association between desired family type (and realized fertility) and cumulative income, i.e. the negative association between desiring a large family (or no family) and cumulative income.

5.5   Labor market channels

In this section, I consider potential labor market mechanisms acting as transmitters for the association between desired family type and cumulative wage earnings over career as described in Section 5.4. I also consider labor market mechanisms responsible for the transmission of a negative association between realized fertility and cumulative wage income over career.

Considering first the subgroup of women desiring a large family, I find small, but statistically significant, associations between desired family type and outcomes related to the sequencing of graduation, family formation and labor market entry, relative to the reference group desiring a small family. As seen from results in columns (1)-(3) of Table 36, desiring a large family is associated with an increased probability of graduating after having had a first birth, postponing labor market entry to after first birth, and postponing labor market entry to after last birth. The increases in probabilities of 4-5 percentage points are statistically significant at the 5%-level. Similarly, I find evidence of a higher average age at employment entry for individuals desiring a large family relative to individuals desiring a small family. The expected delay of 0.8 years is equivalent to a delay of 10 months, and it is significant at the 1%-level.[20]. Since earnings potential and education are included as controls, these results on sequencing and timing are not driven by a higher age at graduation. However, from descriptives, I found that a majority of other respondents postponed their first birth to after employment entry. This implies that results in Table 36 are driven by a minority of individuals. Hence, the results are only suggestive of a different sequencing among individuals desiring a large family.

For labor supply, I find that desiring a large family is associated with significant reductions in extensive and intensive margin labor supply over career as seen from Table 37. Regarding intensive margin labor supply, I find sizeable and statistically significant

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[20]It should be noted, that delay of entry had a positive coefficient in the income estimations reflecting that longer educations are correlated with higher salaries.

Table 36: Labor market outcomes, Large family desired

Variable	(1) Final grad.>Blast		(2) Entry>B1		(3) Entry>Blast		(4) Age at empl. entry	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Family type								
(Ref. Small)								
No desire	0.0570	(0.0455)	-0.0049	(0.0176)	-0.0222	(0.0177)	0.5381	(0.3864)
Large family	0.0519**	(0.0261)	0.0457**	(0.0209)	0.0400**	(0.0177)	0.7921***	(0.2478)
Don't know	0.1180	(0.0764)	0.2096***	(0.0682)	0.0039	(0.0432)	1.0288	(0.7111)
Mom: No Desire	0.2907***	(0.0650)	0.8025***	(0.0429)	0.4957***	(0.0559)	2.0571***	(0.4399)
Mom: Desire	0.2274***	(0.0298)	0.6315***	(0.0326)	0.2228***	(0.0284)	1.2006***	(0.3448)
Family formation								
No. of childr.	0.0375***	(0.0117)	0.0428***	(0.0083)	-0.0700***	(0.0079)	0.0150	(0.1180)
Consid./married*	0.0502**	(0.0248)	0.0180	(0.0203)	0.0301*	(0.0171)	0.3803	(0.2762)
Ever married	0.0774*	(0.0410)	-0.0227	(0.0257)	0.0682***	(0.0242)	-1.6344**	(0.6573)
Completed educ.								
(Ref. 7-8th/Miss.)								
LSE	-1.1932***	(0.0687)	-0.0817	(0.0548)	-0.0293	(0.1016)	-63.2743***	(8.7933)
USE/VOC	-0.8261***	(0.0702)	-0.0820	(0.0533)	-0.0497	(0.1008)	-63.2828***	(8.8064)
Short post-se	-0.8492***	(0.0878)	-0.1578***	(0.0584)	-0.0867	(0.1003)	-63.5321***	(8.6595)
BA	-0.6331***	(0.0706)	-0.0883	(0.0539)	-0.0429	(0.1006)	-62.4082***	(8.7287)
MA or above	-0.5743***	(0.0878)	-0.0463	(0.0637)	-0.0286	(0.1036)	-60.3511***	(8.6816)
Constant	0.8731***	(0.1203)	0.0556	(0.1087)	0.1260	(0.1226)	86.3715***	(8.5687)
Controls, SES	X		X		X		X	
Controls, earn.pot.	X		X		X		X	
Fix	X		X		X		X	
No. of obs.	1177		1177		1177		1177	
R^2	0.34		0.59		0.27		0.76	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Relationship, 1976: Married or has partner and considering marriage. *Controls, SES* are mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects. *Controls, earnings potential* are inductive, spatial and verbal ability, standardized rosenberg's score, educational expectations and time preferences.

negative associations between desiring a large family and both cumulative employment degree over career (-1.8 equivalent to 6.4% of sample mean) and cumulative years with full time employment (-2.1 years equivalent to 9.6% of sample mean). Likewise for extensive margin effects, I find a negative association between desiring a large family and cumulative years with (some) employment during 1980 to 2017 (-1.7 years equivalent to 4.6% of sample mean), and a large positive association between desiring a large family and cumulative unemployment degree over career (+29.6 equivalent to 1/3 of sample mean).

The identified negative labor market effects from desiring a large family go hand in hand with negative labor market consequences arising from realized childbirth. Me-

Table 37: Labor market outcomes, Large family desired

Variable	(1) Cum. empl. degr.		(2) Employment, years		(3) Full time, years		(4) Cum. unempl. degr.	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Family type								
(Ref. Small)								
No desire	-0.7446	(1.2522)	-0.3542	(1.2131)	-1.2527	(1.4532)	1.5086	(20.4200)
Large family	-1.8428**	(0.7375)	-1.6847**	(0.7565)	-2.0961***	(0.8083)	29.5908*	(15.1758)
Don't know	-1.2335	(2.0767)	-0.9682	(2.0506)	-1.6800	(2.3781)	22.6548	(31.6579)
Mom: No Desire	-3.9914**	(1.6989)	-4.0905**	(1.7329)	-3.7804**	(1.9073)	87.4397**	(36.2702)
Mom: Desire	-1.6113*	(0.8390)	-1.4659*	(0.8703)	-1.0166	(0.9172)	45.6633**	(19.1158)
Family formation								
No. of childr.	-0.8695***	(0.3261)	-0.7705**	(0.3379)	-1.3272***	(0.3471)	3.9139	(7.0279)
Consid./married*	-0.6890	(0.6659)	-0.6678	(0.6809)	-1.0040	(0.7412)	-10.7155	(13.7981)
Ever married	2.3339	(1.5201)	3.4078**	(1.4634)	0.9252	(1.6816)	14.5012	(22.2493)
Completed educ.								
(Ref. 7-8th/Miss.)								
LSE	9.5387***	(1.4889)	22.6554***	(2.2302)	8.1131***	(2.1859)	-44.2496	(114.3451)
USE/VOC	11.9013***	(1.4043)	24.7150***	(2.1993)	10.5534***	(2.1322)	-86.6201	(114.6695)
Short post-se	13.7636***	(1.8991)	26.5004***	(2.4721)	12.9601***	(2.5924)	-95.6435	(116.5968)
BA	13.7449***	(1.4487)	26.8485***	(2.1373)	13.1035***	(2.1738)	-85.5812	(114.3973)
MA or above	8.2080***	(2.0618)	21.0604***	(2.5865)	8.6183***	(2.6720)	-41.7818	(117.6144)
Constant	8.5872**	(3.4703)	-3.8021	(3.6842)	7.1585*	(3.9996)	99.8780	(122.8021)
Controls, SES	X		X		X		X	
Controls, earn.pot.	X		X		X		X	
Fix	X		X		X		X	
No. of obs.	1168		1177		1168		1168	
R^2	0.13		0.16		0.15		0.08	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Relationship, 1976: Married or has partner and considering marriage. *Controls, SES* are mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects. *Controls, earnings potential* are inductive, spatial and verbal ability, standardized rosenberg score, educational expectations and time preferences.

chanically realized fertility has a positive association with the probability of initiating childbearing prior to graduation and labor market entry, and a negative association with completing childbearing prior to labor market entry. This is seen from results in columns (1) to (3) of Table 36. As seen from results in column (4), realized fertility is uncorrelated with age at employment entry, when controlling for desired family type and marriage overall. This indicates that desired family type and marriage explain the entire effect of family formation on timing of entry. However, realized childbirth is associated with sizeable and statistically significant reductions in extensive and intensive margin labor supply as seen from results in columns (1) to (3) of Table 37. The associations between

realized childbirth and labor supply factors are qualitatively similar to the ones found for desiring a large family, but smaller (half-sized). The results indicate that labor market consequences of childbirth are not fully foreseen. As mentioned earlier, particularly women with no desire for a family may be more likely to experience unforeseen consequences of childbirth. There is no significant association between cumulative unemployment degree and realized fertility. This indicates that absences from the labor market are accounted for by desired family type.

Additionally, realized childbirths influence cumulative earnings through labor market channels that are not significantly associated with desired family type, indicating that associations are similar across individuals experiencing a childbirth, i.e. independently of desired family type. From Table 38 we see that cumulative years in part-time employment increase by 0.9 years for each realized childbirth, and that the probability of being employed as assisting spouse increases by 19 percentage points for each childbirth. These results corroborate empirical findings of a negative causal effect of childbirth on the labor supply of women (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017; Angelov et al., 2016; Kleven et al., 2018).

Considering now potential differences in labor market outcomes for women with no desire for a family relative to women desiring a family, from results in column (4) of Table 39, we see that having no desire for a family is associated with earlier entry into the labor market by almost 1 year. The anticipation is statistically significant at the 5%-level. Having no desire is also associated with a higher probability of postponed childbearing, cf. column (1) of Table 39. Moreover, from results in columns 2 and 3 of Table 39, we see that having no desire for a family is associated with a less stable working life as reflected in having held 0.8 more jobs over career, cf. column 2, and lower job stability as reflected in having held each job for significantly shorter periods of time, cf. column (3). Though wage earnings do no differ significantly for women with no desire and women desiring a small family, these results indicate that having no desire for a family is associated with earlier entry into the labor market and having a less stable working life. However, women with no desire do not have lower labor supply nor more time out of the labor market than

Table 38: Labor market outcomes, Child penalties

Variable	(1) Part time, years		(2) Prob. Ass. spouse	
	Coef.	SE	Coef.	SE
Family type				
(Ref. Large family)				
No desire	0.5416	(0.8683)	-0.2315	(0.1883)
Small family	-0.5305	(0.5028)	-0.0347	(0.2067)
Mom: Desire	-1.1895**	(0.5886)	-0.4387*	(0.2506)
Mom: No Desire	-0.4726	(1.2229)	-0.2188	(0.2985)
Family formation				
No. of childr.	0.9139***	(0.2358)	0.1923***	(0.0735)
Consid./married*	0.6329	(0.4822)	0.0639	(0.1975)
Ever married	2.7910***	(0.7563)	0.1907*	(0.1114)
Completed educ.				
(Ref. 7th/8th/Miss.)				
LSE	2.8428	(3.2280)	0.9059***	(0.2992)
USE/VOC	2.6937	(3.2169)	0.2572	(0.2077)
Short post-SE	1.6138	(3.3479)	-0.0197	(0.2192)
BA	1.2840	(3.2247)	-0.1116	(0.1985)
MA or above	-0.8276	(3.2720)	-0.0410	(0.2281)
Constant	3.4468	(3.9477)	-1.0307*	(0.5453)
Controls, SES	X		X	
Controls, earn.pot.	X		X	
Fix	X		X	
No. of obs.	1168		1177	
R^2	0.11		0.05	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Relationship, 1976: Married or has partner and considering marriage. *Controls, SES* are mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects. *Controls, earnings potential* are inductive, spatial and verbal ability, standardized rosenberg score, educational expectations and time preferences.

women desiring a small family. Hence, there is not any evidence gross income reductions are related to intensive or extensive margin labor supply effects.

Summing up, I found evidence of reduced labor supply on the intensive and extensive margin for individuals desiring a large family, resulting in a negative association between desiring a large family and cumulative wage earnings over career. Additionally, I found evidence of labor market mechanisms that lead to a negative association between realized fertility and wage income over career both in combination with and independently of desired family type. This indicates that the total negative effect of childbirth on wage

Table 39: Labor market outcomes, No family desired

Variable	(1) Entry>Blast Coef.	SE	(2) No. of jobs Coef.	SE	(3) Avg. job dur., years Coef.	SE	(4) Entry year Coef.	SE
Taste for fertility								
(Ref. Desires family)								
No desire	-0.0331*	(0.0171)	0.8047*	(0.4659)	-1.1115***	(0.4084)	-0.8776**	(0.3967)
Don't know	-0.0070	(0.0431)	1.7119	(1.3854)	-0.2250	(0.7221)	1.0232	(0.7423)
Mom: Desire	0.2229***	(0.0285)	0.5980*	(0.3344)	-0.7217**	(0.3006)	-0.0439	(0.3405)
Mom: No Desire	0.4915***	(0.0559)	0.9028	(0.7597)	-0.5218	(0.5283)	2.1740***	(0.7952)
Family formation								
No. of childr.	-0.0662***	(0.0076)	0.0661	(0.1255)	-0.3336***	(0.1292)	0.0396	(0.1159)
Consid./married*	0.0312*	(0.0171)	-0.2384	(0.2872)	-0.0098	(0.2580)	-0.5614**	(0.2738)
Ever married	0.0656***	(0.0240)	0.5117	(0.5307)	0.6025	(0.5006)	0.4564	(0.4253)
Completed educ.								
(Ref. 7-8th/Miss.)								
LSE	-0.0271	(0.1020)	1.0115	(1.7113)	1.7421**	(0.8255)	-19.3779***	(0.9028)
USE	-0.0490	(0.1012)	1.0715	(1.7109)	2.2260***	(0.8329)	-19.4334***	(0.8520)
Short post-SE	-0.0827	(0.1007)	1.3880	(1.8150)	1.8388*	(1.0331)	-17.3524***	(1.0228)
BA	-0.0398	(0.1011)	1.0856	(1.7121)	2.3774***	(0.8286)	-17.5034***	(0.8657)
MA or above	-0.0281	(0.1043)	2.6320	(1.8291)	0.4900	(0.9179)	-14.0051***	(1.1373)
Constant	0.1328	(0.1239)	4.1314**	(2.0727)	3.5389***	(1.2918)	1990.9014***	(1.0956)
Earnings potential	X		X		X		X	
Controls	X		X		X		X	
Fixed effects	X		X		X		X	
No. of obs.	1177		1160		1160		990	
R^2	0.27		0.08		0.08		0.30	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Relationship, 1976: Married or has partner and considering marriage. *Controls, SES* are mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects. *Earnings potential* are controls for inductive, spatial and verbal ability, standardized rosenberg score, educational expectations and time preferences.

earnings is the sum of foreseen consequences explained by individual fertility preferences and unforeseen labor market consequences resulting from realized childbirth.

6 Discussion

In this paper, I conduct an empirical investigation of the direct link between desired family type of individuals and their later life family formation and economic outcomes. The topic is important, because preferences, skills and personality during adolescence have been found to have a strong correlation with later life economic outcomes and well-being of individuals (see e.g. Golsteyn et al., 2014; Epper et al., 2019; Heckman et al., 2006).

Moreover, a rich applied literature has focused mainly on the causal effects of childbirth on labor market outcomes, ignoring the importance of pre- and post birth selection effects driven by fertility preferences on economic outcomes of women. Hence, I contribute to a greater understanding of the complementarity of expected and unforeseen consequences of childbirth on economic outcomes of women.

I base my empirical investigation on a number of hypotheses inspired by a mature structural literature on joint fertility and labor supply choices of women, particularly the model presented in Adda et al. (2017) as well as on a rich applied literature on the causal effects of childbirth on labor market outcomes of women (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017; Angelov et al., 2016; Kleven et al., 2018; Rosenbaum, 2019). My empirical investigation relies on a comprehensive dataset combining an extensive survey panel, The Danish Longitudinal Survey of Youth, with administrative registers on education, family formation, and labor market outcomes, including on income over working career. The survey evidence includes elicited fertility preferences, paving the way for an empirical investigation the direct link between individual fertility desires and outcomes.

My empirical analysis reveals several results: First, I document that among women, who have not experienced motherhood at elicitation of desires, almost 90% desire children, while 10% have no desire for a family. More than half of individuals desire a family of two children, while 1/3 desire a large family of three or more children. Desires for one child are practically inexistent. The variance of fertility desires increases in cognitive ability and educational aspiration in adolescence, while desires are orthogonal to individual time preferences.

Second, my results confirm that desiring a family translates into a significantly earlier childbearing debut (1.6 to 2.3 years) relative to individuals with no desire for a family, while desiring a large family is associated with slightly larger anticipations of 1/4-1/2 years relative to individuals desiring a small family. Timing of fertility is also strongly associated with individual time preferences and educational aspirations in line with existing evidence, see e.g. (Epper et al., 2020; Geruso and Royer, 2018; Black et al., 2008; Miller, 2011; Herr, 2016). Furthermore, I find that stronger fertility desires are associated with higher levels

of realized fertility. This result is in line with earlier empirical findings, (see e.g. (Mueller et al., 2019; Ejrnæs and Jørgensen, 2018; Berrington, 2004; Doepke and Kindermann, 2019; Bhrolcháin et al., 2010; Günther and Harttgen, 2016). Desiring a small family is associated with an increase of .3 childbirths relative to no desire, while desiring a large family is associated with an additional increase of .5 childbirths. Realizing fertility is also positively and significantly associated with forming stable relationships in early and later adulthood.

Never the less, among the subsample of individuals desiring a family, half end up with fewer children than desired. This is both an extensive and intensive margin issue. 10-15% of individuals with desire for a family end up childless, while 1/5 desiring motherhood end up with no more than one child despite having desires for more. Prior findings of Heiland et al. (2005) indicate that the risk of experiencing fertility below desires is increasing in level of education. Similarly, I find that expecting to complete USE, which paves the way for higher education, is associated with a significant increase of 9 percentage points in the likelihood of experiencing realized fertility below desires. The association between fertility deficits and educational aspirations is an intensive margin result. Neither time preferences, cognitive ability nor educational aspirations influence the likelihood of remaining childless.[21] The main drivers of deficits appears to be delayed formation of stable relationships, instability of relationships overall, and level of desires, which is however a mechanical result. The finding that higher educational aspirations are linked with a higher likelihood of fertility deficits may be an even greater problem today, as women continue to pursue higher educations and are less likely to pursue education and family formation in parallel than women in my sample are.

Third, for educational achievement, my results confirm that completed level of education, both secondary and highest education, are uncorrelated with family formation desires, once the individual's earnings potential is taken into consideration. In line with prior evidence, I find that the main drivers of level of education are own and parental expectations, cognitive ability, time preferences and self-esteem during adolescence (see

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[21]The association between deficits and educational aspirations is robust to controlling for level of education, age at first stable relationship and age at first birth, indicating that delayed childbearing following from education is not driving this result.

e.g. Golsteyn et al., 2014; Epper et al., 2019; Heckman et al., 2006; Abbott et al., 2013). In line with reduced form results in Adda et al. (2017), I find indicative evidence that individuals desiring a large family are more likely to select into less abstract occupations as proxied by field of study than individuals desiring a small family, while individuals desiring a small family are more likely to select into abstract occupations.

Fourth, for labor market outcomes, I find that desiring a large family is associated with significant income losses of 2 mill. DKK in 2019-price levels over working career relative to individuals desiring a small family. The cumulative reduction in gross income is equivalent to 8% relative to the sample mean. The result is robust across samples. Once individual labor market outcomes are controlled for the association practically disappears. This indicates that income effects are transmitted via labor market factors, particularly labor supply. In fact, individuals desiring a large family also experience significant cumulative wage losses equivalent to a 7% reduction over working career relative to the sample mean. The wage losses following from desired family type go hand in hand with wage reductions due to realized fertility of 3.4% per childbirth. This indicates that economic consequences of childbirth remain partially unforeseen, i.e. are independent of desired family type. Investigating the mechanisms responsible for income and wage reductions, I find that individuals desiring a large family are more likely to delay labor market entry, and that they have a lower cumulative labor supply over working career, reflecting both intensive and extensive margin effects, e.g. fewer years in the labor market, fewer years with full time employment and higher cumulative unemployment. Realized fertility exacerbate the negative consequences on labor supply as realized childbirths are associated with a higher share of time spent in part time employment and as assisting spouse, and reductions in overall job stability measured as average job duration.

My findings confirm the importance of both selection effects and direct unforeseen consequences of realized childbirth on labor market outcomes of women. The latter are in line with identification of negative child penalties in the applied literature (see e.g. Bronars and Grogger, 1994; Rosenzweig and Wolpin, 1980; Angrist and Evans, 1998; Lundborg et al., 2017), while the first lends evidence to the importance of self-selection following

from utility trade-offs between family formation and career as a main driver of career costs related to childbearing, Adda et al. (2017).

Somewhat puzzling, I find that having no desire for a family is associated with significant reductions in cumulative gross income over working career equivalent to 11% of the sample mean. The association remains sizeable and significant at 5% after controlling for completed education and labor market outcomes. In fact, wage earnings of women desiring no family are not significantly different from wage earnings of individuals desiring a small family.

Though my results are based on a cohort, which is close to exiting the labor market today, some of my findings have a general relevance. First, my findings are relevant for the general validity of causal effects identified in the applied literature. Particularly, assumptions about the distribution of fertility desires among experimental populations have to be explicit. Second, my results confirm that the inclusion of heterogenous fertility preferences are relevant for the design and estimation of structural models on joint fertility and labor supply choices of women. To the extent that data on fertility desires are available, this data should be included in the estimation of models. With regard to the design of structural models, the desing should allow for economic consequneces of childbearing to vary with the wanted of children. In addition, including a no desire type for children may be warranted as these agents tend to postpone childbearing significantly compared to women with positive desires. However, characteristics of a no desire type needs more investigation.

Future avenues for research that I hope to pursue are the following: Accessing existing historical income data, I intend to split cumulative income reductions associated with fertility desires into average annual reductions occurring in anticipation of and after birth, respectively. Historical income data is a prerequisite for following income across fertile ages of women in my sample, including in their early childbearing years. In addition, I aim to test for heterogeneous consequences of fertility desires for women with fertility deficits, one-to-one transmission of desires, and excess fertility, respectively to understand if the income reductions I find reflect childbearing among women, who are successful in fulfilling

their desires. Finally, gaining access to collected abortion data will allow me to assess to what extent fertility deficits are the result of altered childbearing intentions.

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A   Appendix

A.1   Survey questions on parental status

Before, the elicitation of fertility desires in DLSY, respondents answered the following question on their parental status: *Do you have any children of your own?* To which they could answer: *Yes, Yes - but they do not all live with me* or *No*. This measure reveals that 1/3 of female respondents were already mothers at elicitation of desires. For early mothers, the elicited desire are then no longer in accordance with the idea of an unconstrained fertility target. Particularly, the answer *No desire* could have multiple interpretations, e.g. it could be taken to mean *No, I have already reached my target* or *No, I never wanted children in the first place*. Whether the elicitation of desires for early mothers desiring additional children can be taken as a proxy of an unconstrained fertility target is also subject to doubt. However, I will assume that respondents expressing a desire for additional children, answer *Yes [...I would like additional children]* to confirm that they have not reached their target yet. Such that their reported desires can be interpreted as their unconstrained fertility targets, conditional on current fertility. This assumption follows the evidence of Mueller et al. (2019). To control for parental status at elicitation, I include dummies for early mothers with additional desire and for early mothers with no additional desire in all empirical estimations. My approach has one caveat. The interpretation of parameters for early mothers with no desire is not straight forward.

A.2   Survey questions on early relationships

In 1973 at age 19, DLSY respondents were asked the following question *Do you have a spouse or a partner?* to which they could answer *Married/cohabiting, Partner* or *No partner*, and in 1976, when desires were elicited, they were again asked *Are you married or going steady with someone?* and respondents with a partner were further asked *Have you considered getting married or moving in together?*. From the two 1976 questions, I form the following relationship categories at elicitation of desires: *No partner, partner with no wish to move in, partner and not considering to move in, partner and considering to move in, partner and considering to get married, living with a partner, married*. The

idea is that the categories become more and more stable from no partner to married. Amongst these categories, I find that the categories most strongly correlated with family formation are *Partner and considering to get married* and *Married*. In some specifications I therefore include a joint dummy for these two outcomes to control for relationship status at elicitation.

A.3   Survey questions on earnings potential

Social mobility across generations is a main focus area of DLSY. Therefore the panel contains several questions related to the earnings potential of an invidual including on their time preferences, taste for education and their cognitive ability. Measures on respondent's own educational expectations are indicative of their educational aspirations, while ability scores are indicative of cognitive ability. Finally, Rosenberg's self-esteem score and the socio-economic characteristics of individuals' parents are indicative of respondents' non-cognitive ability (Abbott et al., 2013; Heckman et al., 2006).[22]

For educational aspirations, I rely on elicited expectations of own level of secondary education among respondents, as well as parental expectations on attainment of secondary schooling. In 1968 in 7th grade, the final year of obligatory schooling, respondents were asked : *With which grade do you expect to leave school?* to which they could answer *7th, 8th, 9th, 10th, Lower Secondary Education* or *Upper Secondary Education.* Parents of respondents were asked one year later in 1969: *After which grade you think your child will leave school?.* Answers to these questions are included in most estimations.[23]

Also, in 1968 respondents were asked to complete a row of cognitive ability tests designed to test three aspects of their cognitive ability. The first set of tests dealt with respondents' verbal ability and included several linguistic comprehension and power of

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[22]I report coefficients on factors of individual earnings potential directly, while socio-economic characteristics of parents, such as parental education, occupation, employment status and household income are included only as controls in the empirical analysis. The same is true for factors related to family size, parity of individuals and whether they had a stable childhood as proxied by growing up with both parents.

[23]Lower Secondary Education, *Realeksamen*, is equivalent to the completion of an academic track 8th-10th grade. The resulting LSE exam was a pre-requisite for direct admission into educations such as secretary, nurse and primary school teacher. It was common, but not a requirement for entry into Upper Secondary Education such as *Gymnasium* or *Higher Preparatory Exam (HF).* A non-academic track 8th-10th grade was available for students preparing for vocational education.

abstraction tests. The second set of tests dealt with respondents' spatial ability and included tests of threedimensional and geometric comprehension. Finally, the third set of tests dealt with respondents' inductive ability and included a series of mathematical and logical tests. The three tests are described in Ørum (1971: pp. 25-28) and Hansen (1995: pp. 67-68 and 101-102). Respondents' total score in each of the three tests is available in the DLSY dataset. It turns out that mainly the test score on inductive ability, relying on mathematical and logical tests, is significantly related to earnings in later life. For cognitive ability, I therefore rely on the standardized measure of inductive ability.

In 1973 (at age 19), the respondents answered the following question regarding their time preferences:*If you were offered three jobs now and you could choose, which one would you take?* to which they could choose among the following answers: *A job with average pay right from the beginning, A job with low pay the first two years, but high pay later, A job with very low pay the first four years, but very high pay later* and *Don't know*. This implies 3 degrees of patience leaving out respondents anwering that they *Don't know*. In some descriptives I categorize both respondents answering (b) and (c) as patient in line with Epper et al. (2019), but in the empirical analysis I include the 3-step measure of patience as a continuous variable.

In comparison to other experimental measures of time preferences (see e.g. Frederick et al., 2002; Epper et al., 2011; Cohen et al., 2020), the survey measure on time preferences in DLSY has both advantages and disadvantages. The possibly most important advantages is that the survey question is short, simple, and less abstract than typical intertemporal choices employed in experiments. Specifically, subjects are asked about their choice in a real-life situation with substantial economic consequences. This contrasts with experimental measures that typically ask subjects to repeatedly choose between sooner smaller amounts and later larger amounts (usually materializing within some weeks or a few months). This context-dependence might also be viewed as a shortcoming of the measure, in that considerations other than pure time preferences might lead subjects to choose a particular wage profile.

Risk-averse individuals may, for instance, choose the average pay, fearing they would not reach the high pay (although the question does not explicitly associate risk with future pay raises). The patience indicator from DLSY is both internally and externally valid. Epper et al. (2020) document that the DLSY measure is highly predictive of time preferences elicited in an experiment with real monetary incentives among a broad and heterogeneous population born between 1967 and 1986. Furthermore, examining the validity of the measure in an experiment with a large representative sample of the Danish population, Epper et al. (2020) find that the DSLY survey measure is a good predictor for experimentally elicited time preferences. Finally, the individuals, which are categorized as being patient face significantly better socioeconomic outcomes in adulthood, even when controlling for a wide range of childhood family characteristics.

A.4   Desires and relationship status at elicitation

In Tables A.1 and A.2 distributions of desired family type by relationship status 3 years prior to elicitation (1973) are shown for early mothers and other respondents, respectively.

Table A.1: Desires and relationship of mothers, 1973

Desired family type	No desire	Small family	Large family	Don't know
Relationship				
Married/cohabiting	39.71	36.76	22.06	1.47
Partner	17.39	48.91	31.52	2.17
No partner	16.39	47.54	34.43	1.64
Total	25.20	44.36	28.61	1.84

Note: Relationship status was elicited 3 years before desires at age 19. 35% were married, 48% had a partner, and 16% had no partner.

Table A.2: Desires and relationship of other respondents, 1973

Desired family type	No desire	Small family	Large family	Don't know
Relationship				
Married/cohabiting	15.38	52.31	32.31	0.00
Partner	9.73	60.47	28.33	1.48
No partner	12.60	53.73	31.62	2.06
Total	11.33	57.07	29.99	1.62

Note: Relationship status was elicited 3 years before desires at age 19. 7% were married, 51% had a partner, and 42% had no partner.

