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Meng Le Zhang, Henrik Lindegaard Andersen, Liv Osland

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Measuring the effect of cash incentives on migrant integration in Norway: Early results from a quasi-experiment

Authors: Meng Le Zhang [1]*, Henrik Lindegaard Andersen [2], Liv Osland [2]

[1] Department of Social Science, University of Sheffield, Sheffield, United Kingdom

[2] Department of Economic Administrative Studies, Western Norway University of Applied Sciences, Haugesund, Norway.

*Corresponding author

Email: meng_le.zhang@sheffield.ac.uk (MZ), henrik.lindegaard.andersen@hvl.no (HA), liv.osland@hvl.no (LO)

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Abstract

The Norwegian Introduction Programme (NIP) is a flagship social policy for integrating migrants into Norwegian society. NIP is a two-year programme of full-time education and training. As part of NIP, a special benefit (Introduksjonsstønad) is paid to attendees to incentive participation. The Introduksjonsstønad is substantial, and it increases by 50% when participants reach age 25 (from approx. €12,860 to €19,290). Accounting for age, we find that increasing the Introduksjonsstønad results in increased NIP participation. Despite increasing cost and participation, we do not find any evidence of increased labour market outcomes. Norway's approach to integration is both ambitious and expensive however our early findings show that NIP may not be effective. Given the importance of integration in Norway and the role of NIP, we suggest that further research needs to be done into credible alternatives or improvements to NIP and the Introduksjonsstønad.

JEL: C12, I38, J15, J18

Keywords: Conditional Cash Transfer, Migrants, Integration, Norwegian Introduction Programme, Quasi-experiment, Regression Kink Design

1. Introduction

In the past two decades, various refugee crises have led to a rise in the number of non-economic migrants living in Europe. The optimal strategy for integrating these new residents is a source of public debate, and various countries have different arrangements. To improve policy efforts aimed at integrating immigrants, it is important to perform high-quality evaluations of existing migrant integration programmes. In this project, we study the Norwegian Introduction Programme (NIP) which is a programme of language, employment and cultural training for new migrants. Introduced under the 2003 Introduction Act, NIP is mandatory and obligatory for migrants who are either (i) refugees and/or (ii) family reunion migrants (with non-Nordic spouses) between the ages of 18-55 with a need for basic qualifications. During the period under study (2003 – 2020), it was expected that participants attend NIP courses full-time. It offers up to 2 years of training which is tailored to the individual (up to 3 years under exceptional circumstances).

NIP is an example of a conditional cash transfer (CCT) programme where money is transferred to an individual based on meeting certain conditions (World Bank 2015). Migrants are paid a special benefit called the Introduksjonsstønad (Introductory Benefit) to encourage participation in NIP. The amount earned is conditional on active attendance in NIP and is not means tested. Relative to other CCTs, the conditional benefit for full attendance is considerable and equivalent to double national insurance basic amount (2G[1] ≈ 196,000 NOK ≈ €19,290[2] per year). With few exceptions, the Introduksjonsstønad does not affect the receipt of any other benefits.

Following increased scrutiny of integration programmes in Norway (IMDI 2019, 4; Djuve et al. 2017) in Norway, it is particularly important to evaluate the effect of NIP and the Introduksjonsstønad. We exploit the Introduksjonsstønad rules to estimate the causal effect of increasing cash incentives on programme attendance and integration outcomes. The Introduksjonsstønad is calculated per hour of attendance in NIP and the hourly rate is much lower for those under 25 compared to those 25 and over (Norwegian Government 2003 s3.8). For example, when a participant is under 25 they earn at a rate of 1.33G (€12,860 per year). Once a participant turns 25, their next hours of participation will be paid at a higher rate of 2G (€19,290 per year). Since NIP is usually a two-year programme and there is an increase in benefits rates at age 25, the expected lifetime Introduksjonsstønad earned by each eligible migrant will in theory increase linearly between ages 23 and 25. As incentives increase, we should expect to see a gradual rise in programme participation between ages

[1] The basic amount (G) is set by the Norwegian Parliament (Storting), and is adjusted on a yearly basis. The level of the benefit is intended to be universal, versus locally determined. It is comparable to social insurance paid to people who has not had any previous earned income from work

[2] Based on average exchange rate in 2021: 1,000 NOK = 98.4 EUR

23 and 25—assuming the Introduksjonsstønad was effective. Increased cash transfers and NIP participation combined could also subsequently lead to improved integration outcomes many years later. This results in a type of quasi-experimental design called a Regression Kink (RK) design (Card et al. 2017).

Our research questions are:

1. What is the effect of increased cash transfers on programme participation?
2. What is the effect of increased cash transfers on integration outcomes up to seven years after settling in Norway?
3. Are there any subgroup effects based on sex, migrant type and so forth?

Our hypotheses are that for eligible migrants:

- H1. Migrants who have greater *potential* Introduksjonsstønad earnings also have higher NIP participation.
- H2. Migrants who have greater *potential* Introduksjonsstønad earnings also have greater labour market participation and higher employment rates several years later. This is expected due to increases in NIP participation in addition to increased benefits received.