Among early mothers more than 1/3 were married or cohabiting in 1973 around age 19, 1/2 had found a steady partner, while only 15% had no partner. Among other respondents, only 7 % were married in 1973, 1/2 had a partner and 42 % had no partner. Hence, early mothers were significantly more likely than other respondents to have entered stable relationships by age 19. Whether marriages and cohabitations took place as a consequence of fertility or the other way around is unobserved. Tables A.1 and A.2 show that the tendency to have no (additional) desire for children is stronger among individuals that were already married by 1973, than among individuals with or without a steady partner. Among other respondents desiring children, the share desiring small and large families are similar for individuals, who were either married or without a partner in 1973. Thus, desires are not increasing in stability of relationships 3 years prior to elicitation.

A.5   Additional empirical results

A.5.1   Education: Completion of USE

As a supplement to results on completed level of secondary education presented in Section 5.3.1, I analyse the link between a dummy for USE completion and desired family type.[24]. The results found are similar to the results on completed level of secondary education. As seen from column (3) of Table , desiring children is associated with a reduction in the unconditional probability of completing USE by 10 percentage points, and the effect is statistically signficant at the 5%-level. Being an early mother is associated with significant reductions in the probability of completing USE with coefficients of -.2 for early mothers with additional desire and -.35 for early mothers with no additional desire. However, as seen from results in column (2) of Table , the coefficient on desiring a family is reduced to 2 percentage points and is no longer statistically significant once factors related to the individual's earnings potential are accounted for. Only the negative association between being an early mother with additional desire and completion of USE remains signficant across all specifications, though it is reduced to 7 percentage points. Partially this is due to the inclusion of early relationship outcomes (1973), which are tightly linked with educational outcomes of early mothers. The early relationship outcomes are correlated

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[24]I rely on the linear probability model for these estimations.

with significant reductions of 4-7 percentage points in the probability of USE completion. Again own educational expectations, parental educational expectations and cognitive as well as verbal ability are found to be the main drivers of completing USE, while the association with time preferences is limited. The probability of completing USE is negatively associated with the standardized score on Rosenberg's self-esteem test.

Table A.3: Dummy for Upper Secondary Education, Sample 1

	(1)		(2)		(3)	
Variable	Coef.	SE	Coef.	SE	Coef.	SE
Desired family type						
(Ref. small family)						
No desire	0.0951*	(0.0499)				
Large family	0.0371	(0.0266)				
Don't know	0.3315***	(0.0848)				
Mom: No Desire	-0.3160***	(0.0523)				
Mom: Desire	-0.1984***	(0.0238)				
Desired family type						
(Ref. No desire)						
Small family			-0.0865*	(0.0520)		
Large family			-0.0504	(0.0545)		
Mom: No Desire			-0.3087***	(0.0539)		
Mom: Desire			-0.1996***	(0.0238)		
Taste for fertility						
Desire					-0.1031**	(0.0501)
Mom: No Desire					-0.3441***	(0.0527)
Mom: Desire					-0.2006***	(0.0235)
Constant	0.2730***	(0.0178)	0.3608***	(0.0489)	0.3962***	(0.0476)
No. of obs.	1312		1312		1312	
R^2	0.07		0.07		0.06	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level.

A.5.2   Income: Labor market earnings

Relying on a broader measure of labor market earnings, which included both wage earnings and earnings from independent business and being employed as an assisting spouse, results in column 1 of Table A.5 show that desiring a large family is associated with a statistically significant earnings loss of 1.2 mill. DKK in 2019-price levels over working career compared to the reference group desiring a small family. This is equivalent to an earnings penalty of 6% compared to the sample mean. The reduction in cumulative earnings

Table A.4: Dummy for Upper Secondary Education, Sample 1

Variable	(1) Coef.	(1) SE	(2) Coef.	(2) SE	(3) Coef.	(3) SE
Taste for fertility						
Desire	-0.1031**	(0.0501)	-0.0242	(0.0476)	0.0282	(0.0506)
Mom: No Desire	-0.3441***	(0.0527)	-0.1206**	(0.0545)	0.0036	(0.0615)
Mom: Desire	-0.2006***	(0.0235)	-0.1066***	(0.0222)	-0.0696***	(0.0249)
Earnings potential						
Time prefs.			0.0189	(0.0157)	0.0169	(0.0165)
Rosen score**			-0.0229**	(0.0099)	-0.0224**	(0.0103)
Own exp.						
(Ref. 7th/8th/Miss.)						
Exp. 9th/10th			-0.0444	(0.0294)	-0.0078	(0.0307)
Exp. LSE			0.0974**	(0.0381)	0.0482	(0.0530)
Exp. USE			0.3916***	(0.0493)	0.1522**	(0.0670)
Ability**						
Cognitive			0.0663***	(0.0147)	0.0553***	(0.0147)
Spatial			-0.0132	(0.0128)	-0.0160	(0.0133)
Verbal			0.0393**	(0.0164)	0.0247	(0.0169)
Par. exp.						
(Ref. <LSE)						
Exp. LSE					0.0940**	(0.0419)
Exp. USE					0.3648***	(0.0580)
Early relationships						
(1973)						
Marr./cohab					-0.0674**	(0.0334)
Partner					-0.0472*	(0.0253)
Constant	0.3962***	(0.0476)	0.1527**	(0.0628)	0.0477	(0.0908)
Controls					X	
Fix					X	
No. of obs.	1312		1244		1076	
R^2	0.06		0.30		0.37	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Ability is measured by standardized test scores from 1968. **Standardized Rosenberg's self-esteem score from 1973. Controls are Father's education, mother's education, father's occupation, dummy for working mother, siblings, parity, dummy for growing up with both parents, and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Fixed effects for birth year.

is smaller than the loss found for gross income. This reflects that gross income includes a broader set of sources of income such as capital income and transfers. The reduction in earnings associated with having no desire for a family is slightly larger than that for individuals desiring a large family. However, the association is statistically insignificant across specifications reflecting a large variance in earnings of women with no desire for a family. Early mothers experience additional and sizeable reductions in earnings, how-

ever only for early mothers is the effect robust to controlling for family formation. Once family formation and completed level of education are controlled for the reduction in earnings associated with desiring a large family increases to 7.4%, while reductions for early mothers are reduced significantly. The negative association is robust across samples as seen from Table A.6, though the estimated effect is slightly larger, when considering only women with a desire for a family (sample 3). The sign on the association between realized childbirth and earnings is negative, but the estimated earnings reduction of -0.2 mill. DKK per child seen in column 2 of Table A.5 is insignificant. Once, labor market outcomes such as labor supply are controlled for, the negative association between desiring a large family and earnings disappears. Age of employment entry and labor supply factors over career, as well as occupation over career, are found to have large and significant effects on earnings. Similar to the conclusion for gross income, the negative effect of desired family type on earnings appears driven by sequencing, timing and labor supply over career. Early mothers experience additional and sizeable reductions in earnings, however only for early mothers with additional desire is the effect robust to controlling for family formation. Also, for individuals having no desire for a family a negative association with earnings is found compared to the references group desiring a small family. The reduction in earnings associated with having no desire for a family is slightly larger than that of individuals desiring a large family. However, the association is insignificant across specifications reflecting large standard errors.

A.5.3 Income: After tax income

Regarding after tax income, we see from column 1 of Table A.7 that desiring a large family is associated with average income reductions of 0.45 mill. DKK in 2019-price levels compared to the reference group desiring a small family. This is equivalent to an income reduction of 6% of the sample mean net income over working career. The result is statistically significant at a 10%-level. The fact that the relative effect on disposable income from desiring a large family is lower than the effect found for gross income is likely to reflect tax and transfer measures installed to increase overall equality. Having no desire for a family is associated with twice as large reductions in net income, however the association

is insignificant across specifications. Controlling for family formation and completed level of education reduces the association between desiring a large family and net income to 4%, but the association remain significant. The result is robust across samples as seen from Table A.8. No child penalties on realized childbirth are found for net income. Presumably this is due to favorable tax and transfer measures, which are activated at the onset of parenthood. Once labor market factors are controlled for the association between desired family type and net income is reduced and no longer significant.

Table A.5: Cumulative earnings, 1980-2017 in mill. DKK 2019-levels, Sample 1

Variable	(1) Coef.	SE	(2) Coef.	SE	(3) Coef.	SE
Family type						
(Ref. Small family)						
No desire	-1.4575	(1.2532)	-1.2657	(1.2577)	-0.4320	(0.7290)
Large family	-1.2453*	(0.7264)	-1.5313**	(0.7299)	-0.3447	(0.4543)
Don't know	1.5798	(2.1255)	0.5584	(2.0168)	0.0770	(1.5729)
Mom: No Desire	-2.7533*	(1.4785)	-1.7164	(1.5450)	0.2465	(0.9036)
Mom: Desire	-2.4696***	(0.6835)	-1.1686*	(0.6915)	-0.3763	(0.4684)
Earning pot.						
Time prefs.	1.1009**	(0.4565)	0.7593*	(0.4362)	0.0439	(0.2774)
Std. cog. skills	1.7132***	(0.3806)	1.0742***	(0.3809)	0.7204***	(0.2222)
Own educ. exp.						
(Ref. <LSE/Miss.)						
Exp. LSE	2.9151***	(0.6478)	1.5015**	(0.6687)	0.3987	(0.3582)
Exp. USE	5.6921***	(1.0796)	2.4076**	(1.1016)	2.1481***	(0.7526)
Family formation						
Consid./married			-0.9248	(0.6228)	-0.0286	(0.3322)
Ever married			4.1073***	(1.4224)	1.7982**	(0.8537)
No. of childr.			-0.1980	(0.3054)	-0.0341	(0.1913)
Completed educ.						
ref. 7-8th/miss.						
LSE			7.9012***	(2.6075)	-2.9187	(3.0936)
USE/VOC			9.9063***	(2.6025)	-2.7101	(3.0763)
Short cycle			11.0045***	(2.9223)	-3.0170	(3.2059)
BA			13.6760***	(2.6100)	-1.7773	(3.0635)
MA or above			23.7989***	(4.0252)	9.7030**	(3.9228)
Labor market, 1976-2017						
Age at entry					0.3811***	(0.1179)
Avg. empl. degr.					19.2643***	(6.0498)
No. of jobs					0.1188**	(0.0586)
Share w/ part time					-8.5771***	(1.9105)
Share w/ no empl.					1.7928	(7.2667)
Occupation, 1976-2017, years						
(Ref. Missing)						
Independent					1.0778***	(0.1772)
Ass. spouse					0.7841***	(0.1199)
Employed with company					0.5534***	(0.1323)
Employed					0.8265***	(0.0935)
Employed with UI					0.3603**	(0.1834)
Retired with company					0.4094***	(0.1555)
Retired					0.3670***	(0.1205)
Other					0.4375***	(0.1330)
Early retirement					-0.3551**	(0.1512)
Unemployed					0.3411**	(0.1565)
Unemployed with UI					-0.0140	(0.1804)
Unemployed with SA					0.3762**	(0.1549)
DLSY, 2001						
Ever manager					1.3238***	(0.3439)
Constant	17.5761***	(1.0421)	1.0491	(3.8406)	-28.9645***	(9.9904)
Controls			X		X	
Fix			X		X	
No. of obs.	1257	117	1197		1038	
R^2	0.12		0.24		0.72	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Relation status in 1976: Married or has partner and considers marriage. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

Table A.6: Cumulative Earnings 1980-2017 in mill. DKK, 2019 price-level)

Variable	(1) Sample 1		(2) Sample 2		(3) Sample 3	
	Coef.	SE	Coef.	SE	Coef.	SE
Family type						
(Ref. Small)						
No desire	-1.2657	(1.2577)	-1.2971	(1.2577)		
Large family	-1.5313**	(0.7299)	-1.5681**	(0.7368)	-1.6299**	(0.7340)
Don't know	0.5584	(2.0168)	0.5475	(2.0262)		
Mom: No Desire	-1.7164	(1.5450)				
Mom: Desire	-1.1686*	(0.6915)	-1.1744*	(0.6987)	-1.0334	(0.6990)
Earning pot.						
Time prefs.	0.7593*	(0.4362)	0.6961	(0.4670)	0.5694	(0.4796)
Std. cog. skills	1.0742***	(0.3809)	1.2494***	(0.4087)	1.3788***	(0.4131)
Own. SE exp.						
(Ref. <LSE/Miss.)						
Exp. LSE	1.5015**	(0.6687)	1.3873**	(0.6893)	1.1656	(0.7094)
Exp. USE	2.4076**	(1.1016)	2.0596*	(1.1436)	2.7629**	(1.1912)
Family formation						
Consid./married	-0.9248	(0.6228)	-1.1238*	(0.6583)	-1.1524*	(0.6712)
Ever married	4.1073***	(1.4224)	4.0644***	(1.4251)	4.1896***	(1.5219)
No. of childr.	-0.1980	(0.3054)	-0.1135	(0.3243)	-0.0713	(0.3346)
Completed educ.						
ref. 7-8th/miss.						
LSE	7.9012***	(2.6075)	8.5244***	(2.8846)	8.7083***	(2.8117)
USE/VOC	9.9063***	(2.6025)	10.3261***	(2.8845)	10.6836***	(2.8095)
Short cycle	11.0045***	(2.9223)	11.5844***	(3.1961)	12.1658***	(3.1429)
BA	13.6760***	(2.6100)	14.2717***	(2.8767)	14.8339***	(2.8123)
MA	23.7989***	(4.0252)	24.3815***	(4.2442)	25.6232***	(4.4202)
Constant	1.0491	(3.8406)	1.5342	(4.0903)	1.7413	(4.0482)
Controls	X		X		X	
Fix	X		X		X	
No. of obs.	1197		1107		1025	
R^2	0.24		0.24		0.25	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Relation status in 1976: Married or has partner and considers marriage. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

Table A.7: Cumulative after tax income 1980-2017 in mill. DKK 2019-levels, Sample 1

	(1)		(2)		(3)	
Variable	Coef.	SE	Coef.	SE	Coef.	SE
Family type						
(Ref. Small family)						
No desire	-0.8059**	(0.3613)	-0.4170	(0.2772)	-0.1844	(0.1974)
Large family	-0.4483*	(0.2412)	-0.2999*	(0.1735)	-0.0883	(0.1222)
Don't know	0.3058	(0.5077)	0.3070	(0.4072)	0.0780	(0.4160)
Mom: No Desire	-0.3029	(0.3445)	-0.3070	(0.3273)	-0.0519	(0.2398)
Mom: Desire	-0.6863***	(0.1949)	-0.3253**	(0.1517)	-0.2304*	(0.1224)
Earning pot.						
Time prefs.	0.3389***	(0.1042)	0.2549***	(0.0952)	0.1222*	(0.0717)
Std. cog. skills	0.3065***	(0.0977)	0.2369***	(0.0855)	0.1642***	(0.0622)
Own educ. exp.						
(Ref. <LSE/Miss.)						
Exp. LSE	0.9017***	(0.3161)	0.2986**	(0.1512)	0.2022*	(0.1038)
Exp. USE	1.1987***	(0.2614)	0.5957**	(0.2505)	0.6244***	(0.1951)
Family formation						
Consid./married			-0.4073***	(0.1399)	-0.1457	(0.0905)
Ever married			0.8326**	(0.3291)	0.4501*	(0.2517)
No. of childr.			0.0375	(0.0708)	0.0568	(0.0563)
Completed educ.						
ref. 7-8th/miss.						
LSE			5.5902***	(0.6859)	0.0141	(0.4533)
USE/VOC			5.9852***	(0.6858)	0.1034	(0.4437)
Short cycle			6.1299***	(0.7318)	0.0512	(0.5157)
BA			6.6290***	(0.6724)	0.2767	(0.4397)
MA or above			8.7625***	(0.9072)	2.8716***	(0.7461)
Labor market, 1976-2017						
Age at entry					0.0528*	(0.0309)
Avg. empl. degr.					4.8542***	(1.6966)
No. of jobs					0.0267	(0.0171)
Share w/ part time					-1.8274***	(0.5451)
Share w/ no empl.					1.6737	(1.9908)
Occupation, 1976-2017, years						
(Ref. Missing)						
Independent					0.3263***	(0.0476)
Ass. spouse					0.2484***	(0.0329)
Employed with company					0.1654***	(0.0352)
Employed					0.2319***	(0.0242)
Employed with UI					0.1141**	(0.0481)
Retired with company					0.1784***	(0.0527)
Retired					0.2258***	(0.0304)
Other					0.0912***	(0.0338)
Early retirement					0.0971**	(0.0408)
Unemployed					0.1774***	(0.0390)
Unemployed with UI					0.1555***	(0.0496)
Unemployed with SA					0.2208***	(0.0393)
DLSY, 2001						
Ever manager					0.1551*	(0.0935)
Constant	6.7097***	(0.2566)	-0.6697	(0.7938)	-7.0795***	(2.6112)
Controls			X		X	
Fix			X		X	
No. of obs.	1275	119	1207		1038	
R^2	0.05		0.28		0.57	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level. *Relation status in 1976: Married or has partner and considers marriage. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

Table A.8: Cumulative after tax income 1980-2017 in mill. DKK, 2019 price-level)

	(1) Sample 1		(2) Sample 2		(3) Sample 3	
Variable	Coef.	SE	Coef.	SE	Coef.	SE
Family type						
(Ref. Small)						
No desire	-0.4170	(0.2772)	-0.4261	(0.2784)		
Large family	-0.2999*	(0.1735)	-0.3057*	(0.1758)	-0.3200*	(0.1751)
Don't know	0.3070	(0.4072)	0.3033	(0.4088)		
Mom: No Desire	-0.3070	(0.3273)				
Mom: Desire	-0.3253**	(0.1517)	-0.3124**	(0.1539)	-0.2733*	(0.1542)
Earning pot.						
Time prefs.	0.2549***	(0.0952)	0.2443**	(0.1029)	0.2219**	(0.1072)
Std. cog. skills	0.2369***	(0.0855)	0.2735***	(0.0927)	0.2952***	(0.0941)
Own. SE exp.						
(Ref. <LSE/Miss.)						
Exp. LSE	0.2986**	(0.1512)	0.2591	(0.1591)	0.1959	(0.1659)
Exp. USE	0.5957**	(0.2505)	0.5468**	(0.2615)	0.6839**	(0.2762)
Family formation						
Consid./married	-0.4073***	(0.1399)	-0.4796***	(0.1487)	-0.4914***	(0.1529)
Ever married	0.8326**	(0.3291)	0.8235**	(0.3290)	0.8108**	(0.3459)
No. of childr.	0.0375	(0.0708)	0.0547	(0.0759)	0.0683	(0.0783)
Completed educ.						
ref. 7-8th/miss.						
LSE	5.5902***	(0.6859)	5.6293***	(0.6725)	5.6209***	(0.7295)
USE/VOC	5.9852***	(0.6858)	5.9758***	(0.6739)	6.0274***	(0.7318)
Short cycle	6.1299***	(0.7318)	6.1699***	(0.7228)	6.3195***	(0.7710)
BA	6.6290***	(0.6724)	6.6476***	(0.6588)	6.7456***	(0.7133)
MA or above	8.7625***	(0.9072)	8.7780***	(0.8977)	9.0891***	(1.0055)
Constant	-0.6697	(0.7938)	-0.6463	(0.7931)	-0.5431	(0.8629)
Controls	X		X		X	
Fix	X		X		X	
No. of obs.	1207		1117		1034	
R^2	0.28		0.27		0.29	

Note: Standard errors in parentheses ***/**/* indicate statistical significance at the 1%/5%/10%-level.*Relation status in 1976: Married or has partner and considers marriage. Controls are father's education, mother's education, parity, siblings, dummy for working mother, dummy for grew up with both parents, father's occupation and annual taxable income of family breadwinner in year 1967 in 1000 DKK. Birth year fixed effects.

Chapter 2

Complexity and the Effectiveness of Public Policy

Complexity and the Effectiveness of Public Policy

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Abstract

We study how job seekers' understanding of complex UI benefit rules affects their job search and labor market outcomes based on a randomized controlled trial conducted among the universe of Danish UI benefit recipients. Our intervention exogenously promotes the usage of an online information tool that provides individuals with personalized information on how their accumulated working time can be used to prolong their potential benefit period. We match the data from our experiment with an online survey and comprehensive administrative records. The intervention improves job seekers' understanding of prevailing labor market rules significantly, while consequences for realized labor market outcomes crucially depend on timing of the treatment. For long-term benefit recipients, who are close to benefit expiration, the treatment increases their probability of working in temporary and part-time jobs, but reduces their overall working hours and earnings. Individuals treated at the beginning of their benefit spell on the contrary tend to accept jobs of higher quality.

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1 Introduction

Complexity is a common feature of tax and transfer systems across the world (see, e.g., Chetty and Saez, 2013). For instance, unemployment insurance (henceforth: UI) systems in modern labor markets are riddled with a multitude of rules and regulations governing job seekers' economic situation and their incentives to search for employment. These include detailed regulations specifying individuals' benefit level, the period of benefit payments (Card and Levine, 2000; Schmieder et al., 2012, 2016), job search and work requirements for avoiding benefit sanctions (Lalive et al., 2005; Arni et al., 2013), and specific regulations to work in part-time or short-term jobs (Caliendo et al., 2016). Clearly, a majority of these rules and regulations aim at minimizing moral hazard that typically arises in social insurance systems (Hopenhayn and Nicolini, 1997; Krueger and Meyer, 2002). However, the complexity associated with all the intertwined rules and regulations is likely to be an important factor in itself that may distort individuals' job search incentives as it hampers their understanding of prevailing rules and resulting consequences for their personal economic situation.

In this paper, we investigate how job seekers' knowledge regarding the complex unemployment benefit rules affects their job search and labor market outcomes. We present the results of a large-scale field experiment among Danish UI benefit recipients, in which we encouraged the use of an online information tool providing up-to-date, personalized information on individuals' UI benefit situation and corresponding rules. The digital tool is specifically designed to reduce complexity and increase people's understanding regarding the rules and regulations that govern their economic situation as unemployed. Specifically, the digital tool informs job seekers about two key elements of the UI system: the potential benefit duration (henceforth: PBD) and work requirements to avoid benefit sanctions. As a crucial feature unemployed workers in Denmark face the possibility to prolong the PBD and to avoid sanctions by accumulating working hours, e.g. in part-time or temporary jobs, after their initial registration with public employment services. Participants in our experiment are randomly assigned to three equally sized groups: individuals in our main treatment (henceforth: tool treatment) receive messages that direct their attention to the

online information tool and explain the general rules on possible extensions of the PBD and benefit sanctions. Individuals in the control group receive no additional information, while individuals in a third group receive a generic message (henceforth: message treatment) that contains general information about job search, but is unrelated to the UI benefit rules and the online information tool.[1]

To evaluate the causal effect of our intervention, i.e. the impact of knowledge on search behavior and labour market outcomes, we rely on a combination of comprehensive register data and an online survey that is administered to a subset of participants across all treatments. The latter allows us to directly examine the job seekers' understanding of the UI benefit rules and their personal situation, while the register data provide detailed information regarding subsequent working hours and earnings. As job search incentives in general (Van den Berg, 1990; Card et al., 2007a; Marinescu and Skandalis, 2019) and especially the relevance of the job seekers' knowledge about the PBD rules crucially depend on the time until benefit exhaustion, our empirical analysis focuses on heterogeneous treatment effects with respect to the elapsed benefit duration at the beginning of our intervention, which targets the stock of UI benefit recipients. Our analysis also provides insights with respect to the optimal timing of the information treatment.[2]

Our empirical analysis reveals several results. First, we document that job seekers have limited knowledge regarding the benefit rules and their own economic situation. On average, untreated individuals answer only half of the knowledge questions correctly and about 60% of job seekers report the number of weeks until benefit exhaustion incorrectly. We show that, the digital information tool improves the understanding of rules among treated job seekers by 8-10% relative to the control group. While the increase in knowledge is robust across subgroups at different points of their benefit period, the consequences for realized labor market outcomes crucially depend on the timing of the treatment. Long-term benefit recipients, who are close to benefit expiration, tend to make greater use of marginal work opportunities, i.e. take up temporary and part-time jobs, in response

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[1]The latter allows us to isolate any potential effect of sending out messages independently of the specific content.

[2]A related strand of the literature analyzes the optimal sequencing of labor market policies in a dynamic setting over the course of the unemployment spell taking into account human capital depreciation (see e.g. Pavoni and Violante, 2007; Wunsch, 2013; Spinnewijn, 2013).

to the treatment. The shift from regular to marginal jobs results in significantly fewer accumulated working hours and lower earnings over the first year after intervention. In contrast, treated job seekers who are at the beginning of their unemployment spell accept part-time jobs less often, earn higher hourly wages and obtain slightly improved long-run labor market outcomes. The differential effects of the treatment are likely to reflect the different incentives that job seekers face over the course of the benefit period. For job seekers at the beginning of the benefit period, the prospect of extending the PBD seems to reduce the pressure to accept low-quality jobs. However, those who are close to benefit expiration have larger incentives to work in temporary or part-time jobs in order to actually extend the PBD, which has negative consequences for their subsequent labor market performance.

Our findings have several important implications. First, we show that the usage of online tools provides a promising and potentially low-cost solution for improving individuals' understanding of complex policies and prevailing rules. Especially, providing updated personalized information about underlying incentives effectively generates behavioral changes among the target group (see also Fuentes et al., 2017). The consequences of greater knowledge about the economic incentives for subsequent labor market outcomes, however, depend on the actual design of the underlying policy. Our results show that an improved understanding can have adverse effects on job seekers' subsequent labor market outcomes when the underlying incentives are not suitable to promote sustainable employment. That is, our analysis also provides new insight with respect to the effectiveness of promoting marginal employment (see e.g. Booth et al., 2002; Heinrich et al., 2005) and supports the notion that locking-in effects may hamper the transition from unemployment to regular employment (see e.g. Fremigacci and Terracol, 2013; Kyyrä et al., 2013).

The paper also adds to a growing literature studying policy and program related information interventions, which have addressed limited knowledge, in-transparent choice sets or high transaction costs related to educational choices (Hastings and Weinstein, 2008; Jensen, 2010; Bettinger et al., 2012; Wiswall and Zafar, 2014), income support programs (Alatas et al., 2016; Stephan et al., 2016), food stamps (Finkelstein and Notowidigdo, 2019), tax credits (Bhargava and Manoli, 2015) or medical support (Kling et al., 2012).

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Specifically, and very relevant for the context of our analysis, various studies investigated how misunderstanding of prevailing rules or economic incentives affects job search behavior (Altmann et al., 2018) and labor supply decisions (Chetty and Saez, 2013; Liebman and Luttmer, 2015).[3] Finally, the paper also contributes to an extensive empirical literature investigating the effects of the generosity of UI systems on labor market outcomes. There is comprehensive evidence that extensions of the PBD period encourage individuals to search less intensively for new jobs (Lichter, 2016; Marinescu, 2017), increase the time spent in unemployment and non-employment(Katz and Meyer, 1990; Card and Levine, 2000; Lalive et al., 2006; Van Ours and Vodopivec, 2006; Chetty, 2008; Schmieder et al., 2012, 2016), and result in ambiguous effects on the quality of subsequent job matches(Le Barbanchon et al., 2019; Centeno and Novo, 2009; Nekoei and Weber, 2017; Van Ours and Vodopivec, 2008).[4]

The remainder of the paper is organized as follows. Section 2 briefly describes the complexity of the Danish UI benefit rules. Section 3 presents the experimental design of the intervention. Section 4 documents individuals' existing knowledge and discusses the hypotheses that guide our empirical analysis. Section 5 shows the estimation results. Finally, Section 6 concludes.

2   Complexity of UI Benefit Rules in Denmark

Unemployment insurance benefits in Denmark are organized in a voluntary opt-in system, where unemployed workers are eligible to receive UI benefits for a period of up to two years, if they have paid contributions for at least 12 months within the last three years. The level of monthly benefits is fixed at 90% of prior wage income up to a ceiling of 18,866DKK (2019-level, equivalent to approx. 2,500€) per month before taxes. Around 85% of the Danish wage-earners participate in the system and pay contributions, while around 75% of the actual benefit recipients receive the maximum amount of UI benefits.

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[3]In a similar spirit related information interventions focused on perceived incentives related to financial decision-making (Bertrand and Morse, 2011), consumption behavior and salience of commodity taxes (Chetty et al., 2009) and health-related behavior (Dupas, 2011).

[4]In contrast to the existing evidence, the generosity of the UI system in our setting, however, depends on the individual job search and work effort after the initial registration with public employment services.

Moreover, benefit recipients are confronted with an additional set of rules,[5] which provides them with the opportunity to extend the PBD from two up to three years. The extension is granted if job seekers take up employment between the initial registration with the public employment services and the expiration of UI benefits. Each hour worked during this benefit period is converted into two extra hours of UI benefits at the expiration of the two year benchmark benefit period. Through this mechanism, individuals can prolong their PBD by maximally one year given that they have saved working hours equivalent to six months of full-time employment. The total benefit period can consist of multiple unemployment periods, which are interrupted by episodes of employment, or of periods in which the unemployed works in part-time jobs in parallel with an ongoing period of benefit receipt. An individual who re-enters unemployment after a short period of employment is eligible to receive benefits for the remaining two-year period (plus the earned extension). If the hours worked add up to one year of full-time employment, the individual is eligible for a full new two-year benefit period, while the level of benefits will be recalculated based on the average wage earned in the last jobs.

Beside the possibility to extend the PBD, benefit recipients face the threat of benefit sanctions if they do not fulfill a work requirement when receiving UI benefits. Specifically, UI benefits will lapse for one day every four months (*qualifying days*) if the benefit recipient has not worked at least 148 hours within the past four-months window, equivalent to one week of full-time work every month.

The Danish system is more flexible compared to a UI benefit system with a fixed expiration date. Since individuals can extend their own PBD by collecting working hours after the registration with public employment services, the system generally provides larger incentives to search for employment. This reduces moral hazard without reducing the job seekers' liquidity and their ability to smooth consumption increasing the efficiency of the system.[6] In combination with existing benefit sanctions and job search requirements,

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[5]The additional layer of rules was introduced in 2017 (following a political agreement from 2015) and has been in place for more than a year at the beginning our intervention.

[6]As extensively discussed by the previous literature, UI systems face a trade-off as they aim to guarantee a minimum income, which allows unemployed workers to smooth their consumption and to improve subsequent job match quality, without distorting incentives to search for new employment (see e.g. Hopenhayn and Nicolini, 1997; Acemoglu and Shimer, 2000; Chetty, 2008).

this additional feature, however, makes the system very complex to navigate for individual job seekers. This is in particular the case as the (remaining) benefit period, as well as all possible extensions are calculated on an hourly basis rendering it difficult for individual job seekers to keep an overview of their remaining benefit duration.

3   Experimental Setup

To analyze how job seekers' understanding of the complex unemployment benefit rules affects job search and labor market outcomes, we conduct a large-scale field experiment among the universe of Danish UI benefit recipients. Our intervention aims to foster the utilization of an online information tool that provides continuously updated, personalized information on individuals' benefit situation and the prevailing rules. We combine the intervention data with extensive administrative records and an online survey that allows us to measure the job seekers' knowledge.

3.1   Randomized Controlled Trial

Our randomized controlled trial (RCT) focuses on an intervention that aims to increase the understanding of the complex rules among UI benefit recipients. We rely on an online information tool, which addresses job seekers' knowledge about their personal benefit situation and corresponding rules concerning flexible PBD and the threat of sanctions. The online tool which the intervention relies on is embedded in the official online platform of the Danish public employment agency (*jobnet.dk*). It comprises various pieces of information such as (i) remaining benefit entitlements and the accumulated working hours that could be used for an extension of the PBD, (ii) the past consumption of UI benefits, (iii) the eligibility for a new two-year benefit period and (iv) the avoidance of benefit sanctions. The different elements of the online information tool are depicted in Appendix A.3.

The information provided in the online tool is personalized to the individual job seeker's specific situation and continuously updated throughout the benefit period. While the tool is publicly accessible to all UI benefit recipients during the experiment, our intervention aimed at fostering additional usage among treated individuals by drawing their attention towards the tool. Specifically, at the beginning of the intervention we randomly

divided the stock of all Danish UI benefit recipients into three groups. First, individuals in the main treatment group (*tool group*) received messages providing information about the tool, the underlying rules and a direct link to access the tool. After a somewhat longer first message, individuals received up to four monthly reminder messages conditional on their continued status as benefit recipient. Second, individuals in the *control group* received no messages. Finally, individuals in a third group (*message group*) received generic messages at the same points in time as the tool group. The messages contained general information about job search, but were unrelated to the information tool and the UI benefit rules. The purpose of the message treatment is to identify any potential isolated effect of sending out messages and reminders independent of specific content regarding the UI benefit rules and the tool. Note, all messages were sent out to people through jobnet.dk which also contains a communication module allowing the public employment services to communicate with individual job seekers. The exact content of the messages can be found in Appendix A.1.

3.2 Data

To analyze the effect of the intervention we relied on two complementing data sources. First, we conducted an online survey to directly measure job seekers' understanding of the prevailing rules and their knowledge about their own economic situation. The data from the survey is used to test, whether the intervention had the desired effect of enhancing individuals' knowledge, their sense of being informed and their confidence regarding their own benefit duration. In addition, the survey serves to explore other potential mechanisms through which the intervention might affect the behavior of treated individuals. We examine whether the intervention had effects on (i) individuals' overall motivation and (ii) their subjectively perceived sense of being monitored and its intensity, i.e., their sense of pressure from the labor market authorities. Finally, we elicit additional information on job search behavior, sources of information on rules, and the perceived attractiveness of temporary, respectively part-time jobs, to complement the administrative data on realized labor market outcomes.

Moreover, to investigate the consequences of the intervention for subsequent labor market outcomes, we link the experimental data to the comprehensive register data administered by Statistics Denmark. This provides us with detailed information on socio-demographic background characteristics obtained from population registers, uptake of public transfers (DREAM) including full and partial unemployment insurance benefits, as well as monthly wages and employment status (E-income), including labor market histories of individuals in our sample. We construct various outcome variables that allow us to identify potential effects of the intervention on cumulated labor market outcomes, i.e. overall working hours and earnings, characteristics of accepted jobs and imposed benefit sanctions. Finally, we also exploit data on registered job applications from an official online platform of the public employment services, which is called *joblog*. UI benefit recipients are legally required to document their job search activities, which is typically used as the basis for individuals' meetings with their caseworkers (see Fluchtmann et al., 2019).

3.3 Time Schedule and Sampling

Our intervention targets the full stock of UI benefit recipients at the beginning of March 2018. For our analysis, we focus only on those individuals, who are full-time insured, which yields an estimation sample of 98,641 individuals.[7] The timing of our study is depicted in Figure 1. Sampling and treatment assignment took place one week before the beginning of the intervention ($t = -1$ in Figure 1).

Figure 1: Time line of the RCT (in weeks)

Sampling Pre Survey	Treatment	Reminder 1	Main Survey	Reminder 2	Reminder 3	Reminder 4
$t = -1$	$t = 0$	$t = 4$	$t = 5$	$t = 8$	$t = 12$	$t = 16$

Individuals are randomly assigned to three equally sized treatment groups (tool, message and control) with approximately 33,000 individuals in each. At the beginning of

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[7]We exclude 1,804 benefit recipients who are part-time insured, since they are confronted with a different set of rules. The part-time insured only pay a share of the full contribution and therefore only receive the corresponding share of the full amount of UI benefits.

week $t = 0$ (March 05, 2018), the corresponding messages are sent to the tool and message groups, respectively. Subsequently, individuals in both groups receive up to four reminder messages (in weeks $t = 4, 8, 12$ and 16). Only job seekers who are still registered as unemployed within the four-week period prior to the sending date receive the reminder messages. Importantly, all messages are sent out by the public employment service to the personal inbox on the official online portal, *jobnet.dk*, that all UI benefit recipients are required to visit at least once a week.

Individuals who exit the UI system during the intervention do not receive reminder messages, unless they re-enter the system. In this case, they return into their originally assigned treatment status and receive the subsequent reminders.

We invite a subsample of about 30% of the overall population to the online survey discussed in Section 3.2. Individuals are incentivized to answer the survey as they may participate in a lottery for 200 gift certificates of 60€ each. We invite 22.5% of the total population to our main survey (with 7.5% from each treatment cell). The survey is administered in $t = 5$ and is sent to participants independently of their current employment status. The main survey is preceded by a pre-survey administered in the pre-intervention week $t = -1$, i.e., in the week before the beginning of the intervention. We invite 7.5% of the total population to the pre-survey in $t = -1$ (2.5% each from treatment cells). The pre-survey allows us to measure the baseline (pre-treatment) levels of the survey outcomes to, e.g., document existing knowledge gaps among UI benefit recipients.

Survey participants are officially invited by the public employment service on behalf of the University of Copenhagen using their private e-mail addresses.[8] Using a different communication channel and a different sender than for the treatment messages reduces the risk that respondents connect the online survey to the treatment messages. The overall response rate is about 14%, while it is slightly lower for the main survey, since respondents, who have already left unemployment, are less likely to answer the survey.[9]

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[8]Only participants who agreed to be contacted by the public employment service via e-mail are invited to the survey. This applies for about 50% of the sample population.

[9]Detailed descriptive statistics regarding survey take-up and characteristics of survey respondents are presented in Table A.1 and Table A.2 in Appendix A.4.

4 Descriptive Evidence and Hypotheses

In this section we present some descriptive statistics regarding the most relevant dimensions of our intervention, describe the hypotheses which guide our subsequent empirical analysis and discuss treatment take-up and balancing statistics.

4.1 Knowledge and job search

Before analyzing the effects of the information treatment in Section 5, we first document job seekers' existing knowledge about the UI benefit rules and their personal situation before the intervention. Moreover, we shed light on the association between individual knowledge, elapsed benefit duration and job search behavior. Throughout the following analysis, we focus on job seekers' who have not received an information treatment before being invited to the survey, i.e. respondents from the pre-survey and the control group of the main survey.

UI benefit rules and personal situation: To measure the individual knowledge about the complex rules, we rely on several survey items that test the respondents' understanding of the UI benefit rules as discussed in Section 2. In particular, respondents answer four questions directly related to the possibility to extend the PBD, one question related to the income effect of taking-up a short-term work opportunity and one question related to the avoidance of benefit sanctions.[10] The survey questions aim to pinpoint the understanding of the rules and the resulting economic incentives while being unemployed. We also construct a composite *knowledge index* that measures individuals' understanding based on the frequency of correct answers.[11] Beside measures of individuals' actual knowledge, the survey also elicits perceived difficulties regarding the understanding of the benefit rules.

Panel A of Table 1 shows descriptive statistics for the survey items related to the UI benefit rules. On average, respondents answer about 51% of the knowledge questions cor-

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[10]See questions Q26, Q27, Q31, Q32 (PBD extension), Q28 (income effect) and Q30 (benefit sanction) in Appendix A.2 for the exact survey items.

[11]The index includes all six questions mentioned above. A limited understanding of rules as reflected in a low frequency of correct answers.

Table 1: Knowledge about UI benefit rules and personal situation

	Mean	SD
A. UI benefit rules		
No. of observations	2,181	
Knowledge index	0.512	0.270
PBD extension		
Existence of extension (Q26)	0.760	0.427
Extension gained (Q27)	0.299	0.458
Required period (Q31)	0.389	0.488
Maximum extension (Q32)	0.443	0.497
Income effect (Q28)	0.529	0.499
Benefit sanction (Q30)	0.480	0.500
Rules are hard to understand[a]	6.516	2.795
B. Expiration of UI benefits[b]		
No. of observations	1,843	
Inaccuracy of benefit duration in weeks[c]	8.907	15.164
Overestimating benefit duration	0.288	0.453
Underestimating benefit duration	0.351	0.477
Certainty about expiration date[d]	6.810	2.966

Note: Percentage share unless indicated otherwise. $P-$values measured based on two-tailed t-tests on equal means.
[a] Measured on a 10-point Likert-scale ranging from 1=disagree to 10=agree.
[b] Only observed for individuals who are still unemployed at the moment of the main survey.
[c] Refers to the absolute difference between the subjectively expected and actual remaining benefit duration in weeks.
[d] Measured on a 10-point Likert-scale ranging from 1=low to 10=high.

rectly, which indicates that there is substantial scope for improving on the understanding of rules among Danish jobseekers. Moreover, there is substantial variation with respect to the rate of correct answers across the single questions. While a large share of respondents (about 76%) knows that it is generally possible to extend the PBD (Q26), only 27-42% know how the exact extension is calculated (Q27 and Q31), and about 44% are familiar with the maximum extension that is available (Q32). Similarly, we observe rates of correct answers of 47% and 52% regarding the effect of accepting a short-term work opportunity on the overall personal income (Q28) and the imposition of benefit sanctions (Q30), respectively. Moreover, there is no evidence that job seekers become more knowledgeable about the rules over time, since the share of correct answers does not increase, when comparing the pre-survey to the control group in the main survey, while the perceived difficulty in understanding the benefit rules is slightly lower during the main survey ($p-$value $= 0.063$).

Beside their understanding of the general rules, we also measure individuals' knowledge about their own situation. In particular, we ask them to state the date when their UI benefits will expire (assuming that they will not work any additional hours) and compare their answer to their actual expiration date. As shown in Panel B of Table 1, the absolute difference between the expected and actual expiration date is about nine weeks on average, while about 35% (29%) underestimate (overestimate) their remaining benefit duration. Finally, in a subsequent question respondents are also asked to report how certain they are about their expiration date.

The relevance of job seekers' knowledge about the UI benefit rules and their personal expiration date obviously depends on the likelihood of still being unemployed when UI benefits expire. Various studies have shown that benefit recipients adjust their job search behavior substantially over the course of the unemployment spell (see e.g. Marinescu and Skandalis, 2019) depending on the remaining benefit duration (Lichter, 2016; Marinescu, 2017). Exit rates typically increase when approaching the expiration date (Katz and Meyer, 1990; Card and Levine, 2000; Card et al., 2007a) and spike around benefit exhaustion (Lalive et al., 2006; Card et al., 2007b). Hence, we expect the possibility to extend the PBD and therefore also job seekers' knowledge about the UI benefit rules and their personal situation to have a stronger impact on the incentives to search for employment, the closer they are to their expiration date. In the following, we document the connection between job seekers' personal knowledge and their elapsed benefit duration. Specifically, we divide the stock of UI benefit recipients into three subgroups: (i) short-term benefit recipients with an elapsed benefit period of less than six months at the beginning of the intervention, (ii) medium-term benefit recipients with an elapsed benefit period of six to twelve months and (iii) long-term benefit recipients with an elapsed benefit period of more than a year.[12]

Figure 2 shows the cumulated distributions of the knowledge measures discussed in Section 4.1 for the three groups of job seekers. When considering the job seekers' actual knowledge about the benefit rules (measured by the knowledge index; see Panel A), we

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[12]The first threshold (above/below six months) is oriented towards the official definition of long-term unemployment used by the Danish public employment service affecting the eligibility for various ALMP programs, such as training and wage subsidies. The second threshold (above/below one year) refers to the official international definition of long-term unemployment used by the OECD (2019).

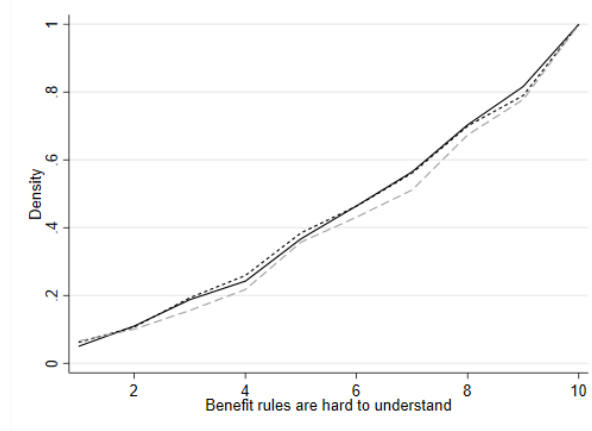Figure 2: Distribution of knowledge by elapsed benefit duration

A. Actual knowledge
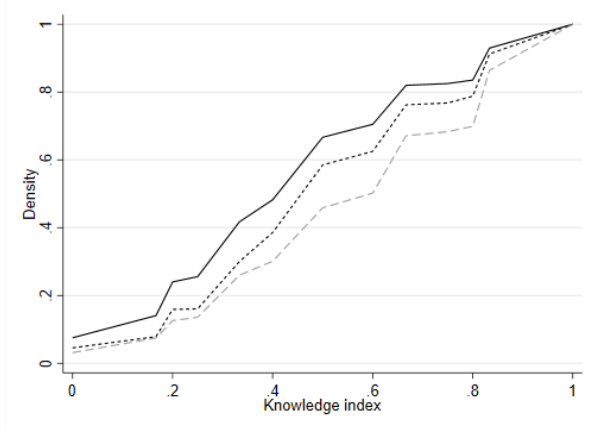
P−value[a]

short v. medium	0.000
short v. long	0.000
medium v. long	0.000

B. Perceived knowledge

P−value[a]

short v. medium	0.970
short v. long	0.323
medium v. long	0.556

C. Accuracy benefit expiration
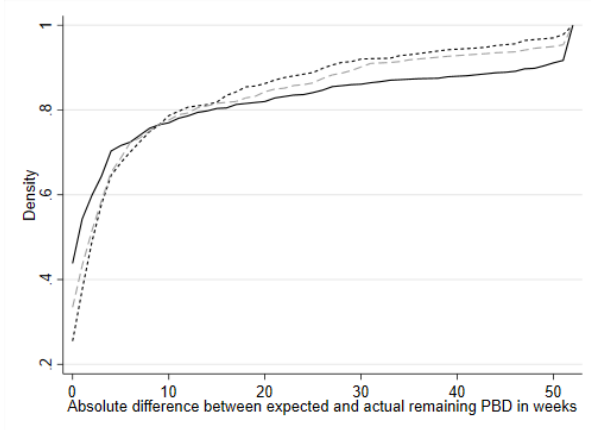
P−value[a]

short v. medium	0.000
short v. long	0.001
medium v. long	0.089

D. Certainty benefit expiration
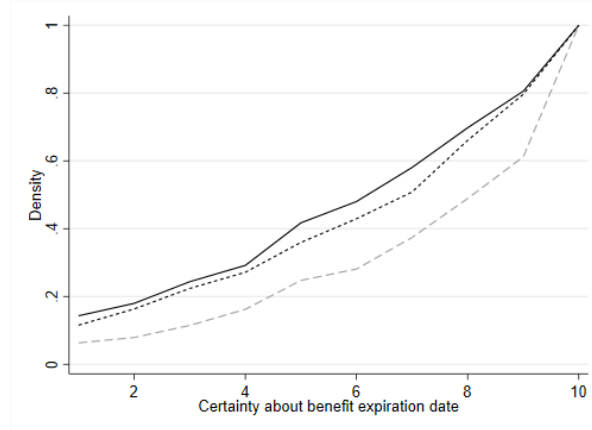
P−value[a]

short v. medium	0.053
short v. long	0.000
medium v. long	0.000

Elapsed benefit duration: ——— ≤26 weeks (short)   ····· 27-52 weeks (medium)   − − − >52 weeks (long)

[a] P-values based on two-sample Kolmogorov-Smirnov tests of the equality of distributions.

can see that knowledge increases in elapsed receipt of UI benefits. This can be seen as the distribution of knowledge for the long-term benefit recipients (gray dashed line) statistically dominates that of the medium-term (dotted line) and short-term (solid line) benefit recipients. The knowledge differences between the three groups are statistically significant at any conventional levels (Mann-Whitney test, p−value < 0.001).

The greater knowledge of individuals with a longer elapsed benefit duration is in line with the notion that the incentives to gather knowledge about the possibility to extend the PBD increase when approaching the expiration date. Interestingly, as shown in Panel B, the differences with respect to the actual knowledge do not translate into different perceptions about the complexity of the rules as we cannot reject the hypothesis that all three groups find it similarly hard to understand the UI benefit rules. Moreover, Figure 2 also reveals that there are substantial differences regarding the job seekers' knowledge about their personal situation, i.e., their remaining benefit duration (see Panel C) and how certain they feel about their benefit expiration date (see Panel D). While long-term benefit recipients generally feel more certain regarding their knowledge about the expiration date than short- and medium-term benefit recipients, the connection between the actual knowledge about benefit expiration and the elapsed benefit period is non-linear.[13]

Next, we turn to the connection between knowledge of the unemployed, their job search behavior and elapsed benefit duration. This allows us to examine the relevance of the elicited knowledge measures and to gain a better understanding of the empirical relationship between personal knowledge and key parameters of the job search process. Table 2 shows the results of a model regressing total number of job applications in the last four weeks (as elicited in the survey) on the different knowledge measures separately for short-, medium- and long-term benefit recipients.[14] Although the results do not allow for a causal interpretation (since we do not exploit the experimental variation yet) they reveal an interesting pattern.

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[13]There is a higher fraction of short-term benefit recipients who predict the expiration date very accurately, but also a higher fraction with very inaccurate predictions.

[14]We estimate separate models for explanatory variables related to the job seekers knowledge about the UI benefit rules (column 1, 3 and 5), respectively their personal situation (column 2, 4 and 6).

Table 2: Knowledge, subjective perceptions and job search behavior

Dependent variable	Number of job applications in last four weeks					
	Elapsed UI benefit duration					
	≤26 weeks		27-52 weeks		>52 weeks	
	(1)	(2)	(3)	(4)	(5)	(6)
Knowledge index (1=high)	-2.191 (1.896)		1.225 (1.975)		5.490** (2.590)	
Rules are hard to understand (10=agree)	-0.238 (0.147)		-0.019 (0.166)		0.385* (0.229)	
Knowledge index × rules are hard to understand	0.159 (0.265)		-0.065 (0.275)		-0.749** (0.350)	
Inaccuracy of PBD in weeks						
Expected > actual PBD		0.014 (0.027)		-0.004 (0.040)		0.069 (0.050)
Expected < actual PBD		0.020 (0.027)		0.011 (0.040)		0.165** (0.064)
Certainty about benefit expiration date		0.130 (0.080)		0.091 (0.091)		0.125 (0.126)
Inaccuracy of PBD in weeks × certainty		0.001 (0.004)		0.001 (0.005)		-0.011* (0.006)
$P-$value joint significance	0.062	0.193	0.678	0.743	0.165	0.111
Mean value (dependent variable)	10.20	10.20	9.94	9.94	10.89	10.89
No. of observations	805	805	511	511	450	450
Control variables	Yes	Yes	Yes	Yes	Yes	Yes

Note: Depicted are conditional correlations between various measures of actual and perceived knowledge and the number of job applications in the last four weeks based on information from the pre-survey and the control group of the main survey. Control variables include socio-demographics, household characteristics and labor market histories. Standard errors in parentheses. ***/**/* indicate statistically significance the 1%/5%/10%-level.

As expected, the relationship between individual knowledge and job search effort crucially depends on elapsed benefit duration and is more pronounced for job seekers closer to benefit expiration. For instance, the correlation between job seekers' actual knowledge and their search effort is increasing in elapsed benefit duration. While higher knowledge is (insignificantly) associated with fewer job applications for those who received UI benefits for less than 26 weeks, long-term benefit recipients search significantly more, when they have higher knowledge about the UI benefit rules. If they answer all six knowledge questions correctly they send out about 50% more job applications on average compared to a job seeker who does not answer a single question correctly. Moreover, for a given level of actual knowledge, long-term benefit recipients search harder, if they have the perception that the rules are difficult to understand, and the connection between actual knowledge and search effort is weaker, the higher the perceived difficulty is. Finally, when considering the connection between search behavior and knowledge about one's personal situation, we see from results in column (6) that long-term benefit recipients search more intensively if they underestimate their remaining benefit duration.

These findings have two important implications. First, the elicited knowledge measures are relevant predictors of the job search behavior and the empirical relationship between knowledge and search effort is in accordance with predictions of standard job search theory. Second, the results emphasize the importance of the elapsed benefit duration when analyzing the connection between knowledge and the behavior of unemployed workers. Therefore, we explicitly consider heterogeneity with respect to elapsed UI benefit duration when analyzing the causal effects of our information treatment.

4.2   Hypotheses

Given the substantial knowledge gaps among benefit recipients, we expect the intervention to increase the knowledge of the treatment group regarding the prevailing UI benefit rules and their personal economic situation. Furthermore, the design of the UI benefit system implies that a better understanding of the rules makes treated individuals aware of the incentives to search for and accept marginal employment. We therefore expect an increased effort devoted towards the search for temporary and part-time jobs in the tool group.

In contrast to the hypothesis regarding knowledge and job search behavior, the consequences for the subsequent labor market prospects remain theoretically ambiguous (see Ek and Holmlund, 2015). On one hand, part-time and temporary jobs can provide a stepping stone towards regular employment as they can send a signal of effort, provide a valuable network, and reduce the employers' uncertainty about the quality of a candidate. Previous evidence indicates that non-regular jobs can be a stepping stone towards permanent employment in the UK (Booth et al., 2002), Germany (Freier and Steiner, 2007), France (Fremigacci and Terracol, 2013), the US (Heinrich et al., 2005), Finland (Kyyrä, 2010) and Denmark (Kyyrä et al., 2013), while in some cases positive effects only appear for the long-term unemployed (Caliendo et al., 2016; Gerfin et al., 2005). On the other hand, working in a non-regular job can be associated with a locking-in effect, since individuals spend less time searching for regular full-time jobs meanwhile, which can lead to negative employment effects in both the short- (see Fremigacci and Terracol, 2013; Kyyrä et al., 2013) and long-run (see Böheim and Weber, 2011).

Moreover, as argued above, the possibility to extend the PBD makes the UI system more generous, in general. Prior to the intervention, some job seekers might simply not be aware of the economic incentives resulting from the UI benefit rules discussed in Section 2, while others may be confused about the incentives due to the complexity of the rules. When considering the individual answers to questions testing job seekers' knowledge about the UI benefit system, a majority of about 70% individuals tend to underestimate the generosity of the system. Moreover, 34% of job seekers underestimate their own remaining benefit period, whereas only 30% overestimate the duration until the UI benefits will expire ($p-$value $= 0.021$).[15] Therefore, informing job seekers about the possibility to extend their benefit period might have similar consequences as actual extensions of the PBD. While a more generous system reduces the pressure to accept low-quality jobs, which could lead to higher realized wages (Nekoei and Weber, 2017), it may also encourage individuals to search less intensively for new jobs (Lichter, 2016; Marinescu, 2017) and increase their

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[15]Since the additional UI benefit rules, including the possibility to extend the PBD, were introduced in 2017, it might be the case that many UI benefit recipients have not updated their knowledge yet.

time spent in unemployment (Katz and Meyer, 1990; Card and Levine, 2000; Lalive et al., 2006; Van Ours and Vodopivec, 2006; Chetty, 2008; Schmieder et al., 2012, 2016).

In addition, in contrast to actual unconditional PBD extensions, our tool treatment might increase job seekers' uncertainty about their remaining benefit period and therefore also about their future income stream. The higher uncertainty about the PBD decreases the value of remaining unemployed for risk averse job seekers ceteris paribus, which could encourage them to exert higher levels of search effort in order to leave unemployment (Pissarides, 1974; Feinberg, 1977).

These ambiguous effects on people's labour market outcomes of course also translate into ambiguous treatment effects on people's ability to avoid benefit sanctions.

As argued before, the relevance of the different mechanisms depends on the remaining period until UI benefit expiration. For instance, job seekers who are close to benefit expiration are more likely to suffer from the exhaustion of UI benefits and therefore have greater short-run incentives to gather and process information about possibilities to extend the PBD, and to adjust their individual behavior accordingly. This argument is supported by the descriptive evidence presented in Section 4.1 documenting a stronger relationship between knowledge and search effort among long-term benefit recipients. However, it has also been shown that long-term benefit recipients already have greater knowledge prior to the intervention and this might limit the impact of the information provided through the online tool. Moreover, job seekers early in their unemployment spell have more time to gather working hours that can be used to extend the PBD. This increases their long-run returns from utilizing knowledge about the benefit rules, but it also increases uncertainty about their remaining benefit duration and the relevance of a potential benefit extension.

4.3 Treatment Take-up and Balancing Tests

To assess treatment take up, we rely on two types of information. First, we observe individual-level data on whether individuals in the treatment, respectively in the message group, opened the messages and clicked on the link to the online information tool. As shown in Table 3, more than 90% of participants in both the message and treatment group opened the first message and more than 85% of those, who received the first re-

minder also opened the corresponding message. It should be noted that the number of

Table 3: Treatment take-up: clicking behavior by treatment status

	Main message	Reminder 1	Reminder 2	Reminder 3	Reminder 4
Date sent	March 05, 2018	April 03, 2018	April 30, 2018	May 28, 2018	June 25, 2018
Tool treatment					
Message received[a]	32,857	30,460	26,905	22,839	19,968
Message opened[b]					
total	30,717	26,806	22,904	19,366	16,777
share of received	0.935	0.880	0.851	0.848	0.840
Click on link[c]					
total	6,539	6,311	4,747	3,949	3,711
share of received	0.199	0.207	0.176	0.173	0.186
share of opened	0.213	0.235	0.207	0.204	0.221
Message treatment					
Messages received[a]	32,874	30,552	26,927	22,801	19,941
Messages opened[b]					
total	30,946	27,420	23,761	20,082	17,663
share of received	0.941	0.897	0.879	0.881	0.886

Note: Depicted are summary statistics with respect to the take-up of the information treatment separated for the tool and message treatment.
[a]Refers to the total number of individuals receiving the corresponding message to their inbox on *jobnet.dk*. Reminders are only sent to individuals who have been registered as UI benefit recipients within the last four weeks before the date of the reminder.
[b]Refers to all individuals opening the corresponding message.
[c]Refers to all individuals clicking on the link to the online information tool.

individuals receiving treatment messages is reduced over the course of the intervention as job seekers gradually exit unemployment. For each message sent to job seekers in the tool treatment, about 20% of participants who receive the message click on the link to the online information tool. Across all treatment messages (the main message and the monthly reminders), about 45% of all participants in the tool treatment eventually clicked on the link to the tool at least once. Moreover, we document the overall usage of the tool by exploiting aggregate data from Google Analytics. Visits to the tool increased by 50% relative to the pre-intervention period during our intervention. As shown in Figure 3, the increase is concentrated to a few days around the dates of sending out the intervention messages, but the additional page visits do not crowd out usage in periods between messages.

Moreover, Table 4 provides an overview of individual background characteristics separated by treatment status. The job seekers in our experiment are on average 40 years old, about 52% of participants are female, 34% are married, and a similar share have a

Figure 3: Treatment take-up: usage of online information tool over time

university degree. The average participant spent about 51 weeks in unemployment during the past five years, had an average gross monthly labor income of 17-20,000DKK (equivalent to approx. 2,250-2,650€), worked on average 19-24 hours per week and had 1.2 to 1.4 different employers on average over a year. It can be seen that most of the background characteristics are balanced across treatment groups (see columns (4)-(6)). We find no statistically significant differences in demographic characteristics between the treatment, message, and control group, except for some minor differences with respect to the time spent in unemployment in the past. We condition on a rich set of covariates in our empirical model to address any minor differences between treatment arms.

Table 4: Descriptive Statistics by Treatment Status

	Treatment status			P−values		
	Control (C)	Message (M)	Tool (T)	M v. C	T v. C	T v. M
No. of observations	32,905	32,876	32,860			
Educational level						
Missing	0.082	0.082	0.087	0.902	0.031	0.023
Less than high school	0.176	0.176	0.181	0.952	0.166	0.148
High school	0.402	0.401	0.397	0.785	0.161	0.259
Bachelor degree (or equiv.)	0.241	0.241	0.241	0.848	0.861	0.987
Master degree (or equiv.)	0.098	0.100	0.095	0.361	0.211	0.031
Male	0.482	0.475	0.481	0.071	0.765	0.133
Age						
18-25 years	0.117	0.118	0.115	0.740	0.355	0.209
26-35 years	0.332	0.332	0.331	0.912	0.703	0.786
36-45 years	0.192	0.191	0.195	0.941	0.257	0.228
46-55 years	0.195	0.198	0.194	0.244	0.904	0.199
56-65 years	0.165	0.161	0.165	0.186	0.838	0.127
Household size						
One person	0.195	0.195	0.193	0.987	0.565	0.555
Two persons	0.344	0.342	0.345	0.681	0.608	0.356
Three persons	0.204	0.204	0.200	0.922	0.180	0.214
Four or more persons	0.258	0.259	0.262	0.602	0.234	0.504
Married	0.338	0.339	0.345	0.806	0.054	0.093
Children						
One child	0.164	0.164	0.162	0.977	0.431	0.414
Two or more children	0.172	0.171	0.173	0.820	0.586	0.440
Migration status						
1st generation	0.193	0.191	0.198	0.524	0.103	0.023
2nd generation	0.032	0.034	0.033	0.222	0.767	0.355
No. of joblogs in last 4 weeks	6.962	6.998	6.926	0.316	0.326	0.045
Weeks of UI benefits	31.776	32.439	32.235	0.003	0.041	0.367
Months employed in year						
t-1	6.049	5.994	6.032	0.109	0.611	0.273
t-2	8.008	7.966	8.043	0.230	0.304	0.026
t-3	8.404	8.350	8.406	0.123	0.942	0.106
t-4	8.093	8.024	8.078	0.059	0.678	0.141
t-5	7.734	7.685	7.717	0.193	0.639	0.406
Average monthly earnings in year						
t-1	17,868	17,752	17,833	0.425	0.838	0.628
t-2	19,841	19,742	19,828	0.423	0.914	0.499
t-3	19,277	19,155	19,262	0.303	0.899	0.362
t-4	18,107	17,928	18,120	0.134	0.911	0.107
t-5	16,904	16,827	16,901	0.605	0.976	0.620
Average employment degree in year						
t-1	0.519	0.517	0.515	0.507	0.191	0.520
t-2	0.638	0.638	0.637	0.908	0.652	0.737
t-3	0.648	0.647	0.650	0.625	0.700	0.382
t-4	0.619	0.615	0.619	0.176	0.927	0.207
t-5	0.586	0.580	0.585	0.070	0.778	0.125
No. of employers in year						
t-1	1.268	1.273	1.284	0.514	0.051	0.195
t-2	1.342	1.334	1.344	0.327	0.800	0.214
t-3	1.348	1.338	1.350	0.201	0.749	0.109
t-4	1.271	1.266	1.277	0.526	0.453	0.167
t-5	1.221	1.206	1.214	0.058	0.374	0.314
Weeks of UI benefits						
in last year	24.073	24.435	24.244	0.003	0.163	0.120
in last 2 years	33.602	34.235	33.889	0.001	0.141	0.077
in last 5 years	50.231	50.802	50.720	0.061	0.109	0.787
Weeks of supplementary UI benefits						
in last year	2.373	2.469	2.442	0.022	0.096	0.525
in last 2 years	3.294	3.474	3.419	0.001	0.023	0.329
in last 5 years	5.177	5.404	5.416	0.007	0.004	0.885

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Note: Percentage share unless indicated otherwise. *P*−values measured based on two-tailed t-tests on equal means.

5 Effects of the Tool Treatment

The primary objective of our intervention is to exogenously increase participants' knowledge regarding the UI benefit rules and their own economic situation. Therefore, we start by presenting treatment effects on job seekers' knowledge based on the survey data collected five weeks after the intervention. In a second step, we analyze whether potential knowledge effects translate into realized labor market outcomes such as overall working hours and earnings. Finally, we investigate consequences for job characteristics and the job search behavior of unemployed workers.

To identify the causal effects of our intervention we estimate the following empirical models:

$$Y_i = \beta_0 + \beta_1 (D_i \times EBD_i) + \beta_2 X_i + \varepsilon_i, \tag{1}$$

where D_i indicates the treatment status (dummy variables for the control, tool, respectively message group) that is interacted with an indicator of an individual's elapsed benefit duration EBD_i at the start of the intervention. As discussed in Section 4.1, we distinguish between three groups (short-, medium-, and long-term benefit recipients), which allows us to examine the optimal timing of treatment during the unemployment spell. Moreover, X_i characterizes a vector of pre-intervention control variables, i.e. socio-demographic characteristics and labor market histories, as presented in Table 4, plus dummies for place of residence (98 municipalities) and membership of unemployment funds (24 in total), while Y_i denotes the different outcome variables of interest. The model estimates intention-to-treat effects (ITTs), ignoring whether treated individuals actually read the message and clicked on the link to the tool in order to avoid selection bias.

5.1 Knowledge about Benefit Rules and Personal Situation

Table 5 shows the effect of the intervention on individuals' knowledge about the UI benefit rules and their own economic situation. The results in column (1) show that individuals in the tool group are significantly better at giving correct answers to the knowledge questions than individuals in the control group. The improvement in knowledge on the composite index amounts to 5 percentage points, equivalent to an improvement of 10% compared

to the baseline level of the control group. When considering the three subgroups based on elapsed benefit period at the beginning of intervention in column (2), we see that the effect of the tool treatment on the score of the composite index is similar across subgroups ranging from 4.5 to 5.4 percentage points. Despite being better at the outset and having the highest benchmark level of knowledge, long-term benefit recipients still experience significant knowledge improvements of a similar order of magnitude as the other subgroups. Moreover, when considering the respondents' perceived knowledge in columns (3) and (4), our results indicate that the tool treatment reduces the perceived difficulties to understand the rules only among long-term benefit recipients who have been unemployed for more than 52 weeks already.

Table 5: Treatment effects on perceived and actual knowledge

Dependent variable	Knowledge index (0=low; 1=high)		Rules are hard to understand (1=disagree; 10=agree)		Inaccuracy of PBD (in weeks)		Certainty about PBD (1=low; 10=high)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tool treatment	0.0498*** (0.0123)		-0.0905 (0.135)		-1.668** (0.837)		0.651*** (0.158)	
Tool × UI benefit duration ≤26 weeks		0.0459*** (0.0175)		0.0976 (0.190)		-2.304* (1.224)		0.657*** (0.228)
Tool × UI benefit duration 27-52 weeks		0.0546** (0.0237)		-0.0163 (0.258)		0.358 (1.534)		0.653** (0.292)
Tool × UI benefit duration >52 weeks		0.0524** (0.0261)		-0.600** (0.285)		-2.885* (1.702)		0.612* (0.324)
Message treatment	0.0247** (0.0123)		0.185 (0.134)		-0.135 (0.819)		0.206 (0.153)	
Message × UI benefit duration ≤26 weeks		0.0341* (0.0180)		0.365* (0.197)		-1.998 (1.251)		0.323 (0.230)
Message × UI benefit duration 27-52 weeks		0.0231 (0.0232)		0.0973 (0.252)		0.168 (1.480)		0.186 (0.279)
Message × UI benefit duration >52 weeks		0.0106 (0.0248)		-0.0767 (0.271)		2.408 (1.582)		0.0231 (0.302)
No. of observations	2,805	2,805	2,724	2,724	2,000	2,000	2,185	2,185
P-value tool v. message								
full sample	0.0453		0.0440		0.0678		0.00493	
UI benefit duration ≤26 weeks		0.518		0.181		0.809		0.157
UI benefit duration 27-52 weeks		0.189		0.662		0.901		0.110
UI benefit duration >52 weeks		0.0979		0.0564		0.00122		0.0577
Mean value control group								
full sample	0.505		6.391		8.782		6.610	
UI benefit duration ≤26 weeks		0.457		6.265		9.639		6.228
UI benefit duration 27-52 weeks		0.522		6.408		7.245		6.388
UI benefit duration >52 weeks		0.591		6.660		9.018		7.698
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Depicted are intention-to-treat effects of the tool and message treatment relative to the control group for participants in the main survey. Standard errors in parentheses. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

A second set of outcome variables refer to the job seekers' knowledge about their remaining benefit duration. As shown in column (5) of Table 5, the tool treatment also improves the participants' knowledge about their benefit duration. On average, it reduces the absolute difference between expected and actual expiration date by about 1.7 weeks and the effect is statistically significant at the 5%-level. Again, the effect is particularly pronounced for long-term benefit recipients, but also for those who are still at the beginning of their unemployment spell (see column (6) of Table 5). Moreover, as shown in columns (7) and (8), the tool treatment increases the perceived knowledge about the remaining benefit duration. Across all three subgroups, treated individuals report a significantly higher certainty about their benefit expiration date.

Finally, we also find a positive effect of the message treatment on the knowledge index relative to the control group, which is, however, significantly lower than the effect of the tool treatment. The subgroup analysis reveals that this effect is completely driven by short-term benefit recipients who have been unemployed for less than 26 weeks. Although the message treatment does not contain any information about the UI benefit rules or the job seeker's personal situation it might encourage job seekers to explore the online portal of the PES (*jobnet.dk*). One could speculate that this is particularly beneficial and increases the knowledge of newly registered job seekers who have not yet become accustomed with the online portal. The message treatment has no effect on the job seekers' actual and perceived knowledge about their personal situation.

5.2 Realized Labor Market Outcomes

In a second step, we examine whether the knowledge increase is associated with treatment effects on realized labor market outcomes. Specifically, we consider ITTs on working hours and labor earnings (i) cumulated over a one-year horizon following the first treatment message, (ii) monthly outcomes for the first 18 months and (iii) job characteristics. In contrast to the knowledge measures elicited through the online survey, realized labor market outcomes are available for the full sample population.

Table 6 presents the treatment effects on cumulated working hours and earnings. As shown in specifications (1) and (3), the tool treatment has no effect on the average UI

benefit recipient. However, when considering the three subgroups based on elapsed benefit duration, the tool treatment significantly reduces cumulated working hours and earnings of long-term benefit recipients. Over the course of one year, individuals in the treatment group work on average about 5 hours less and earn about 3,500DKK less than those in the control group. This is equivalent to relative effects of 3.3%, respectively 3.0% (compared to the baseline level of the control group), while the effects are statistically significant at the 5%-, respectively 10%-level. There are no significant effects on the cumulated labor market outcomes of short-, respectively medium-term benefit recipients. This indicates that the incentives to adjust individual behavior differ across subpopulations based on elapsed benefit period although the intervention had very similar effects on the job seekers' knowledge.

Before focusing on additional outcome variables shedding further light on heterogeneous treatment effects of the tool treatment, it should be noted that the message treatment has no significant effects on employment and earnings among any subgroup. This may indicate that the increase in understanding of the general rules is not applied to the individual's own situation. Since there is no evidence that the message treatment affects realized labor market outcomes, we focus our subsequent discussion on the effects of the tool treatment, but for completeness we also report results for the message treatment.

First, we examine the dynamics behind the cumulated labor market outcomes. Figure 4 shows the effects of the tool treatment on monthly working hours and earnings over the first 18 months after the start of the intervention separated for short-, medium- and long-term benefit recipients.[16] For the long-term benefit recipients who are unemployed for more than 52 weeks, the negative effect of the treatment first appears about four months after the start of intervention, while monthly working hours and earnings remain below the level of the control group over the full observation period. Interestingly, we also find evidence of emerging positive treatment effects (more working hours and higher earnings) for short-term benefit recipients in the long-run (about 16-17 months after the start of the intervention).

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[16]Treatment effects of the message treatment on labor market outcomes over time are shown in Figure A.2 in Appendix A.4.

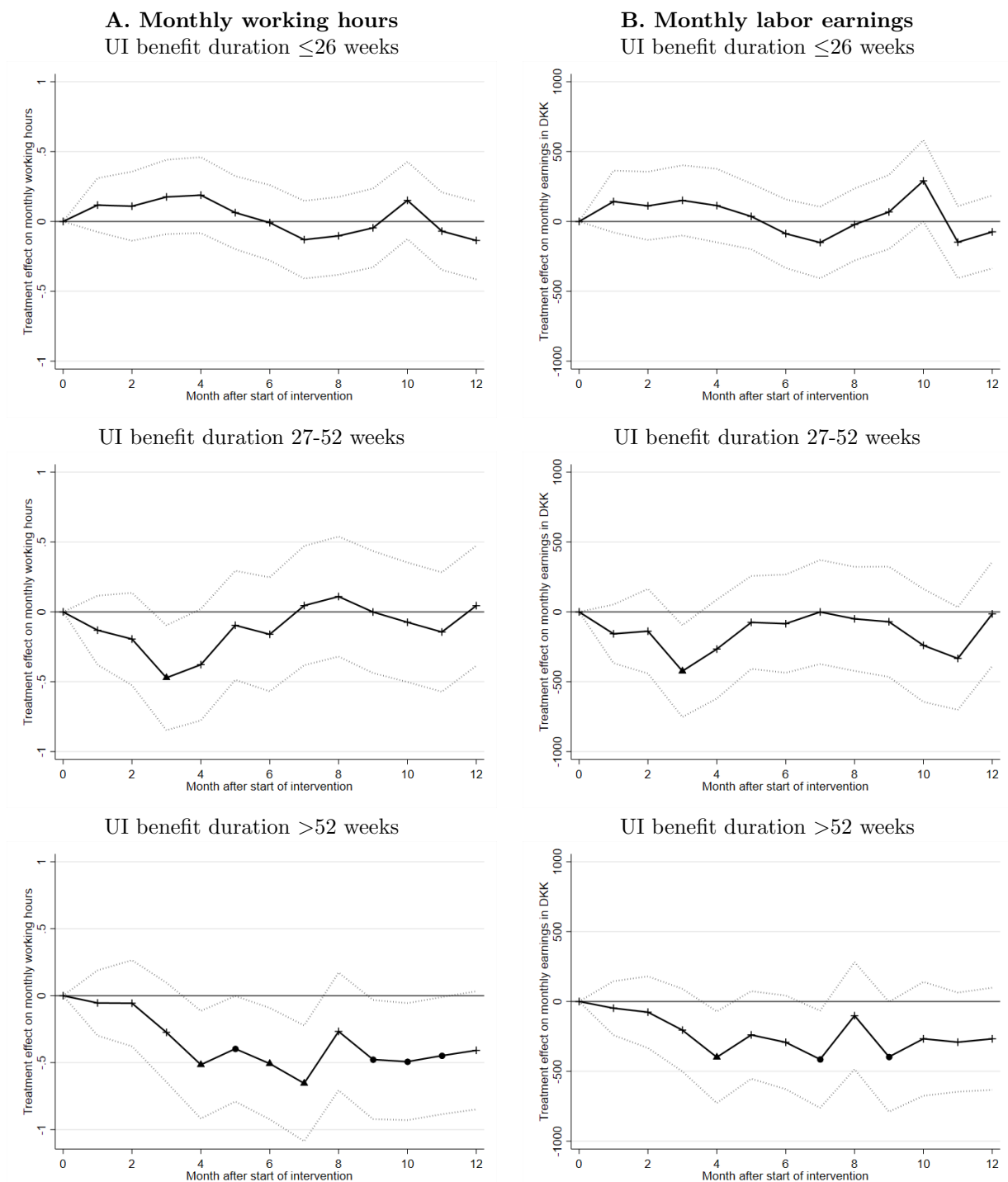Table 6: Treatment effects on cumulated labor market outcomes

Dependent variable	Working hours within 12 months		Labor earnings within 12 months	
	(1)	(2)	(3)	(4)
Tool treatment	-5.21		-911	
	(4.63)		(952)	
Tool × UI benefit duration ≤26 weeks		2.66		703
		(6.28)		(1,290)
Tool × UI benefit duration 27-52 weeks		-5.92		-1,803
		(9.55)		(1,963)
Tool × UI benefit duration >52 weeks		-21.82**		-3,539*
		(9.85)		(2,025)
Message treatment	-0.68		-298	
	(4.63)		(952)	
Message × UI benefit duration ≤26 weeks		-1.06		-391
		(6.27)		(1,289)
Message × UI benefit duration 27-52 weeks		4.67		245
		(9.61)		(1,975)
Message × UI benefit duration >52 weeks		-4.91		-549
		(9.81)		(2,017)
No. of observations	98,641	98,641	98,641	98,641
P-value tool v. message				
full sample	0.328		0.520	
UI benefit duration ≤26 weeks		0.556		0.399
UI benefit duration 27-52 weeks		0.264		0.293
UI benefit duration >52 weeks		0.084		0.138
Mean value control group				
full sample	777.9		148,841	
UI benefit duration ≤26 weeks		869.4		171,129
UI benefit duration 27-52 weeks		673.3		126,622
UI benefit duration >52 weeks		657.6		116,160
Control variables	Yes	Yes	Yes	Yes

Note: Depicted are intention-to-treat effects of the tool and message treatment relative to the control group for all experimental participants. Standard errors in parentheses. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

Job characteristics: Next, we examine whether the treatment affects the type of jobs that unemployed workers accept. Since the benefit rules provide additional incentives to work for a limited number of hours, one would expect the tool treatment to increase the willingness to work in temporary and part-time jobs. Hence, Table 7 shows treatment effects on the characteristics of the first job that individuals start after the beginning of intervention. It should be noted that job characteristics are only observed for job seekers who actually find a job within the observation period and we do not account for the potential selection into employment. However, about 87% of all participants in the

Figure 4: Effects of tool treatment on labor market outcomes over time

A. Monthly working hours
UI benefit duration ≤26 weeks

B. Monthly labor earnings
UI benefit duration ≤26 weeks

UI benefit duration 27-52 weeks

UI benefit duration 27-52 weeks

UI benefit duration >52 weeks

UI benefit duration >52 weeks

Note: Depicted are intention-to-treat effects of the tool treatment relative to the control group for all experimental participants including 90% confidence intervals. In all models, we control for socio-demographic characteristics, labor market histories and place of residence (98 municipalities) and membership of unemployment funds (24 in total). ●/▲/◆ indicate statistically significance at the 1%/5%/10%-level. Effects of the message treatment are depicted in Figure A.2 in Appendix A.4.

experiment start a new job within 18 months and, as shown in columns (1) and (2), there is no evidence that the likelihood of finding a job or the length of period between intervention and the beginning of a first job are affected by treatment. Hence, any potential bias arising from the differential selection of treated and non-treated individuals into employment is expected to be small and analyzing treatment effects on job characteristics will provide important insights into what are the most likely causal pathways.

Table 7: Treatment effects on characteristics of first employment after start of intervention

Dependent variable	Job finding				Characteristics of first job					
	Any empl. within 12 months		Duration until first job in months		Job duration in months		Part-time job (<15 hours)		Hourly wage in DKK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Tool treatment	0.001 (0.003)		0.014 (0.023)		-0.045 (0.057)		0.001 (0.004)		2.791 (1.834)	
Tool × UI benefit duration ≤26 weeks		-0.001 (0.004)		-0.001 (0.030)		0.080 (0.076)		-0.011** (0.005)		4.389* (2.440)
Tool × UI benefit duration 27-52 weeks		0.003 (0.006)		0.005 (0.049)		-0.008 (0.122)		0.007 (0.008)		0.095 (3.895)
Tool × UI benefit duration >52 weeks		0.002 (0.006)		0.064 (0.050)		-0.405*** (0.124)		0.029*** (0.008)		1.433 (3.971)
Message treatment	-0.002 (0.003)		0.007 (0.023)		-0.016 (0.057)		-0.001 (0.004)		1.543 (1.835)	
Message × UI benefit duration ≤26 weeks		-0.006 (0.004)		-0.026 (0.031)		0.010 (0.076)		-0.006 (0.005)		0.334 (2.442)
Message × UI benefit duration 27-52 weeks		0.003 (0.006)		0.009 (0.049)		-0.040 (0.123)		0.006 (0.008)		1.025 (3.919)
Message × UI benefit duration >52 weeks		0.004 (0.006)		0.090* (0.049)		-0.069 (0.124)		0.006 (0.008)		5.129 (3.951)
No. of observations	98,641	98,641	78,789	78,789	78,789	78,789	78,789	78,789	78,789	78,789
P-value tool v. message										
Full sample	0.376		0.736		0.620		0.573		0.497	
UI benefit duration ≤26 weeks		0.182		0.412		0.367		0.300		0.099
UI benefit duration 27-52 weeks		0.985		0.933		0.795		0.927		0.810
UI benefit duration >52 weeks		0.830		0.596		0.006		0.006		0.348
Mean value control group										
Full sample	0.799		4.283		9.695		0.280		196.4	
UI benefit duration ≤26 weeks		0.830		3.884		10.09		0.266		202.2
UI benefit duration 27-52 weeks		0.752		5.088		9.119		0.286		193.4
UI benefit duration >52 weeks		0.769		4.501		9.244		0.312		183.7
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Depicted are intention-to-treat effects of the tool and message treatment relative to the control group for all experimental participants who start employment within the first 12 months after the start of the intervention. Standard errors in parentheses. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

When considering characteristics of first jobs after intervention, we see that the tool treatment has substantial effects that strongly differ between short- and long-term benefit recipients. For those who have been unemployed for less than 26 weeks, the treatment reduces the likelihood to take up a part-time job (with less than 15 hours per week) by 4.1% (which is equivalent to 1.1 percentage points) and increases hourly wages by 2.2% (equivalent to 4.5DKK). Both effects are statistically significant at the 5%-, respectively 10%-level. In contrast, the tool treatment encourages long-term benefit recipients to work in jobs with a shorter job duration (-3.9%) and to start part-time employment more often (+9.0%). The results have important implications for the understanding of the underlying mechanisms. For job seekers who are treated early during their benefit spell, increased knowledge leads them to work in jobs of higher quality, which improves their long-run labor market outcomes overall. However, the higher knowledge translates into completely different labor market effects for unemployed workers who receive the corresponding information towards the end of their benefit period. They are encouraged to increase their acceptance of marginal employment, which leads to inferior labor market outcomes overall, since no additional employment is created.

Figure 5: Treatment effects on maximum monthly earnings
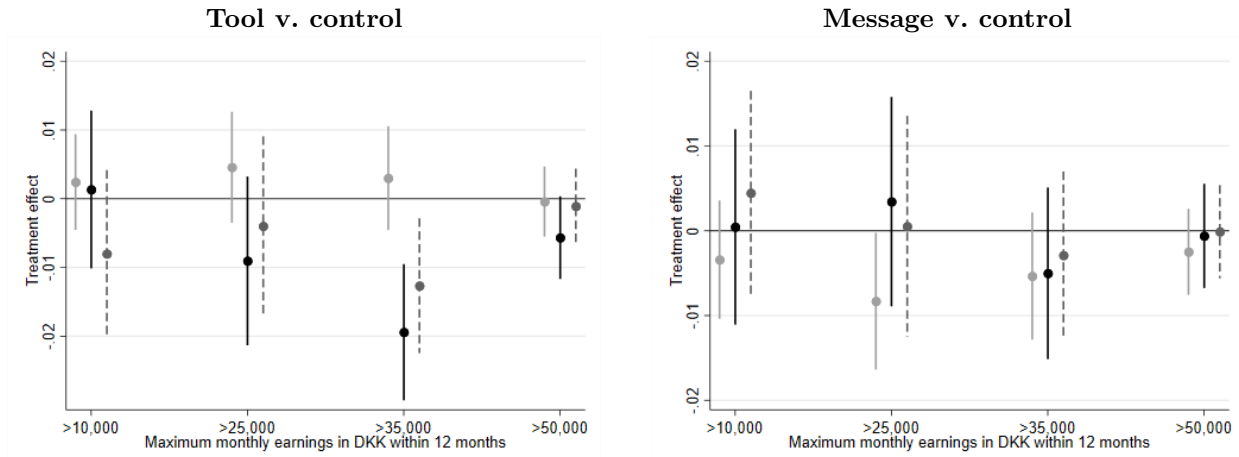
Note: Dependent variables refers to indicators taking the value one if the maximum monthly labor earnings within 12 months are above the corresponding threshold. Depicted are effects of the tool and message treatment relative to the control group and the corresponding 90% confidence interval.

To provide further evidence with respect to consequences for job quality, we also consider the maximum monthly income the individual earns within the first 12 months after intervention. Specifically, Figure 5 shows treatment effects on dummy variables indicating maximum monthly earnings above certain thresholds. It can be seen that the tool treatment significantly reduces the likelihood to obtain a monthly income above 35,000DKK ($\approx 4,700e$), which refers approximately to the 75%-quantile of the income distribution in our estimation sample. Interestingly, the effect appears not only in the sample of long-term benefit recipients, but also among those who have been unemployed for 26 to 51 weeks at the beginning of the intervention. There is no effect of the tool treatment on the short-term benefit recipients. Again, these results indicate that increased knowledge has negative consequences for job seekers' subsequent labor market outcomes among jobseekers late in their unemployment spell as they are less likely to work in high-paid jobs, while these dysfunctional effects do not appear for job seekers who receive the corresponding information early in their unemployment spell.

5.3 Avoidance of Benefit Sanctions

As discussed in Section 2, beside the possibility to extend the PBD, benefit recipients are required to work at least 148 hours within every four-months window and face a benefit sanction, i.e. UI benefits will lapse for one day, if they do not fulfill the requirement. We now investigate how the tool treatment affects the likelihood to avoid these sanctions. One could, for instance, speculate that the negative long-run effects of the tool treatment result from an attempt to avoid a benefit sanction in the short-run. Therefore, Figure 6 shows treatment effects on dummy variables indicating number of benefit sanctions within the first year after intervention. Since benefit recipients can potentially be sanctioned every fourth month, a maximum number of three sanctions can be imposed. Interestingly, the tool treatment significantly increases the likelihood of receiving three sanctions by about 1.2 percentage points for the long-term benefit recipients, but has no effect on the likelihood of receiving only one or two sanctions within the first year after intervention. This suggests that the increased knowledge does not encourage, especially long-term benefit recipients to gear their work and job search strategy towards the avoidance of benefit

sanctions. This is in line with the notion that the behavioral incentives resulting from the possibility to extend the PBD are substantially larger than those resulting from the work requirement. The increased sanction rate due to the tool treatment among long-term benefit recipients is likely to be a direct consequence of the reduced number of working hours.

Figure 6: Treatment effects on imposed benefit sanctions
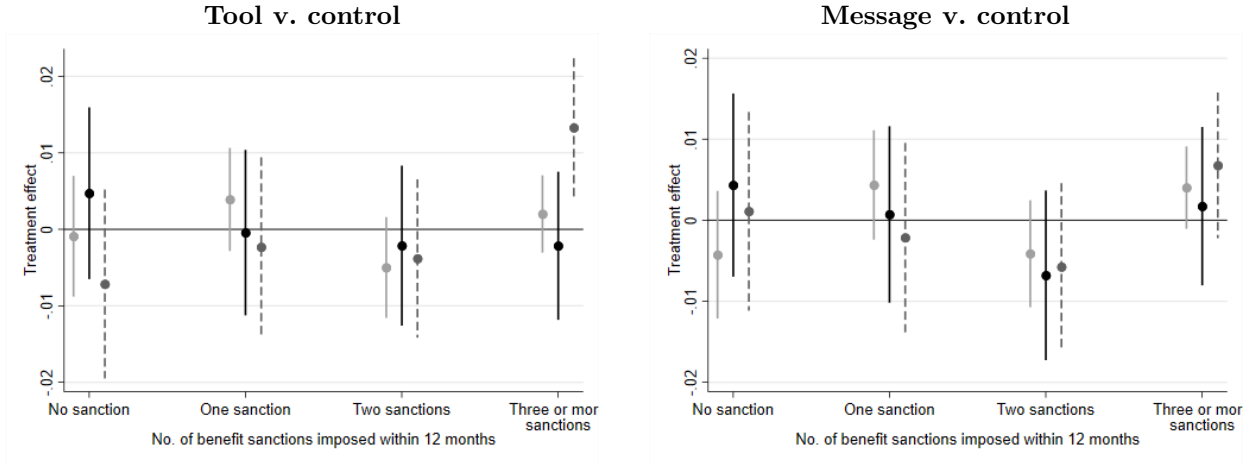
Note: Dependent variables refers to indicators referring to the total number of sanctions imposed. Depicted are effects of the tool and message treatment relative to the control group and the corresponding 90% confidence interval.

Moreover, the work requirement creates binary incentives to work just enough hours to avoid the benefit sanction. Having access to the online information tool allows job seekers to follow their exact number of collected working hours and therefore might give rise to gaming effects. In Table 8, we investigate treatment effects on the degree of bunching around the threshold of the work requirement of 148 hours. In particular, the outcome in model (1) and (2) refers to a dummy variable taking the value one if the benefit recipient worked between 145 and 150 hours within the first full qualification period (four-month window) after the intervention. As shown in column (2) there is some evidence that the tool treatment fosters bunching around the threshold for short-term benefit recipients. The likelihood that the job seekers collect between 145 and 150 working hours increases by 0.2 percentage points. This is equivalent to a relative effect of about 29% and it is statistically significant at the 5%-level. Given that short-term benefit recipients have little incentives to organize their job search and work strategy to extend the PBD, they

Table 8: Treatment effects on likelihood of bunching

Dependent variable	Working hours in first qualification period					
	Overall bunching 145-150 hours		Above threshold 148-150 hours		Below threshold 145-148 hours	
	(1)	(2)	(3)	(4)	(5)	(6)
Tool treatment	0.0009 (0.0006)		0.0005 (0.0004)		0.0004 (0.0005)	
Tool × UI benefit duration ≤26 weeks		0.0020** (0.0009)		0.0009* (0.0005)		0.0010 (0.0007)
Tool × UI benefit duration 27-52 weeks		-0.0017 (0.0013)		-0.0003 (0.0008)		-0.0014 (0.0011)
Tool × UI benefit duration >52 weeks		0.0011 (0.0014)		0.0004 (0.0008)		0.0007 (0.0011)
Message treatment	0.0007 (0.0006)		0.0003 (0.0004)		0.0004 (0.0005)	
Message × UI benefit duration ≤26 weeks		0.0008 (0.0009)		0.0002 (0.0005)		0.0006 (0.0007)
Message × UI benefit duration 27-52 weeks		-0.0003 (0.0013)		-0.0001 (0.0008)		-0.0003 (0.0011)
Message × UI benefit duration >52 weeks		0.0015 (0.0014)		0.0009 (0.0008)		0.0006 (0.0011)
No. of observations	98,641	98,641	98,641	98,641	98,641	98,641
P-value tool v. message						
full sample	0.736		0.543		0.989	
UI benefit duration ≤26 weeks		0.188		0.152		0.546
UI benefit duration 27-52 weeks		0.317		0.797		0.299
UI benefit duration >52 weeks		0.787		0.525		0.909
Mean value control group						
full sample	0.0062		0.0019		0.0043	
UI benefit duration ≤26 weeks		0.0068		0.0020		0.0048
UI benefit duration 27-52 weeks		0.0066		0.0020		0.0047
UI benefit duration >52 weeks		0.0044		0.0017		0.0028
Control variables	Yes	Yes	Yes	Yes	Yes	Yes

Note: Depicted are intention-to-treat effects of the tool and message treatment relative to the control group for all experimental participants on the degree of bunching around the threshold to avoid a benefit sanction (qualification day) of 148 working hours within a four-month period. Standard errors in parentheses. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

might pay more attention to the work requirement and potential sanctions than long-term benefit recipients. Therefore, it is not surprising that we detect a statistically significant treatment effect on the degree of bunching only among job seekers who are treated early in their unemployment spell. Finally, the results in column (3) to (6) reveal that the degree of bunching is similarly affected above and below the threshold of 148 hours (although only the effect on the likelihood of working 148-150 hours is statistically significant at the 10%-level).

5.4   Job Search Behavior

To investigate the underlying mechanisms further, we now examine how the intervention affects individual job search behavior. We rely on two data sources. First, we utilize data on job applications registered in the official online portal of the Danish PES (*joblog*) within the first four weeks after the intervention. Although, benefit recipients are only required to register applications up to a certain threshold in order to fulfill the legal requirement (typically two applications per week), previous evidence by Fluchtmann et al. (2019) shows that joblog covers a large fraction of individual applications made over the unemployment spell. Second, the online survey also includes a variety of questions on individual job search behavior.[17] While information on registered job applications is available for the vast majority of unemployed workers and provides some information on the characteristics of the job (e.g. an indicator for part-time employment and potential commuting time), the online survey provides additional information regarding individual preferences for specific types of employment, e.g., temporary and part-time jobs.

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[17]About 80% of respondents were still unemployed at the time of the main survey, while about 20% of respondents had returned to employment. Unemployed respondents are asked about their search activities during the previous month, while employed respondents are asked about their search effort during the last 4 weeks of their latest unemployment period.

Table 9: Treatment effects on registered job applications within four weeks

	Any registered application		Total no. of registered applications[a]		No. of registered part-time applications[a]		Avg. daily travel time in minutes[b]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tool treatment	-0.00220		0.0500		0.00792		-1.981**	
	(0.00281)		(0.0308)		(0.0146)		(0.866)	
Tool × UI benefit duration ≤26 weeks		-0.00347		0.119***		0.0329		-2.509**
		(0.00381)		(0.0423)		(0.0201)		(1.190)
Tool × UI benefit duration 27-52 weeks		0.00352		-0.0705		-0.0403		-2.026
		(0.00579)		(0.0626)		(0.0297)		(1.761)
Tool × UI benefit duration >52 weeks		-0.00554		0.0107		-0.000844		-0.737
		(0.00597)		(0.0645)		(0.0306)		(1.814)
Message treatment	-0.00172		0.0265		0.00799		-0.528	
	(0.00281)		(0.0308)		(0.0146)		(0.866)	
Message × UI benefit duration ≤26 weeks		-0.00251		0.0657		0.0200		-0.519
		(0.00380)		(0.0422)		(0.0200)		(1.189)
Message × UI benefit duration 27-52 weeks		0.00280		-0.0975		-0.0293		-1.847
		(0.00583)		(0.0629)		(0.0299)		(1.772)
Message × UI benefit duration >52 weeks		-0.00440		0.0631		0.0179		0.835
		(0.00595)		(0.0642)		(0.0305)		(1.805)
No. of observations	98641	98641	82279	82279	82279	82279	79877	79877
P-value tool v. message								
full sample	0.864		0.445		0.996		0.0939	
UI benefit duration ≤26 weeks		0.802		0.212		0.524		0.0972
UI benefit duration 27-52 weeks		0.901		0.664		0.709		0.918
UI benefit duration >52 weeks		0.848		0.415		0.540		0.384
Mean value control group								
full sample	0.836		7.597		1.005		168.2	
UI benefit duration ≤26 weeks		0.815		7.365		0.927		169.2
UI benefit duration 27-52 weeks		0.858		7.808		1.051		169.6
UI benefit duration >52 weeks		0.864		7.927		1.143		164.4
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Depicted are intention-to-treat effects of the tool and message treatment relative to the control group on job applications registered in the common online portal of the Danish PES (*joblog*) within four weeks after the start of the intervention. Standard errors in parentheses. ***/**/* indicate statistically significance at the 1%/5%/10%-level.
[a] Considering individuals who register at least one application within four weeks after the start of the intervention.
[b] Considering individuals who register at least one application with non-missing information on the potential travel time within four weeks after the start of the intervention.

Table 10: Treatment effects on job search behavior elicited through online survey

Dependent variable	Total no. of applications in last month		No. of appl. to part-time jobs		No. of appl. to temporary jobs		Looking for part-time employment		Looking for temporary employment		Attractiveness working one week (1=low;10=high)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Tool treatment	-0.236 (0.275)		-0.202 (0.133)		-0.0247 (0.138)		-0.00740 (0.0279)		0.0520** (0.0263)		0.498** (0.217)	
Tool × UI benefit duration ≤26 weeks		-0.0368 (0.389)		-0.136 (0.189)		0.0559 (0.196)		0.0173 (0.0394)		0.0765** (0.0371)		0.936*** (0.315)
Tool × UI benefit duration 27-52 weeks		-0.436 (0.529)		-0.204 (0.258)		0.0175 (0.267)		-0.0663 (0.0538)		-0.0107 (0.0506)		-0.262 (0.399)
Tool × UI benefit duration >52 weeks		-0.437 (0.576)		-0.357 (0.279)		-0.233 (0.289)		0.00582 (0.0582)		0.0755 (0.0548)		0.557 (0.443)
Message treatment	0.000785 (0.274)		-0.0662 (0.130)		-0.114 (0.135)		-0.0186 (0.0271)		0.0195 (0.0256)		0.0320 (0.209)	
Message × UI benefit duration ≤26 weeks		-0.148 (0.402)		-0.0757 (0.195)		-0.102 (0.203)		-0.00777 (0.0408)		0.0274 (0.0384)		0.394 (0.325)
Message × UI benefit duration 27-52 weeks		-0.0810 (0.512)		-0.0960 (0.236)		-0.0951 (0.245)		-0.0395 (0.0492)		-0.0305 (0.0464)		-0.420 (0.364)
Message × UI benefit duration >52 weeks		0.336 (0.554)		-0.0257 (0.260)		-0.165 (0.269)		-0.0193 (0.0542)		0.0601 (0.0511)		-0.0286 (0.413)
No. of observations	2,956	2,956	1,799	1,799	1,799	1,799	1,799	1,799	1,799	1,799	1,259	1,259
P-value tool v. message												
full sample	0.396		0.309		0.518		0.689		0.218		0.0291	
UI benefit duration ≤26 weeks		0.785		0.760		0.445		0.546		0.209		0.102
UI benefit duration 27-52 weeks		0.508		0.670		0.667		0.612		0.690		0.683
UI benefit duration >52 weeks		0.165		0.212		0.806		0.650		0.767		0.155
Mean value control group												
full sample	10.26		1.938		2.119		0.536		0.674		5.771	
UI benefit duration ≤26 weeks		10.25		1.904		1.968		0.465		0.608		5.225
UI benefit duration 27-52 weeks		9.877		1.648		2.045		0.575		0.754		6.206
UI benefit duration >52 weeks		10.75		2.391		2.558		0.645		0.717		6.337
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Depicted are intention-to-treat effects of the tool and message treatment relative to the control group for participants in the main survey (who answer the corresponding survey item) on job search characteristics. Standard errors in parentheses. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

Again, we separate the three subgroups based on their elapsed benefit duration at the start of intervention. The results are summarized in Tables 9 and 10. First, it should be noted that, despite working less hours and having lower earnings subsequent to the intervention (see Table 6), long-term benefit recipients generally send out more job applications.[18] This is in line with the notion that longer unemployment periods are connected to human capital depreciation (Pissarides, 1992; Pollak, 2013) and tend to stigmatize individuals (Vishwanath, 1989; Biewen and Steffes, 2010) leading to fewer call-backs despite increased or unchanged search effort.

Moreover, the registered job applications and survey responses provide indicative evidence that job seekers, especially those who are treated early in their unemployment spell, adjust their job search behavior in response to the tool treatment. As shown in Table 9, the tool treatment increases the total number of registered job applications among the short-term benefit recipients by about 1.6% ($p-$value<0.01), while there is no effect on the number of registered applications for part-time jobs. Interestingly, the higher number of job applications among treated short-term benefit recipients is associated with a reduction in geographical search radius. The average potential daily commuting time decreases by 1.5% relative to the control group ($p-$value<0.05). When considering the effects on the survey outcomes in Table 10, it should be noted that we are not able to detect any effects on the self-reported application behavior, probably due to the limited number of observations in our online survey. However, as shown in columns (9) to (12) of Table 10 there is some evidence that treatment increases the willingness of short-term benefit recipients to work in temporary jobs. This is in line with the notion that the tool treatment is informative on job seekers' incentives to search for and accept marginal employment.

It should be noted that overall search effort and willingness to accept jobs of lower quality is already increasing as jobseekers approach benefit exhaustion (see, e.g., Van den Berg, 1990; Marinescu and Skandalis, 2019, or mean values of the control group presented in Table 9 and Table 10), independently of our tool treatment. Hence, there might be a greater scope for the tool treatment to affect the search behavior of short-term benefit recipients compared to those who are treated later in their unemployment spell, which

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[18]This true for registered as well as self-reported job applications.

could explain the stronger effects on search behavior among short-term benefit recipients. However, since long-term benefit recipients have greater difficulties to find regular employment (despite their higher search effort), accepting a temporary or part-time job becomes generally more attractive over the course of the unemployment spell. Therefore, emphasizing the associated incentives through the information treatment might have a stronger impact on the realized labor market outcomes of long-term benefit recipients.

5.5 Alternative Channels

Our results show that the usage of the online tool had the desired effect of increasing people's knowledge. Additionally, we want to test whether the intervention had other behavioral effects that could affect individual labor market outcomes. For instance, since our intervention involves communication with treated individuals (treatment messages) and the provision of personalized information, it may also directly influence their overall motivation. To identify potential differences in motivation and stress, the survey includes a psychological measure that captures positive and negative affect (*PANAS*; Watson et al., 1988). Documentation that this measure is related to individual job search behavior is found in Burger and Caldwell (2000). The scale consists of two 10-item scales that describe different feelings and emotions related to positive, respectively negative affect. Each item is rated on a 5-point Likert scale (Q40).

Finally, utilizing the online information tool could increase the job seekers' awareness that their job search activities are being monitored by the authorities, which could induce pressure to search more intensively (see e.g. Gorter and Kalb, 1996; McVicar, 2008; Cockx and Dejemeppe, 2012; Van den Berg and Van Der Klaauw, 2019). Therefore, the survey includes questions asking about perceived monitoring intensity with respect to job search activities (Q11) and the subjectively perceived external pressure to search for (Q36) and accept (Q37) available job offers.

Table 11: Treatment effects on general motivation and perceived pressure

| Dependent variable | General motivation (PANAS)[a] | | | | Perceived pressure and monitoring[b] | | | | | |
| | Positive affect | | Negative affect | | Feels pressure to search for job | | Feels pressure to accept job | | Feels monitored by the authorities | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Tool treatment	0.00766 (0.0357)		0.0385 (0.0419)		0.0729 (0.123)		0.122 (0.145)		0.148 (0.131)	
Tool × UI benefit duration ≤26 weeks		-0.0633 (0.0504)		0.110* (0.0592)		0.233 (0.173)		0.228 (0.205)		0.172 (0.186)
Tool × UI benefit duration 27-52 weeks		0.0998 (0.0683)		-0.100 (0.0802)		-0.269 (0.235)		-0.288 (0.278)		0.199 (0.252)
Tool × UI benefit duration >52 weeks		0.0561 (0.0758)		0.0473 (0.0889)		0.129 (0.260)		0.392 (0.309)		0.0303 (0.279)
Message treatment	-0.0143 (0.0356)		-0.00421 (0.0417)		0.165 (0.122)		0.0370 (0.145)		0.0151 (0.131)	
Message × UI benefit duration ≤26 weeks		-0.0816 (0.0521)		0.0342 (0.0611)		0.231 (0.179)		0.0813 (0.212)		0.0931 (0.192)
Message × UI benefit duration 27-52 weeks		0.0501 (0.0665)		-0.0821 (0.0780)		0.0530 (0.228)		-0.302 (0.271)		-0.0531 (0.245)
Message × UI benefit duration >52 weeks		0.0435 (0.0721)		0.00907 (0.0846)		0.158 (0.248)		0.349 (0.293)		-0.0547 (0.265)
No. of observations	2,657	2,657	2,657	2,657	2,657	2,657	2,657	2,657	2,657	2,657
P-value tool v. message										
full sample	0.544		0.315		0.462		0.565		0.320	
UI benefit duration ≤26 weeks		0.729		0.219		0.994		0.493		0.683
UI benefit duration 27-52 weeks		0.468		0.823		0.171		0.960		0.317
UI benefit duration >52 weeks		0.863		0.657		0.908		0.884		0.753
Mean value control group										
full sample	3.029		2.539		7.943		6.240		7.455	
UI benefit duration ≤26 weeks		3.105		2.408		7.795		6.164		7.474
UI benefit duration 27-52 weeks		2.958		2.606		8.220		6.355		7.479
UI benefit duration >52 weeks		2.948		2.753		7.926		6.266		7.379
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Depicted are intention-to-treat effects of the tool and message treatment relative to the control group for participants in the main survey. Standard errors in parentheses. ***/**/* indicate statistically significance at the 1%/5%/10%-level.
[a] The PANAS consists of two 10-item scales, where each item is rated on 5-point scale (1 ≡ not at all) to (5 ≡ very much) and averages are reported separately for items related to positive, respectively negative affect.
[b] Measured on a 10-point scale (1 ≡ completely disagree; 10 ≡ completely agree).

As shown in Table 11, there is no evidence that the tool treatment affects individual behavior through these alternative channels. For the tool treatment, only one out of 20 estimated coefficients is statistically significant at the 10%-level. Given the relative large and significant treatment effects on the job seekers' knowledge, this indicates that the treatment effects on labor market outcomes can indeed be attributed to an improved understanding of the UI benefit rules and jobseekers' own personal situation.

6   Conclusion

This paper presents results from an online intervention providing information about complex UI benefit rules to unemployed workers in the Danish labor market. The intervention rests on an online information tool providing up-to-date, personalized information on individuals' remaining UI benefit period, and on their accumulated working time that can be used to prolong potential benefit period (PBD). Moreover, the tool provides general information on essential rules regarding job seekers' benefit period and benefit sanctions. Based on a randomized controlled trial among the population of Danish UI benefit recipients, we show that our intervention increases the knowledge of treated individuals regarding prevailing rules and their economic situation. The knowledge increase is stable across subgroups at different points of their benefit period. This shows that digital tools are suitable to transmit relevant knowledge about rules and regulations that have the potential to mitigate information frictions and improve the job search process of the unemployed.

However, the consequences for realized labor market outcomes crucially depend on the elapsed benefit duration of jobseekers, which most likely reflects the differential incentives at different stages of the benefit period. At the beginning of the unemployment spell, higher knowledge has two effects. On one hand, it creates the perception that the system is more generous since job seekers have the possibility to extend the PBD. On the other hand, it leads to greater uncertainty about the PBD and therefore also about future income if remaining unemployed. Hence, treated individuals might search harder for high-quality jobs that allow them to leave the UI system. However, as benefit expiration approaches, higher knowledge creates stronger short-run incentives to work in non-standard jobs in

order to actually extend the potential benefit duration. A second factor that contributes to the differential effects might be the dynamic selection out of unemployment over the course of time. Among long-term benefit recipients there is typically a larger fraction of low-ability individuals who are in general more likely to work in temporary and part-time jobs. The additional incentives make these jobs even more attractive, but this comes at the cost of reduced prospects of finding regular employment due to locking-in effects.

Our analysis shows that, since no additional employment is created, treated long-term benefit recipients end up working fewer hours and having lower earnings over the first year after intervention than the control group, and focusing on marginal employment increases their risk of receiving benefit sanctions. Our findings imply that an improved understanding of the built-in incentive structure of public policies can have adverse effects on job seekers' subsequent labor market outcomes when the underlying incentives are not suitable to promote sustainable employment. It should be noted that the overall labor market situation was very good during the experimental period, while the treatment might have been more effective during a downturn characterized by greater needs for marginal employment.

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A   Appendix

A.1   Text of Messages

Main message to treatment group:

Dear X,

Your unemployment benefits will expire at some point, but did you know that you can influence the duration of your unemployment benefit period yourself? Every hour you work translates into up to two extra hours of unemployment benefits, which you can use to extend your unemployment benefit period. At the same time, every hour you work helps you avoid a qualification day, at which you receive no unemployment benefits.

A new tool on *jobnet.dk* makes it easy for you to keep an eye on your accumulated working hours and get an overview of the most relevant benefit rules. The dynamic and personalized tool is called "Dagpengetæller" ["benefit meter"]. It is continuously updated with your unemployment benefit hours and your working hours; and you can calculate how extra working hours will affect your unemployment benefit period.

Your benefit meter gives you an overview of:

1. The hours you have worked

2. Your consumption of unemployment benefits and your remaining benefit hours

3. Rules that are important for you. Check the information boxes by clicking on the "i"-button

Learn more about your unemployment benefits now. [LINK]

Use your benefit meter regularly to know your possibilities and make the most out of them. You may, for instance, check your benefit meter when you log on to jobnet.dk to check your suggested job ads or register your job applications.

Did you know that there are about 20,000 vacancies available at jobnet.dk right now? There are more possibilities than you may think.

Good luck with your job search.

Reminder message to treatment group:

Dear X,

Your unemployment benefits will expire at some point in time, but did you know that you can influence the duration of your unemployment benefit period yourself?

A new tool on *jobnet.dk* makes it easy for you to keep an eye on your accumulated working hours and get an overview of the most relevant benefit rules.

Learn more about your unemployment benefits now. [LINK]

Use your benefit meter regularly to know your possibilities and make the most out of them.

Did you know that there are about 20,000 vacancies available at jobnet.dk right now? There are more possibilities than you may think.

Good luck with your job search.

Message to message group:

Dear X,

Use *jobnet.dk* regularly to know your possibilities and make the most out of them.

Did you know that there are about 20,000 vacancies available at jobnet.dk right now? There are more possibilities than you may think.

Good luck with your job search.

A.2   List of Survey Questions

(Q01) When did you register as unemployed either at jobnet.dk or at your unemployment fund?

  – Day
  – Month
  – Year

(Q02) What type of job are you looking for? *You may mark multiple answers.*

  – Full-time (37 or more working hours per week)
  – Part-time (less than 37 working hours per week)

(Q03) What type of job are you looking for? *You may mark multiple answers.*

  – Permanent job
  – Temporary job

(Q04) How many jobs did you apply for during the last month? *Please state the exact total number of jobs.*

(Q05A) How many of your applications were for full-time and part-time jobs? *Please state the exact number of full-time and part-time jobs.*

  – Full-time (37 or more working hours per week)
  – Part-time (less than 37 working hours per week)

(Q05B) How many of your applications were for permanent and temporary jobs? *Please state the exact number of permanent and temporary jobs.*

  – Permanent jobs
  – Temporary jobs

(Q06) How often did you apply for jobs using the online platform jobnet.dk during the last month?

(Q07) How many hours did you spend on job search activities during the last week?

(Q08) Did you receive any invitations to job interviews during the last month?

  – Yes
  – No

(Q09) If yes: How many invitations to job interviews did you receive during the last month?

(Q10) When do you expect to deregister as unemployed at jobnet.dk or your unemployment fund? *Please indicate the point in time in which you expect to deregister.*

  – Within 1 month
  – Within 2 months
  – Within 3 months
  – Within 4 months
  – Within 5 months

- Within 6 months
- Within 7 months
- Within 8 months
- Within 9 months
- Within 10 months
- Within 12 months
- Within 13 months
- Within 15 months
- Within 16 months
- Within 18 months
- Within 22 months
- Within 24 months
- In more than 24 months

(Q10) How sure are you about your answer above? *Please enter your assessment on a scale from 1 to 10, where 1="Not sure at all" and 10= "Completely sure".*

(Q11) Do you agree with the following statement? My job search activities are regularly monitored by the authorities. *Please enter your assessment on a scale from 1 to 10, where 1="Do not agree at all" and 10="Completely agree".*

(Q12) Overall, how easy/difficult is it for you to find a new job? *Please enter your answer on a scale from 1 to 10, where 1="Very easy" and 10="Very difficult".*

(Q13) Which of the following statements describes your usage of joblog best?

- I register a sufficient number of jobs to comply with the search requirements, but I have often searched for additional jobs that I do not register.
- I register a sufficient number of jobs to comply with the search requirements and I have rarely searched for further jobs.
- I always register all jobs I apply for regardless of the search requirements.
- Usually, I do not register my search activities in joblog.

(Q14) How many jobs did you apply for in the last month without registering them in joblog? *Please state the number of jobs.*

(Q15) How often should you check your suggested job ads on jobnet.dk?

- Every day
- Every week
- Every month
- Every second month

(Q16) Suppose you have forgotten to check your suggested job ads on time. What would be the consequence?

- You will receive a reminder
- You will be unsubscribed and no longer receive unemployment benefits

– Nothing

(Q17) What was your income in your last job? *Please indicate your monthly salary before tax.*

(Q18) What was your level of unemployment benefits (paid by your unemployment fund) in the last month? *Please indicate your monthly benefits before tax.*

(Q19) Did you work during the last month?

– Yes
– No

(Q20) How many hours did you work during the last month? *Please indicate the number of hours.*

(Q21) How much did you earn for these working hours? *Please indicate the total amount before tax.*

(Q22) Are you in job training or do you work in a subsidized job?

– Yes
– No

(Q23) Do you receive supplementary unemployment benefits?

– Yes
– No

(Q24) When will your unemployment benefits expire? Enter the date your unemployment benefit period ends if you include current extensions. Assume that you do not take any further work.

– Day
– Month
– Year

(Q25) How sure are you about your answer in the previous question? *Please enter your assessment on a scale from 1 to 10, where 1="Not sure at all" and 10="Completely sure".*

(Q26) Suppose you will work for two full weeks while being on unemployment benefits. How will this affect your situation at the end of the two-year unemployment benefit period? Can you use the two weeks to extend your benefit period?

– Yes
– No

(Q27) For how long can you extend your unemployment benefit period if you have been working for two weeks? Please indicate the number of weeks.

(Q28) Suppose that you have an offer of working for one week (equivalent to 37 hours). The salary before tax is 5.500 kr and you receive unemployment benefits for the rest of the month. How will it affect your total monthly income (working salary and unemployment benefits) in comparison to a month where you receive unemployment benefits only, if you accept the job?

- My income decreases
- My income is the same
- My income increases

(Q29) How attractive is such a job to you? *Please enter your assessment on a scale from 1 to 10, where 1="Not attractive at all" and 10="Very attractive".*

(Q30) Suppose you have received unemployment benefits for a period of 4 months and you are not working during the period, how will it affect your unemployment benefit in the fourth month compared to the first 3 months of the period? My benefits in the 4th month are:

- Lower
- Unchanged
- Higher

How large do you expect that the benefit reduction in the fourth month will be? *Give it your best shot.*

- Amount before tax:

(Q31) The unemployment benefit period is two years with the possibility of an extension. How many hours do you have to work to extend the benefit period by 12 weeks? *(This could be by working in a small job during the benefit period.)*

- 111 hours (3 weeks)
- 222 hours (6 weeks)
- 444 hours (12 weeks)
- 666 hours (18 weeks)
- 888 hours (24 weeks)

(Q32) In general, by how much can the two-year unemployment benefit period be extended by working while you receive unemployment benefits?

- 481 hours (3 months)
- 962 hours (6 months)
- 1443 hours (9 months)
- 1924 hours (12 months)
- 2405 hours (15 months)
- 2886 hours (18 months)

With the following questions, we are interested in your perception of the job search process. You will be confronted with several statements. Please indicate on a scale from 1 to 10 whether your agree with the statement, where 1="completely disagree" and 10="completely agree".

(Q33) I generally feel well informed about the rules, rights, and regulations that are relevant for me.

(Q34) It is easy for me to find the information that I need.

(Q35) The rules are hard to understand.

(Q36) I constantly feel pressured to search for a job.

(Q37) I constantly feel pressured to accept job offers.

(Q38) Where do you find information regarding your own unemployment benefit situation? *Check the most important answer.*

- Job center or unemployment fund
- Jobnet.dk
- Other online sources, incl. social media
- Newspaper or TV
- Family and friends

(Q39) Where do you find information about the rules regarding the job search process? *Check the most important answer.*

- Job center or unemployment fund
- Jobnet.dk
- Other online sources, incl. social media
- Newspaper or TV
- Family and friends

(Q40) The following question is about your feelings in the last four weeks. Below you can see a list of words describing different emotions and conditions:

1. Attentive	6. Upset	11. Excited	16. Scared
2. Strong	7. Irritable	12. Hostile	17. Enthusiastic
3. Inspired	8. Active	13. Proud	18. Distressed
4. Afraid	9. Guilty	14. Unhappy	19. Determined
5. Alert	10. Nervous	15. Ashamed	20. Interested

Please mark the answer that describes in the best way your feelings in the last four weeks. *Mark one answer for each feeling.*

- Not at all
- Slightly
- Somewhat
- Pretty much
- To a great extent

A.3 Elements of the Online Tool

Figure A.1 shows the different elements of the online tool that visualizes the UI benefit rules. (a) displays the possible extension of the PBD based on the accumulated working hours. (b) shows the consumption of benefit hours within the current benefit period. (c) shows the working hours saved for gaining a new 2-year benefit period. (d) shows how many working hours have been saved within the current 4-months window to reach the goal of 148 hours and avoid a benefit sanction.

Figure A.1: The Online Information Tool

(a) Extension of PBD

(b) Usage of UI benefits

(c) Renewing two-year PBD

(d) Avoidance of benefit sanction

Note:

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A.4 Additional Tables and Figures

Figure A.2: Effects of message treatment on labor market outcomes over time

A. Monthly working hours
UI benefit duration ≤26 weeks

B. Monthly labor earnings
UI benefit duration ≤26 weeks

UI benefit duration 27-52 weeks

UI benefit duration 27-52 weeks

UI benefit duration >52 weeks

UI benefit duration >52 weeks
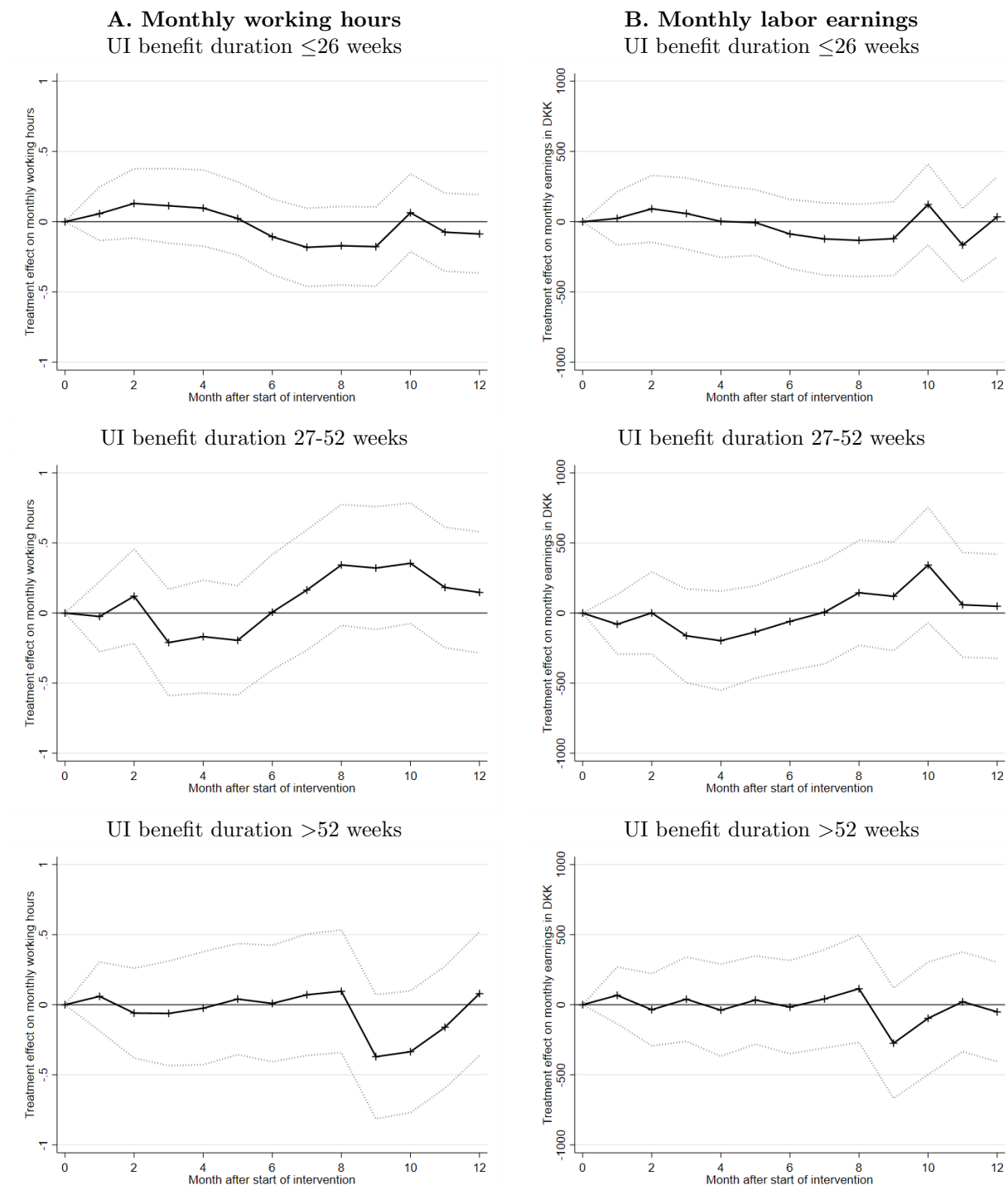
Note: Depicted are intention-to-treat effects of the message treatment relative to the control group for all experimental participants including 90% confidence intervals. In all models, we control for socio-demographic characteristics, labor market histories and place of residence (98 municipalities) and membership of unemployment funds (24 in total). ●/▲/◆ indicate statistically significance at the 1%/5%/10%-level.

Figure A.3: Effects of message treatment on cumulated labor market outcomes over time
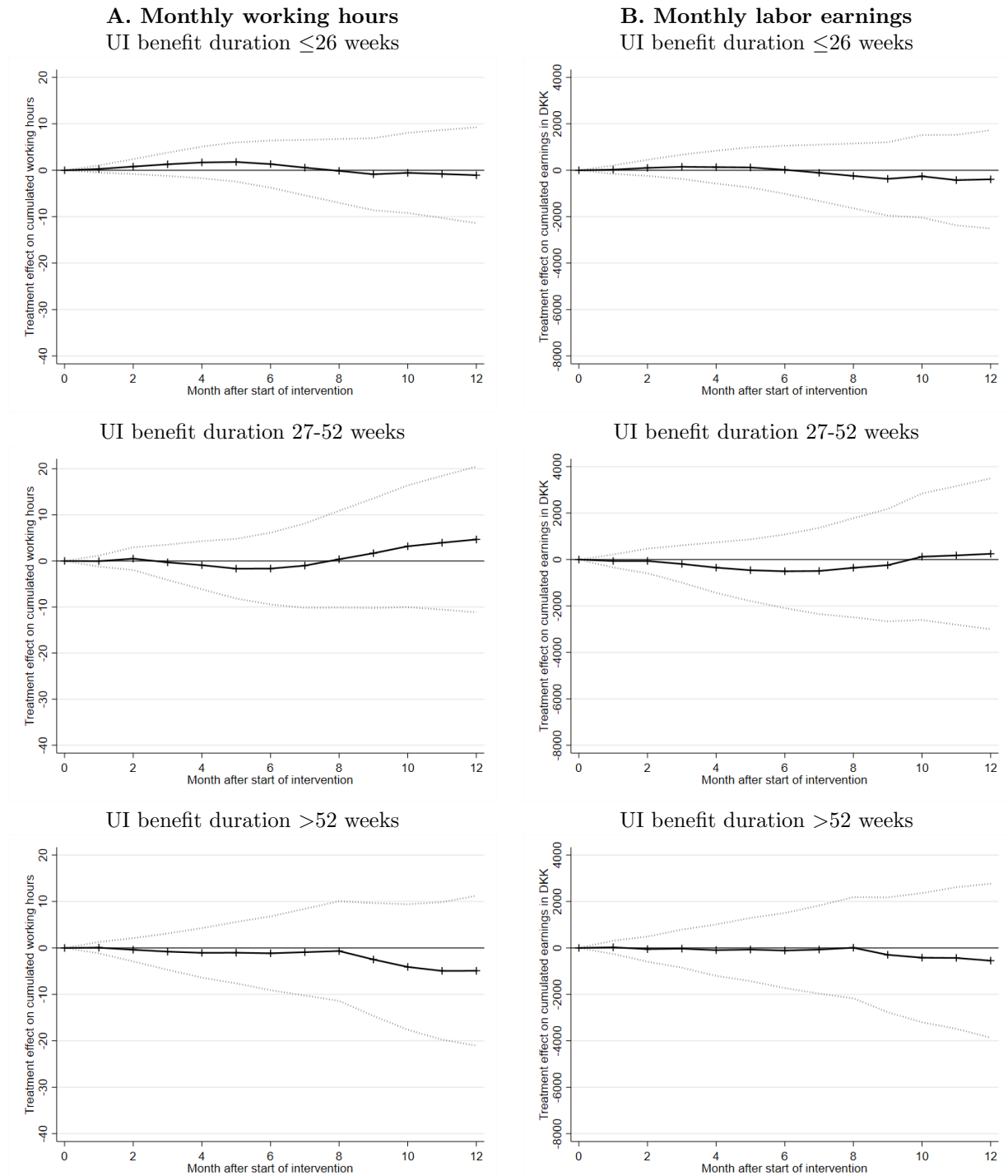
A. Monthly working hours
UI benefit duration ≤26 weeks

B. Monthly labor earnings
UI benefit duration ≤26 weeks

UI benefit duration 27-52 weeks

UI benefit duration 27-52 weeks

UI benefit duration >52 weeks

UI benefit duration >52 weeks

Note: Depicted are intention-to-treat effects of the tool treatment relative to the control group including 90% confidence intervals. In all models, we control for socio-demographic characteristics, labor market histories and place of residence (98 municipalities) and membership of unemployment funds (24 in total). ●/▲/◆ indicate statistically significance at the 1%/5%/10%-level. Effects of the message treatment are depicted in Figure A.2 in Appendix A.4.

Table A.1: Survey take-up by treatment status

	Total number of observations Treatment status			Share of observations Treatment status		
	Control	Message	Tool	Control	Message	Tool
No. of observations	32,905	32,876	32,860			
Pre survey						
Invited	2,481	2,467	2,482	0.075	0.075	0.076
Started	507	494	494	0.204	0.200	0.199
Completed	423	406	404	0.170	0.165	0.163
Main survey						
Invited	7,442 (1,592)	7,459(1,560)	7,451(1,615)	0.226	0.227	0.227
Started	1,145	1,075	1,053	0.154	0.144	0.141
Completed	947(163)	894(116)	879(168)	0.127	0.120	0.118
Total						
Invited	9,923	9,926	9,933	0.302	0.302	0.302
Started	1,652	1,569	1,547	0.166	0.158	0.156
Completed	1,370	1,300	1,283	0.138	0.131	0.129

Note: Numbers in parentheses are individuals, who are in employment at the time of the survey wave.

Table A.2: Descriptive Statistics for Participants in Main Survey

	Treatment status			$P-$values		
	Control (C)	Message (M)	Treatment (T)	M v. C	T v. C	T v. M
No. of observations	986	918	901			
Educational level						
Missing	0.026	0.029	0.034	0.687	0.309	0.545
Less than high school	0.093	0.105	0.101	0.410	0.573	0.802
High school	0.345	0.365	0.349	0.360	0.867	0.465
Bachelor degree (or equiv.)	0.355	0.329	0.330	0.233	0.247	0.976
Master degree (or equiv.)	0.181	0.172	0.186	0.631	0.740	0.425
Male	0.438	0.408	0.457	0.191	0.404	0.036
Age						
18-25 years	0.040	0.037	0.044	0.775	0.600	0.427
26-35 years	0.234	0.227	0.230	0.690	0.816	0.872
36-45 years	0.174	0.174	0.176	0.993	0.908	0.903
46-55 years	0.259	0.281	0.279	0.271	0.329	0.907
56-65 years	0.293	0.281	0.271	0.561	0.283	0.626
Household size						
One person	0.220	0.220	0.246	0.998	0.177	0.184
Two persons	0.398	0.401	0.396	0.883	0.953	0.840
Three persons	0.205	0.178	0.171	0.131	0.060	0.709
Four or more persons	0.177	0.202	0.186	0.181	0.614	0.417
Married	0.421	0.412	0.401	0.687	0.373	0.630
Children						
One child	0.166	0.141	0.130	0.119	0.026	0.506
Two or more children	0.123	0.142	0.132	0.224	0.543	0.554
Migration status						
1st generation	0.095	0.100	0.102	0.720	0.622	0.894
2nd generation	0.013	0.011	0.007	0.648	0.156	0.334
No. of joblogs in last 4 weeks	7.416	7.792	7.698	0.062	0.156	0.660
Weeks of UI benefits	33.227	36.960	33.615	0.004	0.767	0.014
Months employed in year						
t-1	5.948	5.386	5.710	0.004	0.231	0.103
t-2	8.360	7.998	8.172	0.074	0.356	0.404
t-3	8.787	8.742	8.635	0.821	0.445	0.595
t-4	8.550	8.669	8.430	0.564	0.566	0.259
t-5	8.427	8.416	8.186	0.960	0.271	0.305
Average monthly earnings in year						
t-1	23,016	21,284	23,003	0.112	0.991	0.127
t-2	25,380	24,099	25,330	0.134	0.955	0.172
t-3	24,562	23,744	24,624	0.336	0.942	0.293
t-4	24,177	22,928	22,893	0.140	0.138	0.966
t-5	22,712	21,701	21,541	0.246	0.171	0.850
Average employment degree in year						
t-1	0.559	0.529	0.554	0.091	0.781	0.174
t-2	0.707	0.687	0.700	0.236	0.698	0.443
t-3	0.707	0.708	0.719	0.978	0.479	0.508
t-4	0.694	0.700	0.681	0.736	0.442	0.279
t-5	0.662	0.663	0.649	0.983	0.487	0.486
No. of employers in year						
t-1	1.156	1.154	1.127	0.951	0.477	0.534
t-2	1.235	1.237	1.264	0.955	0.467	0.510
t-3	1.254	1.246	1.274	0.846	0.594	0.474
t-4	1.190	1.209	1.216	0.593	0.470	0.847
t-5	1.141	1.150	1.148	0.796	0.854	0.943
Weeks of UI benefits						
in last year	26.121	27.908	25.374	0.012	0.302	0.001
in last 2 years	35.195	38.815	35.078	0.001	0.918	0.001
in last 5 years	53.282	56.353	54.921	0.084	0.371	0.445
Weeks of supplementary benefits		180				
in last year	2.288	2.295	1.865	0.976	0.057	0.063
in last 2 years	3.086	3.339	2.707	0.424	0.189	0.046
in last 5 years	4.856	5.355	4.718	0.311	0.759	0.203

Note: Percentage share unless indicated otherwise. $P-$values measured based on two-tailed t-tests on equal means.

Chapter 3

Punishing the Poor? Work Requirements and Benefit Sanctions for Welfare Recipients

Punishing the Poor?
Work Requirements and Benefit Sanctions for Welfare Recipients

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September 30, 2020

Abstract

Monitoring and benefit sanctions are one of the major tools to enforce compliance with job search and work requirements among unemployed workers. We present results from a large-scale field experiment. Based on a 2×3 between-subjects design, we identify the causal effects of warnings and information about enforced sanctions on subsequent labor market outcomes of welfare recipients. We disentangle the effect of providing (i) general information about existing work requirements and (ii) personalized information regarding the individual's own situation. Although warnings generally reduce the likelihood of being sanctioned, the provision of general information reduces subsequent earnings and working hours. Providing access to personalized information counteracts the negative effect of the warning and stimulates exits from welfare. The latter is partly explained by a greater usage of other types of income support not subject to the sanction regime. Information about already enforced sanctions improve the labor market performance of welfare recipients without a partner.

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1 Introduction

Sanctions and monitoring schemes are commonly used to enforce rule compliance in various situations. A key example are social security systems, such as unemployment insurance (UI) or social assistance[1], where social planners are typically confronted with a trade-off: They aim to guarantee a minimum income, which allows unemployed workers to smooth consumption and to improve subsequent job match quality, without distorting the incentives to search for new employment (see e.g. Hopenhayn and Nicolini, 1997; Acemoglu and Shimer, 2000; Chetty, 2008). This leads many developed countries to combine the provision of a minimum income with job search or work requirements, while non-compliance leads to a reduction of the benefit level. As shown by Boone *et al.* (2007), a system with sanctions and monitoring can lead to welfare improvements relative to a system with a lower overall benefit level.[2]

In this paper, we investigate how the threat of being sanctioned, respectively the enforcement of benefit sanctions, affect the labor market performance of welfare recipients. We report the results of a large-scale field experiment conducted among the universe of welfare recipients in the Danish labor market. Individuals who receive welfare benefits are required to work at least 225 hours within any 12-month window, while non-compliance with the requirement leads to a permanent reduction of the monthly benefit level. We exogenously vary the degree of information that welfare recipients receive about the requirement and potential benefit sanctions within a 2×3 between-subjects design. In a first treatment arm, individuals receive monthly messages with *general information* about the underlying rules emphasizing the threat of a sanction. For individuals who are *at-risk* to incur a reduction of the benefit level, this represents a warning and allows us to examine how the presence of such a policy affects their subsequent labor market outcomes. Moreover, the permanent benefit sanction is only lifted once the individual manages to fulfill the work requirement. Hence, a substantial share of the experimental population

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[1]Sanctions and monitoring schemes are also used in variety of other policy domains, e.g. to prevent shirking of workers in firms (Nagin *et al.*, 2002), to encourage firms to comply with labor market regulations (Cobb-Clark *et al.*, 1995) or to ensure the provision of public goods (Fehr and Gächter, 2000).

[2]Imposing job search requirements can lead to further welfare improvements, when job seekers have hyperbolic time preferences, by limiting harmful procrastination (DellaVigna and Paserman, 2005; Paserman, 2008; Cockx *et al.*, 2014).

already receive benefits at a reduced level at the beginning of the intervention. For those who are *already sanctioned*, the information treatment allows us to analyze the effect of being exposed to sanctions (see Arni *et al.*, 2017, for a further discussion of the differential effects) if they are not perfectly informed about their benefit status.

We also analyze the role of *personalized information* about the individual's own situation with regard to the work requirement. Therefore, in addition to the message, individuals in a second treatment arm gain access to an online tool that provides personalized continuously updated information regarding the number of working hours that are missing to comply with the work requirement and the specific deadline for rule compliance, i.e. the potential benefit reduction date. Welfare benefit recipients in Denmark have no direct access to information about the number of working hours they have already collected. Hence, the tool treatment allows us to investigate how information frictions about one's own situation influence the effectiveness of warnings and benefit sanctions.

Our experimental approach provides important new insights and overcomes various limitations of the existing literature. First, it allows us to provide a clean identification of causal effects regarding various aspects of work requirements and benefit sanctions that are crucial to assess the overall effectiveness of such a policy.[3]

Previous evidence indicates that warnings as well as the imposition of benefit sanctions increase transition rates to employment (Abbring *et al.*, 2005; Van den Berg *et al.*, 2004; Lalive *et al.*, 2005; Svarer, 2011), but lead to lower wages and reduced job stability (Arni *et al.*, 2013; Van den Berg and Vikström, 2014).[4] Due to the absence of experimental variation, existing studies, however, rely on strong identification assumptions whose violation could bias the estimation results.[5] Moreover, our study is the first that consid-

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[3]The only related experimental evidence based on field data analyzes changes in search requirements and monitoring of job search in the US (see e.g. Johnson and Klepinger, 1994; Klepinger *et al.*, 2002), while there are no explicit benefit sanctions involved. In a laboratory experiment, Boone *et al.* (2009) show that the threat of getting a sanction has substantially bigger effect on outflow from unemployment than the exposure to a sanction.

[4]Moreover, warnings and benefit sanctions can be associated with various other unintended consequences such as an increased usage of sickness benefits (van den Berg *et al.*, 2019), mental health issues (Caliendo *et al.*, 2020) or increased crime (Machin and Marie, 2006).

[5]Identification based on the so-called timing-of-events approach (Abbring and Van den Berg, 2003) typically requires that individuals do not anticipate when a sanction, respectively a warning is imposed. This seems questionable given that both events are typically a direct consequence of the individual's non-compliance with the prevailing rules.

ers information frictions regarding the individual's own situation in the context of work requirements and benefit sanctions. Finally, in contrast to the vast majority of papers, we focus on welfare rather than UI benefit recipients (see Van den Berg *et al.*, 2004; Van der Klaauw and Van Ours, 2013, for two exceptions), who have very limited attachment to the labor market (Moffitt, 1992).

Our results show various interesting patterns. First, for those who are *at risk of being sanctioned*, we find evidence of a substantial threat effect. Receiving repeated warnings, including information regarding the general rules, reduces the likelihood of being sanctioned as, compared to the control group, the treated are more likely to leave the welfare system. However, this does not translate into a higher probability of finding paid employment, but leads to a greater usage of other types of income support, such as educational benefits, and a higher likelihood to retire. Thus, warnings stimulate exits from the welfare system, but reduce subsequent earnings and working hours. Second, we can show that combining warnings with personalized information about work requirements counteracts the negative effects of the pure warnings, and can even improve the labor market performance for those, who stand to incur a sanction. Considering treated individuals who make use of the online tool, the provision of personal information results in improved labor market outcomes, i.e. higher earnings and more working hours.

Third, when considering welfare recipients who are *already sanctioned* and receive benefits at a reduced level, the effectiveness of the information treatment crucially depends on the individual's marital status. This is not surprising given that the partner's income typically reduces the individual benefit level and the incentives to react to the information treatment. Our results show that both treatments (generalized and personalized information) increase earnings and working hours relative to the control group of welfare recipients without a partner living in the same household. However, personalized information is again more effective than pure warnings. We also find evidence that both information treatments generally seem to encourage individuals to accept different types of jobs. General information, rendering the sanction more salient, leads to acceptance of jobs with a higher hourly wage, whereas personalized information encourages individuals to start jobs with a higher number of working hours.

Our findings are in line with previous evidence for UI benefit recipients. Specifically, warnings are found to be an effective tool to stimulate exits from the UI system in Switzerland (Lalive *et al.*, 2005; Arni *et al.*, 2013), Sweden (Lombardi, 2019) and Germany (Boockmann *et al.*, 2014). However, we can show that this does not lead to higher employment rates, but welfare recipients switch to other types of income support that are not subject to the sanction regime. Together, the differential effects on exits from welfare, the occurrence of sanctions and employment prospects, indicate that the consumption smoothing aspect of welfare benefits is particularly important (Card *et al.*, 2007a,b; Chetty, 2008), whereas the threat of sanctions might be less effective in restoring search incentives. This argument finds further support in the fact that (general and personalized) warnings lead to a substantial reduction of the individual's total income, the sum of benefit payments and labor earnings.

Finally, our findings also highlight the relevance of information frictions regarding the work requirement. This supports the notion that the communication of policies matters, which has been shown in various domains such as educational choices (Hastings and Weinstein, 2008; Jensen, 2010; Bettinger *et al.*, 2012; Wiswall and Zafar, 2014), income support programs (Alatas *et al.*, 2016; Stephan *et al.*, 2016), food stamps (Finkelstein and Notowidigdo, 2019), tax credits (Bhargava and Manoli, 2015) or medical support (Kling *et al.*, 2012). Information frictions in the welfare system can be reduced at a low marginal cost per user by increasing access to and utilization of digital information tools. This is important as a misunderstanding of prevailing rules or economic incentives might distort job search (Altmann *et al.*, 2018) and labor supply decisions (Chetty and Saez, 2013; Liebman and Luttmer, 2015). Two aspects are essential for rendering online information effective: (i) information should be updated and personal rather than static and general (Fuentes *et al.*, 2017), and (ii) incentives to access and utilize the information must be in place.

2   Design of the Experiment

To analyze the effect of work requirements, warnings and benefit sanctions on employment prospects of individuals with limited attachment to the labor market, we conduct a

field experiment among welfare recipients in the Danish labor market. These individuals are confronted with a work requirement, while non-compliance is punished with a reduction of their benefit level. Our information intervention aims to increase the individuals' awareness of the general rules and their personal situation in relation to the rules. We combine the experimental data with detailed administrative records to measure the effect on labor market outcomes.

2.1 Welfare Benefits and Sanctions

Our experiment targets recipients of welfare benefits, which provide a safety net for those unemployed workers who are not entitled to any other social security benefits such as unemployment insurance. Welfare benefits are means-tested and the benefit level depends on the individual's age, the presence of children in the household, and the income of a potential spouse. A single person (older than 30 years) receives 11,554DKK ($\approx$1,680\$, 2020-level) per month, while the amount increases to 15,355DKK ($\approx$2,230\$, 2020-level) when children live in the household. Welfare recipients, who are younger than 30 years, receive about 65% (without children), respectively 96% (with children), of the baseline level. The benefit level of individuals with a working spouse is corrected to insure that the gross household income does not exceed two times the benefit level of the individual recipient.

There is no limit regarding the duration of welfare benefit receipt, but, since 2016, benefit recipients are confronted with specific work requirements. Specifically, individuals are entitled to the full amount of welfare benefits only if they have worked for at least 225 hours in a non-subsidized job within the previous 12 months (we refer to this as the '225-hours rule' in the following).

If the welfare recipient does not comply with the work requirement, a benefit sanction is imposed that reduces the individual's benefit level by approximately 500 to 1,000DKK per month depending on the individual's family status. The criteria must be fulfilled at each point in time (considering the preceding 12-month window). This implies that even if the individual complies with the rule in a given month, she might be subject to a reduction the following month. The first benefit sanction can be imposed at the earliest 12 months

after the initial registration for welfare benefits, while working hours are counted from the first day of registration onwards. After a sanction has been imposed resulting in a permanently lower monthly benefit level, the count of accumulated working hours is set to zero and the individual has to restart collecting 225 hours of employment before the sanction is removed.

Six months before the individual is potentially sanctioned, she receives an official warning from the municipality, which is responsible for the benefit payments (we also refer to this as the 'default warning' in the following). All welfare recipients who would be sanctioned if they do not work any additional hours during the next six months receive such a warning letter. It informs them about the potential reduction date and the number of working hours that are missing to prevent the sanction. Those who are still at risk of being sanctioned receive another warning letter one month before a potential reduction of their benefit level. Apart from these two official warnings sent out per default, welfare recipients have only limited access to direct information about their own situation, i.e. the number of working hours they have already collected. Individuals can arrange to meet their caseworker, who can then contact a corresponding database to receive information about the number of collected working hours. Alternatively, benefit recipients can keep count of the working hours themselves. However, both options require substantial effort and welfare recipients might not be aware of the importance of keeping such a count, respectively the option to contact the corresponding database.

Finally, it should be noted that the work requirement applies to welfare recipients deemed capable of working a minimum of five hours per week. Exemptions from the rule are granted to welfare recipients at the discretion of their caseworker, typically to individuals with very limited ability to work due to either mental or physical constraints.

2.2 Randomized Controlled Trial

Our experiment builds on the fact that individual welfare recipients face a substantial degree of uncertainty regarding their personal risk of receiving a benefit sanction, respectively the likelihood of being released from an existing sanction. Therefore, our information intervention targets two groups of individuals from the stock of welfare recipients. First,

the *at-risk group* stands to incur a benefit reduction within the next six months (target group A). Hence, they already received their first official default warning, but a sanction has not yet been imposed. They may need to work additional hours in order to avoid a benefit sanction. Second, the *sanctioned group* contains all welfare recipients who have already received a sanction and are currently subject to a reduced benefit level (target group B). They have to collect additional working hours in order to be released from the sanction and receive full benefits again. For both groups, we want to investigate how the provision of information about the general work requirement and their personal situation causally affects the individuals' labor market performance. Therefore, in our 2×3 between-subjects design, we divide both groups of welfare recipients into three treatment arms (see Table 1 for an overview). First, the control group receives no additional information beyond the official warning letters sent out by the municipality. This implies that among those who are at risk of receiving a sanction, the control group received the first default warning within the last six months and might receive the second default warning one month before the potential benefit reduction. Those who are currently sanctioned have not received any further information, since the sanction was imposed.

Table 1: Overview of experimental design

A) At-risk group	Treatment status		
	A.1) Control	*A.2) Message*	*A.3) Tool*
Received default warning Stand to incur reduction within six months	*No additional information:* Received only default warning	*Monthly warning:* Reminder about upcoming benefit reduction General information about work requirement of 225 hours/year	*Monthly warning:* Reminder about upcoming reduction General information about work requirement of 225 hours/year
		No personalized information No access to information about collected working hours	*Online tool:* Link to online tool providing personalized information Illustrating collected working hours within 12-month-window prior to next potential reduction date

B) Sanctioned group	Treatment status		
	B.1) Control	*B.2) Message*	*B.3) Tool*
Currently sanctioned Receiving reduced benefits	*No additional information* since benefit sanction was imposed	*Monthly message:* Information that benefit sanction is currently in place General information about work requirement of 225 hours/year	*Monthly message:* Information that benefit sanction is currently in place General information about work requirement of 225 hours/year
		No personalized information No access to information about collected working hours	*Online tool:* Link to online tool providing personalized information Illustrating collected working hours since imposition of benefit sanction

Note: Depicted are the six treatment arms of the 2×3-design.

Second, individuals in the *message treatment* receive up to six additional monthly reminders. The at-risk group is reminded that they still stand to incur a reduction, while the sanctioned group is informed that they are currently receiving a reduced amount of welfare benefits. Independently of the target group, the messages contain general information about the requirement of working 225 hours per year and provide some examples on how to fulfill the criteria. The messages emphasize the risk of receiving reduced welfare benefits and the importance of keeping an overview of one's collected working hours. For the at-risk group, the treatment makes the threat of benefit sanctions more salient and is therefore similar to a warning about an upcoming sanction (as investigated, e.g., by Lalive *et al.*, 2005; Arni *et al.*, 2013). For recipients who are already receiving reduced welfare benefits, the information treatment increases the salience of the imposed sanction. It might be the case that individuals are not aware that they are currently sanctioned as they, e.g., do not check their benefit payments regularly, or that they have already gotten accustomed to the reduced benefit level. Importantly, the treatment only informs the individual that there is a risk of receiving a sanction, respectively that a sanction is in place. It does not provide personalized information on how many hours the specific individual has to work in order to avoid or remove the sanction.

Third, the *tool treatment* additionally provides the individual with personalized information about collected working hours and the deadline for rule compliance. This personal information clearly communicates to the individual what is required of her to comply with the working requirement and continue to receive the full level of welfare benefits. Similar to the message treatment, individuals receive monthly reminders containing general information, but the messages also include a link to an online tool that provides personalized information about their collected working hours. For the at-risk group, the tool displays the next potential reduction date and graphically illustrates how many hours the individual has already worked within the 12-month window prior to their reduction date (see Figure A.1 in Appendix A.2). For those who are already sanctioned, the tool illustrates the number of hours worked since the imposition of the sanction. Moreover, since only the last 12 months are relevant for the work requirement, the tool also shows the next date (and the corresponding number of hours), when collected working hours will forfeit.

Finally, the tool also provides more detailed general information about the 225-hours rule, which are accessible by pressing information buttons on the tool-webpage.

2.3 Procedural Details

All individuals, who are registered as welfare recipients and belonged to one of the two target groups described above on August 15, 2018, were randomly assigned to one of the three treatment groups. Individuals in message and tool group received the initial message on August 15, 2018, followed by up to five monthly reminders as long as they continued to belong to one of the two target groups, i.e. at-risk or sanctioned. It should be noted that welfare recipients who were initially at-risk of receiving a sanction might actually have been sanctioned over the course of the experiment. If this was the case, they switched from target group A to B and received the corresponding message adapted to their new situation, but the assignment to the different treatment arms remained constant. All messages were sent out by the Danish public employment services (PES) to the individual's inbox at the official web portal of the Danish PES (*jobnet.dk*). The web portal also contained the online tool, activated only for individuals assigned to the tool treatment during the experiment.

2.4 Hypotheses

Previous evidence indicates that warnings as well as the enforcement of benefit sanctions increase transition rates from unemployment to employment (Abbring *et al.*, 2005; Van den Berg *et al.*, 2004; Lalive *et al.*, 2005; Svarer, 2011), but lead to lower wages and reduced job stability (Arni *et al.*, 2013; Van den Berg and Vikström, 2014). [6] We thus expect the message treatment, which comprises monthly warnings, to stimulate additional exits from welfare due to an increased likelihood of accepting jobs with a lower match quality, e.g. jobs with fewer hours, lower pay or reduced job stability. The consequences for cumulated earnings and overall employment remain theoretically ambiguous depending on the importance of the effects on the intensive relative to the extensive margin. Moreover, we also expect a reduced probability of receiving sanctions for two reasons. First,

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[6]Similarly, tighter search requirements are found to be successful in reducing benefit payments, but do not translate into stable employment (Petrongolo, 2009; Arni and Schiprowski, 2019).

welfare recipients might collect more working hours such that compliance with the work requirement is ensured. Second, individuals who leave the welfare system are no longer subject to the requirement. Mechanically, this will reduce their risk of being sanctioned.

For welfare recipients, who already receive benefits at a reduced level, a similar mechanism should apply. Reminding sanctioned individuals that they could receive higher benefits might motivate them to intensify their job search or to exert effort to increase working hours. However, we expect the sanctioned group to react somewhat different to the intervention than individuals in the at risk-group for three reasons. First, those who are already sanctioned may be a different selection of the population of welfare recipients, characterized by longer benefit spells and weaker labor market attachment. This might reduce the overall impact of the information treatment on labor market outcomes. Second, increasing the salience of a reduction may have no or only minor effects on those, who are already perfectly informed about the benefit reduction. Finally, a third argument can be derived from prospect theory (Tversky and Kahneman, 1979). Assuming that welfare recipients are loss averse relative to a reference point, which depends on their previous income, those who receive benefits at a reduced level might have already adapted their reference point to the lower benefit level. This would result in a lower search intensity and a reduced likelihood of accepting low paid or unstable jobs in response to the treatment (see e.g. DellaVigna *et al.*, 2017).

In comparison to the message treatment, the tool treatment contains precise personal information about collected working hours and the deadline for rule compliance. This may encourage individuals to organize their working hours in a way that minimizes the risk of being sanctioned, respectively maximizes the probability of having an imposed sanction removed. Thus, we expect that labor market outcomes resulting from the tool treatment would be associated with higher individual utility compared to the outcomes resulting from the message treatment due to a higher degree of efficiency resulting from precise personalized information. It is, however, a priori unclear whether this would lead to an increase of overall employment as the binary incentive (associated with the 225-hours) might also give rise to gaming effects. Specifically, welfare recipients might want to exert just enough effort to fulfill the work requirement, which could encourage some of them to

reduce their working hours, while others with collected hours below the cut-off would try to increase their working hours. A comparison of the distribution of yearly working hours across treatment arms will allow us to assess, whether bunching has a higher prevalence among the tool treated individuals relative to individuals in the message and control groups. [7] It should be noted that the online tool does not take the dynamic nature of the 225-hours rule into account. Due to the fact that the work requirement has to be fulfilled at each point time (when considering the past 12 months), collected hours will continuously forfeit. Therefore, the welfare recipient might be still at risk of being sanctioned in the following month even after fulfilling the requirement in a given month. Since the tool only displays information related to the next potential reduction date, a forward-looking optimization of employment with the aim to minimize work effort and fulfill the work requirement might still be difficult.

2.5 Data

To investigate the consequences of the intervention for subsequent labor market outcomes, we link the experimental data to comprehensive register data administered by the Danish Employment Agency and Statistics Denmark. This provides us with detailed information on socio-demographic background characteristics obtained from population registers, benefit payments (DREAM), as well as income and employment (E-income), including labor market histories of individuals in our sample. We construct various outcome variables that allow us to identify potential effects of the intervention on labor market outcomes and benefit receipt. First, we consider whether the individual received any welfare benefits, respectively any wage income for the first six months following the intervention. Moreover, we are specifically interested in the effect of the intervention on working hours and earnings. We therefore consider cumulative number of working hours and labor earnings within the first year after the beginning of the intervention.

It should be noted that our measures of employment and earnings are constructed from a database that covers only paid employment, characterized by employer-reported hours and earnings, while other types of employment, such as self-employment, may also

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[7]Chetty and Saez (2013) find evidence of similar behavioral responses when individuals receive information about tax credits.

contribute to the fulfillment of the work requirement.[8] Hence, the observed working hours do not allow us to draw direct conclusions on, whether the individual has been sanctioned or not. Therefore, we also rely on a database administered by the Danish PES that stores information on imposed benefit sanctions and exemptions from the 225-hours rule granted by the caseworker. Finally, we exploit data on registered job applications from an official online platform of the PES, which is called *joblog*. Although welfare recipients are not legally required to document their job search activities, they are encouraged to do so as it is typically used as the basis for individuals' meetings with their caseworkers (see Fluchtmann *et al.*, 2019).

2.6   Descriptive Statistics

Table 2 presents summary statistics with respect to the overall number of welfare recipients, granted exemptions and imposed sanctions. On average, about 25% of all welfare recipients are exempted from the work requirement. In September 2018 (immediately after the start of our intervention), the work requirement applies to about 92,500 welfare recipients, while the share of sanctioned individual has been stable at 11-13% per month from 2017 to 2019.

Table 2: Descriptive statistics: welfare recipients, exemptions and sanctions over time

	Mar 2017	Sep 2017	Mar 2018	Sep 2018	Mar 2019	Sep 2019
Total no. of welfare recipients (in 1,000)	152.6	139.6	137.8	125.0	126.2	115.1
Exemption granted	39.7	36.3	35.8	32.5	32.8	29,9
Sanction imposed	17.4	15.5	16.4	16.0	15.4	14.5

Source: Danish Agency of Employment and Labor Market Recruitment (STAR).

From the full stock of welfare recipients on August 15, 2018, our intervention targets all individuals who already received their first official default warning (which took place approximately six months after their initial registration). In total, our estimation sample comprises 64,476 welfare recipients who are randomly assigned into the three treatment arms as described in Section 2.2.

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[8]Income earned from self-employed work is translated into working hours using a specific translation rule. Approximately, 20% of individuals subject to the work requirement gather (some) working hours from self-employed work.

Figure 1: Distribution of working hours
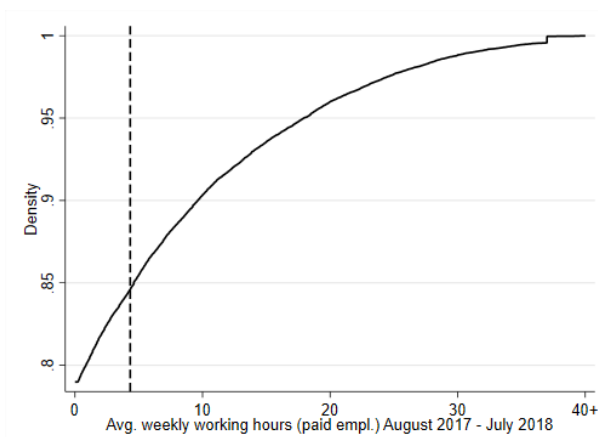
Note: Depicted is the cumulated density function of the average weekly working hours in paid employment within the 12-month period before the start of the intervention. The dashed characterizes the weekly equivalent of the work requirement of 225 hours per 12-month period.

Figure 1 shows the distribution of working hours in paid employment for the experimental population in the 12-month period before the start of the intervention. The vast majority (79%) of the welfare recipients did not work at all during the last year, while only about 15% worked more hours than postulated by the work requirement.[9] This illustrates that our intervention mainly targets individuals with a very limited attachment to the labor market, which is further supported by the individual background characteristics shown in Table 3. The welfare recipients had an average monthly gross labor income of only 1,600DKK (equivalent to approx. 235$), already received welfare benefits for more than two years without interruption and 60% categorized as 'requiring activation' (as opposed to 'being ready for employment'). The latter implies that their caseworkers do not consider them capable of starting full time employment without further support.

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[9]Note that this does not allow to draw conclusions about the share of welfare recipients who actually received a benefit sanction since not all individuals have been subject to the work requirement in the corresponding period.

Table 3: Summary statistics

	Control (C)	Message (M)		Tool (T)	
	Mean	**Mean**	**P−value**	**Mean**	**P−value**
No. of observations	21,498	21,491		21,487	
Female	0.484	0.480	0.442	0.488	0.427
Married	0.171	0.172	0.796	0.176	0.217
Age					
16-25 years	0.198	0.202	0.357	0.197	0.773
26-35 years	0.270	0.264	0.153	0.266	0.316
36-45 years	0.220	0.219	0.812	0.227	0.058
46-55 years	0.201	0.201	0.929	0.201	0.944
56-65 years	0.111	0.114	0.286	0.109	0.539
65 years+	0.001	0.001	0.684	0.001	0.684
Type of benefits					
Welfare	0.640	0.640	0.988	0.640	0.976
Integration	0.077	0.077	0.965	0.077	0.940
Education	0.283	0.283	0.992	0.283	0.991
Requires activation	0.626	0.626	0.997	0.626	0.999
Children					
No children	0.621	0.620	0.892	0.613	0.099
One child	0.150	0.152	0.544	0.153	0.296
Two children	0.110	0.107	0.305	0.113	0.406
Three or more children	0.119	0.121	0.605	0.121	0.610
Household size					
One person	0.299	0.298	0.761	0.303	0.428
Two persons	0.236	0.230	0.177	0.233	0.484
Three persons	0.170	0.168	0.664	0.171	0.889
Four persons	0.119	0.126	0.018	0.119	0.890
Five or more persons	0.176	0.177	0.798	0.175	0.666
Ethnicity					
Danish	0.708	0.700	0.069	0.705	0.457
Descendant	0.036	0.037	0.604	0.037	0.869
Immigrant	0.256	0.263	0.095	0.259	0.481
Living in Capital Region	0.312	0.309	0.526	0.308	0.319
Subsequent weeks on welfare	133.9	133.4	0.702	134.3	0.769
Any employment in year $t - x$					
$t - 1$	0.210	0.206	0.272	0.214	0.393
$t - 2$	0.247	0.244	0.485	0.249	0.564
$t - 3$	0.246	0.244	0.643	0.247	0.714
Labor earnings in DKK in year $t - x$					
$t - 1$	1,646	1,592	0.222	1,633	0.769
$t - 2$	2,362	2,311	0.374	2,361	0.985
$t - 3$	2,562	2,533	0.640	2,569	0.907

Note: Depicted are descriptive statistics separated for individuals in the three treatment arms. Percentage share unless indicated otherwise. P−values measured based on two-tailed t-tests on equal means.

Moreover, about 47% of the participants are younger than 35 years, about 48% are female and 17% are married. It can be also seen in Table 3 that most of the background characteristics are balanced across treatment groups. There are almost no statistically significant differences in socio-demographic characteristics and labor market histories between the tool, message, and control group. Nevertheless, we condition on the full set of covariates in our empirical model to address any minor differences.

To assess treatment take up, we consider individual-level click data. Around 42.6% of all welfare recipients in the message and tool treatment opened at least one of the messages that they received, while there is no difference with respect to the likelihood of reading the message between the two groups ($p = 0.987$). Moreover, 11.6% of the individuals in the tool treatment clicked on the link to the online tool at least once within a year after the intervention.

3 Empirical Analysis

Before examining the effect of our intervention on the avoidance of sanctions in Section 3.2 and the welfare recipients' subsequent labor market outcomes in Section 3.3, we present our estimation strategy. Afterward we investigate the underlying mechanisms in Section 3.4 and analyze heterogeneous treatment effects in Section 3.5.

3.1 Estimation Strategy

To identify the causal effects of our intervention we estimate the following empirical model:

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 X_i + \varepsilon_i, \tag{1}$$

where D_i indicates the treatment status (dummy variables for the tool, respectively message group), X_i is a vector of pre-intervention control variables, i.e. socio-demographic characteristics and labor market histories, as presented in Table 3 and dummies for place of residence (98 municipalities), and Y_i denotes the different outcome variables of interest. The model estimates the intention-to-treat effect (ITT), ignoring whether treated individuals actually read the message, respectively clicked on the link to the tool. As discussed in Section 2.2, the nature of the intervention differs between individuals who are at-risk

of receiving a sanction and those who are already sanctioned. Therefore, we estimate separate models for the at-risk and the sanctioned group.

Moreover, relative to the control group, the message treatment should only affect the individuals' behavior if they open the message. Therefore, we also estimate *treatment effects on the treated*, i.e. treatment effects for those who actually opened the treatment message, by two-stage least squares (see e.g. Angrist *et al.*, 1996). Specifically, we instrument the dummy indicating whether the individual opened the message with an indicator of the treatment status ($0 \equiv control$ and $1 \equiv message$). Similarly, we can also estimate the effect on those who clicked on the link to the online tool. Since the only difference between the tool and the message treatment is the link to the online tool, we can use the treatment status ($0 \equiv message$ and $1 \equiv tool$) as an instrument for the individual decision to click on the link. Since only about 43% of the welfare recipients opened the treatment message and about 12% clicked on the link, this allows us to scale the treatment effects by the share of compliers.

3.2 Avoidance of Benefit Sanctions

Our first objective is to analyze whether and how the information treatment affects the likelihood to receive, respectively to avoid a benefit sanction. All participants in the at-risk group already received their first default warning and face a potential sanction within the upcoming six months. Hence, our first set of results considers treatment effects on outcome variables related to the avoidance of benefit sanctions within the first six months after the start of the intervention. One possibility to avoid a sanction is to receive an exemption from the work requirement, which could be granted by the caseworker. To achieve an exemption, the welfare recipient would have to argue that it is unreasonable to fulfill the work requirement due to their personal mental or physical condition. Alternatively, a welfare recipient can start a regular job. This increases the likelihood that they fulfill the requirement, but also that they eventually leave welfare and therefore are no longer subject to the 225-hours rule. Finally, individuals can also leave the welfare system without finding a paid job, e.g., if they can rely on another source of income such as financial help from relatives or a partner, respectively self-employed, undeclared work or other income

support programs. Hence, we also consider the likelihood to receive an exemption, to start paid employment and to have at least one month without benefit payments within the first six months after the intervention as supplementary outcome variables.

Figure 2: ITTs on realized sanctions and labor market outcomes after six months

A. At-risk group **B. Sanctioned group**

Message - control   Tool - control   Tool - message
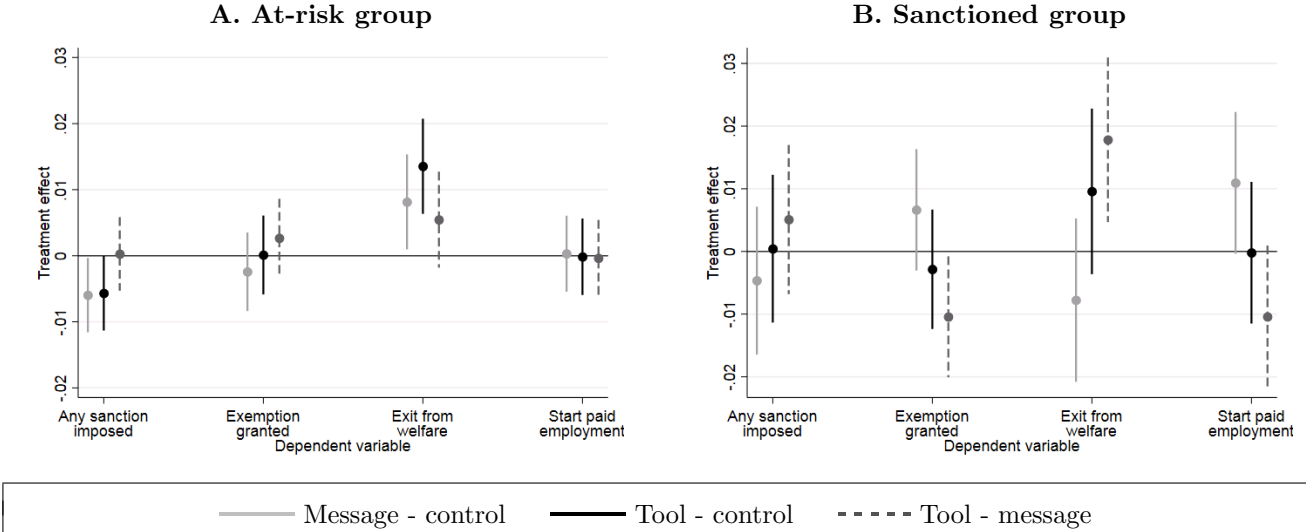
Note: Depicted are intention-to-treat effects including 90% confidence intervals.

Figure 2 shows the ITTs of the message, respectively the tool treatment. Panel A reveals that both message and tool treatment reduce the likelihood that individuals in the at-risk group actually receive a benefit sanction by about 0.6 percentage points. Both effects are statistically significant at the 10%-level and are equivalent to a reduction of the sanction rate by 5%. Moreover, there is no evidence that the lower sanction rate is provoked by an increased likelihood to get an exemption from the work requirement. This gives a first indication that our intervention must either increase employment and/or encourage individuals to leave welfare, since these are the only possibilities to prevent a benefit sanction, if the individual is not exempted from the work requirement. Although our estimates show no effect on the probability to start any paid employment, both message and tool treatments lead to an increased likelihood of having a month without any welfare benefit payment. This discrepancy is particularly interesting as it indicates that warnings encourage individuals to leave the welfare system even when they cannot find paid employment. When considering welfare recipients who are already receiving benefits at a reduced level, we find no significant effect of the two information treatments

on the likelihood of being released from the benefit sanction or getting an exemption from the work requirement relative to the control group (see Panel B of Figure 2). It should be noted, however, that there are some statistically significant differences between individuals in the tool and the message treatment. Specifically, having access to the personalized information in the online tool increases the likelihood of not receiving welfare benefits for at least one month by 1.8 percentage points relative to those who only receive the message. The effect is equivalent to a reduction of about 6% and is statistically significant at the 5%-level. Moreover, individuals in the tool group are also less often exempted from the work requirement. One could speculate that the two effects offset each other and therefore do not lead to a change in the sanction rate.

3.3   Cumulated Labor Market Outcomes

In a second step, we further investigate the behavioral consequences of our information treatments by considering realized labor market outcomes cumulated over the first year after the start of the intervention. Specifically, our main outcome variable comprise the total number of months without any benefit payments, the cumulated working hours from paid employment and the corresponding total earnings. Moreover, we also consider the individuals' total income as the sum of benefit payments and labor earnings.

3.3.1   Intention-to-treat Effects

Table 4 shows ITTs on the four outcome variables. The message treatment has no statistically significant effect on the number of months without welfare payments, however total working hours and earnings of welfare recipients in the message group work about 5% lower than in the control group. Both effects are statistically significant at the 10%-level. Moreover, we also observe a reduction of the total income, which is even stronger than the negative effect on labor earnings and statistically significant at the 1%-level. Together with the evidence presented in Section 3.2, the estimation results indicate that welfare recipients who receive repeated warnings about upcoming sanctions change their behavior to avoid the sanction in the short-run, but this comes at the cost of reduced long-run labor market performance. This is in line with previous evidence for UI benefit recipients

showing that warnings increase exit rates from benefit systems (Lalive *et al.*, 2005), but lead to lower subsequent labor market earnings (Arni *et al.*, 2013).

Table 4: Treatment effects on cumulated labor market outcomes after 12 months

	Mean value control group	Intention-to-treat effects		
		Message - control	Tool - control	Tool - message
A. At-risk group				
Dependent variable:				
No. of months w/o welfare benefits	2.031	0.056 [0.142]	0.159 [0.000]	0.098 [0.012]
No. of working hours	123.1	-5.513 [0.082]	0.828 [0.799]	6.347 [0.046]
Labor earnings in DKK	19,288	-1,017 [0.053]	-82 [0.879]	921 [0.076]
Total income in DKK[a]	133,781	-1,473 [0.004]	-1,420 [0.006]	27 [0.958]
No. of observations		31,533	31,530	31,525
Control variables		Yes	Yes	Yes
B. Sanctioned group				
Dependent variable:				
No. of months w/o welfare benefits	2.417	0.029 [0.675]	0.145 [0.037]	0.127 [0.069]
No. of working hours	150.7	6.451 [0.300]	5.944 [0.343]	-0.385 [0.951]
Labor earnings in DKK	23,338	1,255 [0.214]	1,293 [0.209]	-29 [0.978]
Total income in DKK[a]	127,852	1,649 [0.056]	499 [0.574]	-1,273 [0.147]
No. of observations		11,456	11,455	11,453
Control variables		Yes	Yes	Yes

Note: Depicted are intention-to-treat-effects on the number of months without benefit payment, cumulated working hours in paid employment, labor earnings and the total income within the first 12 months after the start of the intervention. $P-$values are shown in square brackets.
[a]Total income refers to the sum of the sum of labor earnings, welfare benefits and educational support payments.

When considering the effect of the tool treatment that gives welfare recipients access to personalized information about their own situation, i.e. the number of collected working hours and the deadline for rule compliance, the picture looks very different. Having access to the tool increases the number of months without welfare benefits by about 8% (the effect is statistically significant at the 1%-level) and there is no reduction of the total working hours and labor earnings compared to the control group. Relative to the message, the tool treatment significantly increases working hours and earnings, which indicates that receiving personalized information counteracts the negative effect of the pure warning. At the same time, the tool treatment decreases the time spent in welfare, which explains the lower sanction rate. Interestingly, the tool treatment leads to a similar reduction of the total income as the message treatment. Although this is driven by reduced benefit payments rather than lower labor earnings, the reduction in total income for both treatments shows that a substantial share of individual leaving the welfare system does not start a new job. Since this is true independently of the informational content of the warning, the it seems unlikely that the sanction regime efficiently reduces any distortionary effects of the welfare system (Feldstein, 2005).

For those who already receive reduced benefits at the beginning of the intervention, the message treatment increases the individuals' total income by about 1.3% (the effect is statistically significant at the 10%-level), while having access to the online tool increases the number of months without benefit payments by about 5.9% (the effect is significant at the 5%-level). There are also insignificant increases in the number of working hours and in earnings. The relative effect size is of a similar order of magnitude as the significant reduction of benefit payments. Hence, it appears likely that the increased total income (for the message treatment) and the reduced benefit payments (for the tool treatment) are partly provoked by increased paid employment.

3.3.2 Treatment Effects on Treated

For welfare recipients who are at-risk of receiving a sanction repeated warnings through the message treatment reduce the individual's labor market performance with respect to working hours and earnings from paid employment. Moreover, having additionally access

to personalized information about the collected working hours through the tool treatment seems to counteract the adverse affects of the warning. This leads to an overall effect that is statistically insignificant and close to zero when comparing the individuals in the tool treatment and the control group. This, however, neglects the fact that only 12% of the individuals receiving the tool treatment actually click on the link to the online tool, while about 43% read the message. Hence, we now estimate treatment effects on the treated of reading the message, respectively clicking on the link as described in Section 3.1 to assess the effect of actually accessing the provided information. The results are shown in Table 5. It can be seen that the treatment effects on the treated are qualitatively similar to the ITTs presented in Table 4, which is reassuring as it confirms that observed changes in labor market outcomes are driven by individuals, who actually read the message, respectively utilize the online tool. Moreover, we can see that the positive effect of receiving personalized information through the tool treatment is substantially larger than the negative effect of the pure warning through the message treatment. Therefore, the overall effect of receiving warnings in combination with personalized information about one's own situation is likely to be much more favorable than indicated by the findings presented in Table 4.

Table 5: Treatment effects on treated for cumulated working hours and earnings

	Mean value control group	Treatment effects on treated	
		Message - control	Tool - message
A. At-risk group			
Dependent variable:			
No. of months w/o welfare benefits	2.031	0.154 [0.141]	1.028 [0.012]
No. of working hours	123.1	-15.06 [0.082]	66.46 [0.046]
Labor earnings in DKK	19,288	-2,777 [0.052]	9,640 [0.075]
Total income in DKK[a]	133,781	-4,022 [0.004]	285 [0.958]
F-statistic for weak identification		10,339	1,765
No. of observations		31,533	31,525
Control variables		Yes	Yes
B. Sanctioned group			
Dependent variable:			
No. of months w/o welfare benefits	2.417	0.049 [0.673]	0.745 [0.067]
No. of working hours	150.7	10.88 [0.297]	-2.26 [0.952]
Labor earnings in DKK	23,338	2,118 [0.212]	-170 [0.978]
Total income in DKK[a]	127,852	2,783 [0.055]	-7,476 [0.145]
F-statistic for weak identification		9,993	1,224
No. of observations		11,456	11,453
Control variables		Yes	Yes

Note:

Note: Depicted are treatment effects on treated of (i) opening the message (relative to the control group) and (ii) clicking on the link to the online tool (relative to the pure message) on the number of months without benefit payment, cumulated working hours in paid employment, labor earnings and the total income within the first 12 months after the start of the intervention. $P-$values are shown in square brackets.

[a] Total income refers to the sum of the sum of labor earnings, welfare benefits and educational support payments.

3.4 Mechanisms

Next we want to investigate the underlying mechanisms in more detail. Specifically, we further investigate the exits from the welfare system, examine the characteristics of jobs accepted by the welfare recipients and test whether the information treatment encourages individuals to game the system. Finally, we also provide evidence regarding job applications registered in the official online portal of the PES.

3.4.1 Exits from Welfare

The discrepancy between treatment effects on working hours, respectively earnings on one hand and benefit payments, respectively imposed sanctions on the other hand implies that the treatment has behavioral consequences beyond the take up of paid employment. In the following, we investigate these effects in more detail. One explanation might be that individuals switch to other types of income support, which are not subject to the sanction regime. For instance, when being enrolled in a higher educational program (secondary or tertiary education), Danish citizens are generally eligible to public support. Since this also applies to our sample of welfare recipients, one could expect that treated individuals, for whom the welfare system appears less attractive due to the sanction regime, make greater use of this opportunity. Similarly, welfare recipients who are already eligible to early retirement schemes could rely on pension payments[10], while others may avoid a sanction by reporting sick.[11]

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[10] Individuals are eligible to early retirement five years before the legal retirement age. The latter depends on the individual's birth date and is currently fixed at 67 years for those born after 1955.

[11] van den Berg *et al.* (2019) investigate the role of this additional layer of moral hazard in the German UI system. They find evidence for strategic sick-reporting in order to avoid benefit sanctions.

Table 6: Treatment effects on other types of income support after 12 months

	Mean value control group	Message - control	Tool - control	Tool - message
			Intention-to-treat effects	
A. At-risk group				
Dependent variable:				
No. of months w/o welfare benefits	2.031	0.056 [0.142]	0.159 [0.000]	0.098 [0.012]
No. of months receiving				
educational support	0.535	0.060 [0.011]	0.064 [0.006]	0.005 [0.873]
pension payments	0.393	0.044 [0.040]	0.034 [0.105]	-0.009 [0.696]
parental or sick leave	0.167	0.012 [0.408]	-0.005 [0.712]	-0.016 [0.257]
No. of months w/o paid employment or public benefit payments[a]	0.464	0.032 [0.085]	0.029 [0.110]	-0.005 [0.801]
No. of observations		31,533	31,530	31,525
Control variables		Yes	Yes	Yes
B. Sanctioned group				
Dependent variable:				
No. of months w/o welfare benefits	2.417	0.029 [0.675]	0.145 [0.037]	0.127 [0.069]
No. of months receiving				
educational support	0.587	0.050 [0.221]	0.066 [0.112]	0.017 [0.689]
pension payments	0.203	0.014 [0.612]	0.027 [0.339]	0.017 [0.551]
parental or sick leave	0.182	0.024 [0.337]	0.017 [0.514]	-0.004 [0.891]
No. of months w/o paid employment or public benefit payments[a]	0.639	-0.062 [0.064]	-0.026 [0.441]	0.041 [0.210]
No. of observations		11,456	11,455	11,453
Control variables		Yes	Yes	Yes

Note: Depicted are intention-to-treat-effects on the number of months (i) without receiving benefit payment and (ii) receiving other types of income support within the first 12 months after the start of the intervention. P−values are shown in square brackets.

[a]Refers to the number of months without any paid employment and without any public benefit payment (including unemployment benefits, welfare benefits, educational support, pension payments, parental or sick leave and other benefit payments). This covers periods of (i) self-employed work and (ii) out of the labor force.

Table 6 shows the treatment effect on the usage of these other types of income support. For the at-risk group (see Panel A), we find strong evidence that both types of warnings, including general (message treatment), as well as personalized information (tool treatment), encourage welfare recipients to enroll in an educational program. Both treatments have very similar effects and significantly increase the number of months receiving educational support by about 11-12% relative to the control group. Moreover, the pure warning without personalized information also has a positive significant effect on the number of months receiving pension payments (+11.2%) and we find a slightly smaller insignificant effect for the warning with personalized information (+8.7%). When considering the group of welfare recipients who are already sanctioned (see Panel B), the overall patterns looks very similar, while again the estimates are less precise presumably due to the smaller sample size. For instance, the tool treatment increases the number of months receiving educational support by 11.2% relative to the control group, but the effect is not significant at conventional levels. Nevertheless, all together, our estimates provide evidence that the exits from welfare in response to our information treatment can be partly attributed to the greater usage of other types of income support, especially associated to enrollments in educational programs.

Another important factor contributing to the increased time without benefit payments might be that welfare recipients engage in self-employed work, which is not covered by our dataset containing information on working hours and earnings. To get a sense of the relevance of such an effect, we additionally consider the number of months without any paid employment or public benefit payments.[12] By definition, this residual category comprises episodes (i) of self-employed work or (ii) of being out of the labor force. As shown in Panel A of Table 6, the message treatment significantly increases the number of months without any paid employment or income from public benefits, while it has a negative effect on this residual outcome for those who are already affected by the sanction (Panel B).

Hence, warnings about potential benefit sanction may also affect the likelihood to become self-employed. One could speculate that the labor market attachment of many

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[12]This includes all potential types of income support, i.e. unemployment benefits, welfare benefits, educational support, pension payments, sick or parental leave and other benefits.

welfare recipients is low and therefore their only chance to comply with the work require-
ment is to engage in self-employed work on an irregular basis. Since any earned income
would be deducted from the benefit payments, one would expect to observe short-term
interruptions of welfare payments. Alternatively, individuals might de-register themselves
permanently from welfare in response to the treatment relying on alternative income
sources, such as, e.g., self-employment on a permanent basis, financial support from rela-
tives or other public benefits.

Figure 3: ITTs on distribution of subsequent months w/o benefit payments
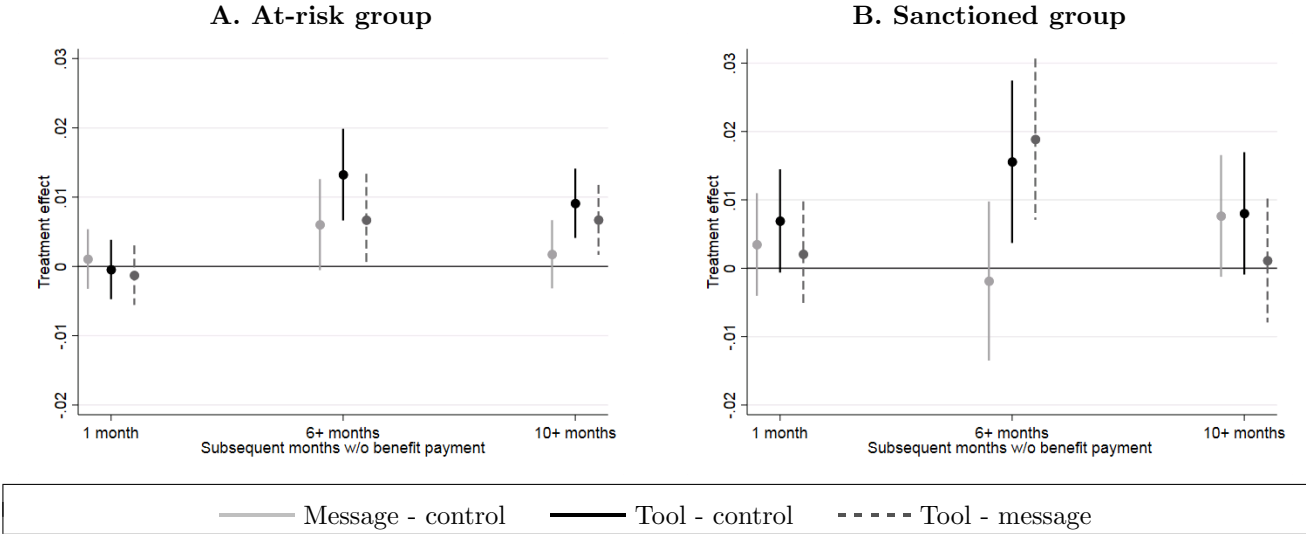
A. At-risk group   **B. Sanctioned group**

Note: Depicted are intention-to-treat effects on the distribution of subsequent months w/o benefit payments including 90%
confidence intervals.

Therefore, we now consider the number of subsequent months without receiving welfare
benefit payments to investigate whether the information treatments provoke permanent
exits from welfare. Specifically, we estimate ITTs on three indicator variables reflecting
the distribution of subsequent months without benefit payments. The first variable takes
the value one if the individual does not receive welfare benefits for one month, but does
in the month after. This allows us to measure short-term interruptions that might be
associated with irregular work opportunities. The other two outcome variables indicate
longer interruptions of the welfare spell of at least six subsequent months, respectively ten
months within the first year after the intervention. The results are presented in Figure
3 separated for the at-risk group and for the sanctioned group. The findings reveal that
the tool treatment significantly increases the probability of leaving welfare for at least six

months for both target groups. Since there is no effect on short-term interruptions, i.e. only one month without benefit payments, this indicates that the treatment encourages welfare recipients to leave the system permanently and rely on other income sources.

3.4.2 Job Characteristics

Gaining access to personalized information regarding one's own situation in relation to the work requirement seems to have more favorable consequences for subsequent labor market outcomes than just receiving pure warnings. This raises the question how welfare recipients use the personalized information that is provided. In a next step, we estimate treatment effects on characteristics of the first paid job individuals accept after the start of the intervention, i.e. hourly wage and weekly working hours of their first job. The analysis is somewhat descriptive as it does not take into account that only a selection of individuals accept a job at all. However, as we shall see, data patterns provide suggestive evidence into what are the most likely causal pathways.[13]

Table 7: Treatment effects on characteristics of first job

	Mean value control group	Intention-to-treat effects		
		Message - control	Tool - control	Tool - message
Full sample				
Dependent variable:				
Hourly wage in DKK	157.07	7.3615 [0.076]	0.4410 [0.793]	-6.7861 [0.102]
Weekly working hours	16.05	-0.0412 [0.839]	0.6042 [0.003]	0.6668 [0.001]
No. of observations		12,211	12,396	12,315
Control variables		Yes	Yes	Yes

Note: Depicted are ITTs on the characteristics of the first job after the intervention pooled for the at-risk and the sanctioned group. $P-$values are shown in square brackets.

Table 7 shows the corresponding estimation results. Since only about one fourth of all welfare recipients accept any paid employment within 12 months after the intervention, we now pool the two target groups (those who are at risk, respectively those who are already sanctioned) and consider the full sample to increase statistical power.[14] The findings

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[13]It should be noted that there are no statistically significant treatment effects on the likelihood of accepting any employment, which mitigates potential concerns regarding the selectivity of the considered sample.

[14]The results are qualitatively similar, when performing separate analyses for both groups.

indicate that the message and tool treatment have very different effects on the types of jobs that welfare recipients accept. While individuals who receive warnings about upcoming or already imposed sanctions tend to accept jobs with a higher hourly wage than the control group, while those who additionally receive access to the online tool start jobs with a higher number of weekly working hours. Having access to personalized information seems to encourage welfare recipients to collect more working hours (and waive a higher hourly wage) in the short-run. This, however, translates into higher cumulated earnings in the long-run.

3.4.3 Gaming Effects

As mentioned above welfare recipients face binary incentives to work exactly as many hours as are necessary to fulfill the work requirement of 225 hours per year. Hence, one might expect that especially individuals in the tool group who have the possibility to follow their exact number of collected working hours would try to 'game the system' by providing just enough effort to comply with the requirement. To test whether the information treatment creates such dysfunctional effects, we consider three indicator variables reflecting the distribution of total working hours within 12 months after the intervention (1-100 hours, more than 225 hours, and more than 350 hours) in Figure 4. For those who are at risk of being sanctioned neither the message nor the tool treatment seem to have consequences for the distribution of working hours. However, for those who are already sanctioned the tool treatment significantly reduces the likelihood of working between one and 100 hours per year, and significantly increases the likelihood of working more than 350 hours (+1.1 percentage points). Since the effect is smaller and statistically insignificant when considering the likelihood of working more than 225 hours (which refers to the legal requirement), the findings do not support the notion that personalized information promote gaming effects. Having access to personalized information about the number of collected working hours rather seems to encourage welfare recipients to provide substantially more work effort than legally required.

Figure 4: ITTs on distribution of total number of working hours within 12 months

A. At-risk group   **B. Sanctioned group**

——— Message - control ——— Tool - control - - - - - Tool - message
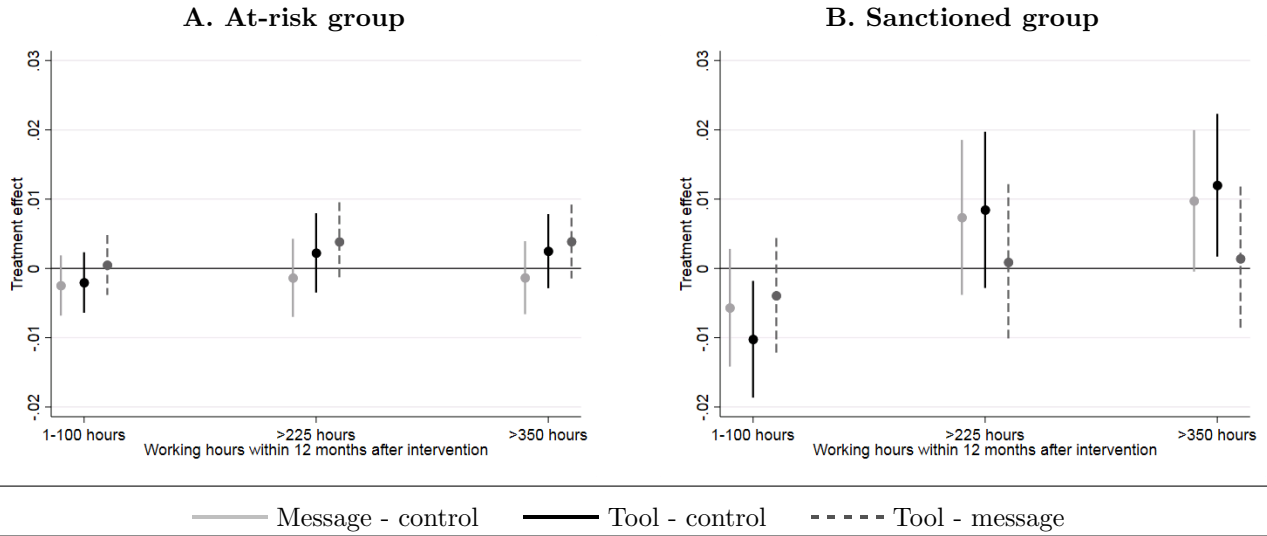
Note: Depicted are intention-to-treat effects on the distribution of the total number of working hours within 12 months after the intervention including 90% confidence intervals. The dashed line refers to the average number of weekly working hours required to comply with the work requirement of 225 hours within 12 months.

3.4.4 Job Search

Finally, we also consider a measure of individual job search behavior by exploiting information on job applications registered in the official online portal (*joblog.dk*). It should be noted that welfare recipients are not legally required to document their job search activities and therefore their number of registered job applications provides only an imperfect proxy for their job search effort. Nevertheless, analyzing treatment effects on registered job application might provide interesting insights as it could reveal behavioral responses beyond realized labor market outcomes. Specifically, we consider, whether the individual registers any applications at all, as well as the total number of registered applications within the first four weeks after the intervention. As shown in Panel A of Table 8, there is no evidence that the information treatment affects the behavior of the at-risk group.

For the sanctioned group who already receive benefits at a reduced level, the message treatment, however, increases the likelihood of registering any job applications by about 4.8% and the total number of applications by 5.8%. The effects are statistically significant at the 5%-, respectively 10%-level. This might reflect an increase in job search effort, which translates into the positive (insignificant, but sizable) employment effects documented in Table 4. It should be noted, however, that the tool treatment has no effect on registered job

Table 8: Treatment effects on registered job applications

	Mean value control group	Intention-to-treat effects		
		Message - control	Tool - control	Tool - message
A. At-risk group				
Dependent variable:				
Any registered job application	0.0846	0.0020 [0.4476]	0.0035 [0.1776]	0.0011 [0.6690]
No. of registered job applications	0.5364	0.0296 [0.1873]	0.0445 [0.0553]	0.0131 [0.5796]
No. of observations		31,533	31,530	31,525
Control variables		Yes	Yes	Yes
B. Sanctioned group				
Dependent variable:				
Any registered job application	0.2660	0.0128 [0.0383]	0.0020 [0.7429]	-0.0115 [0.0648]
No. of registered job applications	1.7488	0.1018 [0.0889]	-0.0054 [0.9253]	-0.1172 [0.0497]
No. of observations		11,456	11,453	11,456
Control variables		Yes	Yes	Yes

Note: Depicted are intention-to-treat-effects on the likelihood of registering any applications, respectively the total number of registered applications in the online registration portal of the Danish PES (*joblog.dk*) within the first four weeks after the start of the intervention. $P-$values are shown in square brackets.

applications relative to the control group. One could speculate that those who have access to the online tool understand that registering job applications does not increase their likelihood of fulfilling the work requirement. Therefore, they have no incentives to use the registration portal. On the contrary, the message treatment might create the perception that greater search effort helps to avoid a sanction, such that welfare recipients who only receive the message will aim to document their search activities towards the authorities.

3.5 Heterogeneous Treatment Effects

Finally, we examine heterogeneous treatment effects with respect to different background characteristics. Specifically, we focus on the consecutive time spent in welfare and the presence of a spouse both measured at the beginning of the intervention.

3.5.1 Welfare spell duration

Since welfare benefits are means-tested and do not expire (in contrast to UI benefits), the welfare duration has no direct influence on the individual incentives to search for a job.

However, it can be assumed that those who have been out of the system recently have a stronger connection to the labor market and therefore have greater possibilities to respond to the new information regarding the work requirement and the risk of sanctions. Hence, we divide our estimation sample into two groups based on the number of consecutive weeks in which the individual has received welfare benefits at the beginning of our intervention. We use a cut-off of 26 consecutive weeks of welfare receipt.[15]

The results are shown in column (1) to (4) of Table 9. Among individuals who are at-risk of being sanctioned (Panel A), take-up of the treatment is greater among those with a shorter welfare duration. They more often read the treatment message and they make greater use of the online tool.[16] Moreover, the negative effects, especially of the message treatment, on cumulated working hours and earnings are completely driven by those who have been out of the system recently. This indicates that the threat of receiving a benefit sanction creates the strongest reaction and is particularly harmful for labor market outcomes among welfare recipients with a relatively high labor market attachment. Additionally, access to personalized information in the tool is found to partially mitigate these large negative effects from warnings, however sizeable negative effects on earnings and cumulated working hours remain. The latter effect are, however, statistically insignificant. There is no evidence of heterogeneous treatment effects with respect to the welfare duration among those who are already receiving reduced benefits. As sanctions related to the work requirement can only occur, when welfare recipients have accumulated a year of welfare receipt, the group of sanctioned is likely to be more homogeneous and consist of individuals who are generally further from the labor market than the at-risk group.

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[15]The Danish PES officially distinguishes between welfare recipients' who are capable of taking up full-time employment and those who require some form of activation program. The consecutive welfare duration is strongly correlated with this official assessment. The share of welfare recipients who are classified as 'requires activation' is about 68% among those who received benefits for more than 26 consecutive weeks, while it is only 38% among those with a consecutive welfare duration of 26 weeks or less.

[16]This is not surprising given that those who are classified as being capable of working full-time are required to visit the online portal of the PES (*jobnet.dk*) at least once a week, while those who are assumed to require an activation program are not.

Table 9: Heterogeneous effects on cumulated labor market outcomes after 12 months

	Consecutive weeks on welfare				Partner living in same household			
	26 weeks or less		more than 26 weeks		No		Yes	
	Message - control	Tool - control	Message - control	Tool - control	Message - control	Tool - control	Message - control	Tool - control
A. At-risk group								
Dependent variable:								
No. of working hours	-26.60 [0.009]	-13.85 [0.185]	-0.74 [0.805]	5.43 [0.078]	1.46 [0.757]	2.67 [0.573]	-10.56 [0.014]	-1.01 [0.820]
Labor earnings in DKK	-4,541 [0.009]	-2,322 [0.188]	-185 [0.703]	614 [0.215]	14 [0.987]	389 [0.632]	-1,756 [0.011]	-488 [0.495]
Total income in DKK[a]	-4,193 [0.006]	-3,629 [0.019]	-772 [0.129]	-700 [0.169]	-1,722 [0.024]	-1,157 [0.131]	-1,234 [0.074]	-1,497 [0.031]
Share in treatment group								
red message	0.473	0.448	0.338	0.346	0.369	0.366	0.363	0.367
clicked on link		0.145		0.083		0.101		0.091
No. of observations	6,460	6,457	25,073	25,073	12,963	13,106	18,570	18,424
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
B. Sanctioned group								
Dependent variable:								
No. of working hours	10.72 [0.620]	-3.62 [0.863]	6.20 [0.315]	8.98 [0.153]	18.04 [0.050]	26.14 [0.007]	-0.79 [0.925]	-9.61 [0.248]
Labor earnings in DKK	1,527 [0.664]	1,042 [0.770]	1,309 [0.190]	1,532 [0.131]	2,962 [0.047]	5,230 [0.001]	211 [0.878]	-1,701 [0.204]
Total income in DKK[a]	-30 [0.993]	1,382 [0.671]	1,979 [0.017]	655 [0.441]	1,567 [0.207]	1,833 [0.174]	1,728 [0.143]	-790 [0.503]
Share in treatment group								
red message	0.551	0.572	0.600	0.592	0.624	0.646	0.570	0.548
clicked on link		0.178		0.169		0.1930		0.1539
No. of observations	1,831	1,874	9,625	9,581	4,702	4,772	6,754	6,683
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Depicted are intention-to-treat-effects on cumulated working hours and labor earnings within the first 12 months after the start of the intervention for different subgroups. $P-$values are shown in square brackets.
[a] Total income refers to the sum of the sum of labor earnings, welfare benefits and educational support payments.

3.5.2 Marital status

The presence of a partner affects the individual's incentives to react to information treatment for various reasons. First, a partner with working income reduces the individuals' dependence on welfare benefits, e.g., they can rely on the partner's income for consumption smoothing. Moreover, the overall amount of welfare benefits for a married person is limited such that the gross income of the couple does not exceed an amount equal to twice the regular benefit level of a single person. Both reasons imply that the incentives to react to the information treatment are lower for welfare recipients who have a partner. However, the size of the benefit sanction also depends on the situation of the partner. In most cases, the sanction will be larger as the presence of a partner generally implies that the benefit level of a welfare recipient who does fulfill the work requirement is reduced by 100%.[17] In summary, the presence of a partner increases the complexity of the underlying incentives, which may distort the effectiveness of the information treatment.

In column (5) to (8) of Table 9, we show separate treatment effects for welfare recipients with and without a partner in the same household. Interestingly, for the at-risk group, we detect a negative effect of the message treatment only for individuals with a partner. This is likely to reflect the greater rule complexity and uncertainty regarding optimal responses to the working requirement and the information treatment for individuals with a partner. Since the size of the potential benefit sanction may be larger for individuals with a partner, they might have stronger incentives to work some hours in order to avoid the upcoming benefit sanction. However, since they can also rely on the partner's income to smooth their consumption they might have a higher willingness to accept part-time or temporary jobs with a relatively low wage, which could explain the weaker cumulated labor market outcomes. Overall, contradictory incentives and reater rule uncertainty are likely to reduce the efficiency of responses for individuals with a partner.

For individuals in the sanctioned group, we find a strong positive effect of receiving warnings messages and an even larger positive effect of the tool treatment on those without

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[17]There is one exception when both partners receive welfare benefits and do not fulfill the work requirement. In this situation, only the benefit level of the partner who first misses the target is reduced by the full amount, while the other partner does not face a reduction of the benefit level. In such a situation, the couple can only increase their income if both partners comply with the work requirement.

a partner. This is presumably the group, who has the strongest incentives to react to treatment as they are already experiencing the negative implications of the lower income level and they cannot rely on a partner's income to smooth consumption. At the same time, their incentives to react to the treatment are not complicated by the presence of a spouse. The strong positive effect on realized labor market outcomes emphasizes that providing very basic information about individuals' own personal situation can substantially improve their subsequent labor market outcomes.

4 Discussion

In this paper we investigated how warnings and information about already enforced sanctions affect the labor market outcomes of welfare recipients, a group of unemployed workers with very limited labor market attachment. Based on a large-scale randomized experiment we make various contributions. First, we find conclusive evidence of substantial threat effects of warnings. Welfare recipients, who receive repeated warnings with general information about the welfare rules avoid the enforcement of a sanction by leaving the welfare system. In isolation, exits from welfare may reduce public expenditures on transfers. However, we can also show that they do not translate into improved labor market outcomes, but rather contribute to reduced working hours and earnings, while treated individuals tend to switch to other types of income support not subject to the sanction regime. To some extent these causal estimates corroborate earlier non-experimental findings (see e.g. Arni *et al.*, 2013; Van den Berg and Vikström, 2014) suggesting that the threat of a sanction encourages individuals to accept lower quality jobs. However, we can also show that the adjustment of the welfare recipients' behavior not only concerns realized labor market outcomes, but also affects educational or retirement decisions.

Moreover, our analysis adds first time evidence on a second important dimension, which has been neglected by previous studies, namely the informational content transmitted through warning messages. Specifically, we can show that personalized warnings, informing individuals about their own situation and the underlying incentive structure, are substantially more effective than providing general information. They lead to improved

labor market outcomes for those actually utilizing the information. This shows that information frictions regarding the individual's own situation may play an important role in the context of work requirements and benefit sanctions. For policy makers, who aim to design a sanction regime, it is important to note that the way of communicating the associated rules may be crucial for the effectiveness of the overall system.

This argument is further supported by the fact that also information about already enforced sanctions can improve welfare recipients' subsequent labor market outcomes, especially among those without a partner who have clear incentives to react to the information. Again, personalized information is more effective than pure warnings, but both treatments have a positive effect on labor market outcomes pointing towards substantial information frictions for this group. This underlines the importance of transparent incentive structures with regard to work requirements and benefit systems in general. The importance of individual knowledge about eligibility criteria is an important aspect, which is surely a worthwhile focus for future research.

Finally, it is worth noting that our information treatment generally stimulates exits from welfare to a greater extent than the creation of new employment. This is particularly interesting from a welfare perspective as it indicates that the sanction regime is not necessarily effective in restoring search incentives and reducing moral hazard. However, it may harm the individual's possibility to smooth consumption, which is typically seen as a socially beneficial feature of welfare systems to correct failures in the credit and insurance market (Chetty, 2008).

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A Appendix

A.1 Text of Messages

Message to treatment group A

How to avoid loosing your cash, integration or education benefits

Due to the 225-hours rule, you risk to incur a benefit reduction or to loose your benefits altogether. This will happen, if you have not worked at least 225 hours within the previous year. The rule applies, when you have received benefits for a total of 12 months within the last 3 years.

If you want to avoid loosing or incurring a reduction of your benefits, you need to pay attention to how many hours you are missing to gather a total of 225 hours.

Check your working hours regularly so you can plan how many hours to work a week in order to reach a minimum of 225 hours. Just a few hours of work per week can help you reach 225 hours and avoid a reduction of your benefits.

225 hours are equivalent to:

- 5 hours a week for 52 weeks

- 10 hours a week for 23 weeks

- 20 hours a week for 12 weeks

- 37 hours a week for 7 weeks

All the hours, you work today, will keep counting for the full next year. Therefore it still pays off to gather working hours after you have worked for 225 hours.

There are currently 20.000 job adds posted on jobnet.dk. Start in good time to collect working hours so you do not risk loosing money.

How to avoid loosing your cash, integration or education benefits
Due to the 225-hours rule, you risk to incur a benefit reduction or to loose your benefits altogether. This will happen, if you have not worked at least 225 hours within the previous year. The rule applies, when you have received benefits for a total of 12 months within the last 3 years.
If you want to avoid loosing or incurring a reduction of your benefits, you need to pay attention to how many hours you are missing to gather a total of 225 hours. A new tool on jobnet.dk can help you keep track of your working hours. The tool is called 'counter of hours' and is personal. The tool is regularly updated with your working hours.

Your 'counter of hours' gives you an overview of:

1. Hours you have worked that will be included in the count of 225 hours

2. Hours you are missing to reach 225 hours

3. Your deadline for gathering 225 hours

Check your 'hours counter' now. [LINK]

Check your working hours regularly, so you can plan how many hours to work a week in order to reach a minimum of 225 hours. Just a few hours of work per week can help you reach 225 hours and avoid a reduction of your benefits.

225 hours are equivalent to:

- 5 hours a week for 52 weeks

- 10 hours a week for 23 weeks

- 20 hours a week for 12 weeks

- 37 hours a week for 7 weeks

All of the hours you work today will keep counting for the full next year. Therefore it still pays off to gather working hours after you have worked for 225 hours.

When you log on to jobnet.dk to check your job adds, it is easy to keep track of your 'counter of hours'. You can find it on jobnet.dk under the menu item MY BENEFITS on the left side of the screen. Press the menu item '225-hours rule'.

There are currently 20.000 job adds posted on jobnet.dk. Start in good time to collect working hours so you do not risk loosing money.

Message to treatment group B

How to avoid loosing money every month

Your monthly benefits have been reduced due to the 225-hours rule. You can regain your right to full monthly benefits by collecting 225 hours of employment. Only hours you have worked after the reduction of your benefits are included in the count.

Check your working hours regularly, so you can plan how many hours to work in order to reach a minimum of 225 hours and return to full benefits. It takes approximately 12 weeks to reach 225 hours, if you take a part-time job.

All of the hours you work today will keep counting for the full next year. Therefore it still pays off to gather working hours after you have worked for 225 hours.

There are currently 20.000 job adds posted on jobnet.dk. Start collecting your 225 employment hours right away to regain your right to full benefits.

Message to treatment group B with link to tool
How to avoid loosing money every month

Your monthly benefits have been reduced due to the 225-hours rule. You can regain your right to full monthly benefits by collecting 225 hours of employment. Only hours you have worked after the reduction of your benefits are included in the count.

A new tool on jobnet.dk can help you keep track of your working hours. The tool is called 'counter of hours' and is personal. The tool is updated every month with your working hours.

Your 'counter of hours' gives you an overview of:

1. Hours you have worked that will be included in the count of 225 hours

2. How far you are from returning to full benefits

3. Hours you are missing to reach 225 hours

Check your 'hours counter' now. [LINK]

Check your working hours regularly, so you can plan how many hours to work in order to reach a minimum of 225 hours and return to full benefits. It takes approximately 12 weeks to reach 225 hours, if you take a part-time job.

All of the hours you work today, will keep counting for the full next year. Therefore it still pays off to gather working hours after you have worked for 225 hours.

When you log on to jobnet.dk to check your job adds, it is easy to keep track of your 'counter of hours'. You can find it on jobnet.dk under the menu item MY BENEFITS on the left side of the screen. Press the menu item '225-hours rule'.

There are currently 20.000 job adds posted on jobnet.dk. Start collecting your 225 employment hours right away to regain your right to full benefits.

A.2   Additional Tables and Figures

Figure A.1: The online tool
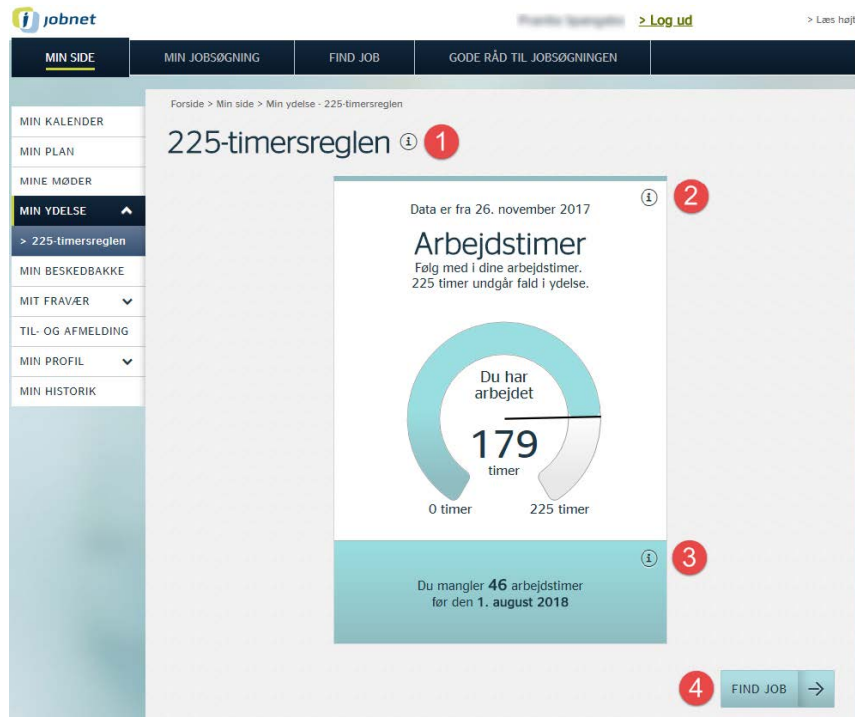
Note: Depicted is the online tool that provides personalized information about the welfare recipients own situation related to the requirement of working 225 hours within 12 months.
(1) provides general information about work requirement.
(2) explains number of collected working hours as of today.
(3) informs about potential reduction date and the number of hours that is missing to comply with the work requirement.
(4) link to online job search platform.