This paper reports on the early results of our project: we restrict our sample to migrants who settled in Norway between 2005 and 2013 and focus on labour market outcomes only. We also focus on the validity of the quasi-experimental design since it is scalable to future migrant cohorts and it addresses an evidence gap in Norway for robust policy evaluation. Following increased scrutiny of integration programmes in Norway (IMDI 2019, 4; Djuve et al. 2017) in Norway, it is particularly important to evaluate the effect of this programme. Our scientific approach is also uncommon for economics: before we obtained any data we pre-registered our study protocol including our hypothesis and formal analysis plan (Zhang, Andersen, and Osland 2021). We were completely blind to the results of this study whilst designing our methods and research questions. Our motivation was to avoid the so-called 'con' in econometrics: unscientific practices such as manipulating p-values, post hoc hypothesis generations, not publishing null results, and so forth (Leamer 1983). This paper follows our pre-registration as closely as possible, and we note any deviations from our protocol.

In section 2, we explain the context behind migration in Norway and the policy challenges that NIP was designed to address. We also detail the specific interventions in NIP in particular the details about the Introduksjonsstønad. We also discuss related previous literature about the theory of change behind NIP and other similar programmes as well as the empirical evidence behind these programmes. In section 3, we explain the research design including the data, statistical analysis, design validity and our adherence to our original study protocol. In section 4, we discuss our results and conduct further post hoc exploratory analyses. We conclude in section 5 with a discussion of our early results, its relevance to current Norwegian policy and study limitations. We also indicate our future research plans in this section.

2. Background

Norway has a long history of labour migration. Pre-modern Norway was a peripheral state in Europe that relied on the migration of experts for its technological advancement. With the growth of Norway's oil industry in the 1970s, an upsurge of highly skilled immigrants entered the country. Norway is part of the Nordic Council and citizens from other union members (Iceland, Denmark, Sweden and Finland) can travel, work and reside in Norway. As such, Norway's wealth continues to attract citizens across Scandinavia (Kjeldstadli 2013). Whilst Norway is not a member of the EU, it is part of the European Economic Area (EEA) and the expansion of the EU has continued further migration to Norway from accession countries (particularly Poland and Lithuania). Whilst Norway has a history of receiving refugees, there has been a steep increase in the past two decades because of refugee crises in Somalia, Eritrea, Afghanistan and the Middle East. Since Norway is a welfare state with a strong principle of universalism, there is an ongoing debate in the country regarding the financial implications of migration (Kjeldstadli 2013). Furthermore, since most refugees and asylum seekers originate out of Europe, there are concerns regarding the need to socially integrate migrants into Norwegian society.

Outside of labour migrants and students, the majority of migrants gain legal residence in Norway either through acquiring refugee status or through family-related reasons. Most refugees arrive in Norway and then make an asylum application within a country. After a successful application, these refugees then gain legal permission to reside in Norway. We refer to this group as (former) asylum seekers through the paper. A minority of refugees have their applications resolved outside of Norway. This is usually through special arrangements such as the United Nations Higher Commissioner for Refugees (UNHCR) quota scheme (Tønnessen and Andersen 2019). Regarding family-related migrations, existing family members outside Norway can apply to gain legal residence if a close family member has legal residence in Norway. This is classified as Family Reunification. Furthermore, existing Norwegian residents (migrants or non-migrants) who later married non-Norwegian residents can apply for legal residence for their spouses (hereafter Family Expansion).

Since the 2000s, Norway has actioned several policies and legal changes in response to increasing migration. Proficiency in Norwegian (or Sámi) was introduced as a pre-requisite for citizenship in 2008 and, since 2014, citizenship applicants must also pass an exam about Norwegian society, laws, and history. However, the most important legalisation was the Introduction Act (2003-2020)[3] which is the first Norwegian law explicitly designed to address migrant integration (Ugland 2014). A key aspect of the act was the creation of the Introduction Programme (NIP). NIP is an integration programme aimed at addressing Norwegian language proficiency, vocational skills, and social studies (programme summarised in Box A). NIP participation is mandatory and obligatory for migrants who are either (i) refugees and/or (ii) family reunion migrants (with non-Nordic spouses) between

[3] The Introduction Act was replaced by the Integration act in January 2021. NIP is currently regulated in the new law. The new law regulates the content of NIP and the linked benefit payments, which has been modified in the new Integration act. The Introduction Act itself has also been modified between 2003 and 2020.

18-55, with the need for basic qualifications as defined in the Introduction Act[4] (Norwegian Government 2003). It is expected that participants attend the course full-time. It offers up

Box A: Core interventions in NIP (2003-2020)

- Each participant has a primary context who develops an individual career plan with them
- Language and culture training. Language apprenticeships where students spend time in a workplace.
- Employment training
- Example courses: computer courses, women's group, a traffic course, sewing courses, job-seeking courses, courses directed specifically towards work in the health sector or in catering (Hagelund 2005)
- Up to 2 years of training (up to 3 in exceptional circumstances)
- Holiday and breaks structured similarly to paid employment
- Cash transfers (Introduksjonsstønad) paid per hour of NIP attendance. Paid in arrears and not means-tested
- Exit conditions: participant has accepted work or education; used all available time or kicked out due to other issues (e.g. poor attendance, disruptive behaviour). No punitive element for non-attendance.

to 2 years of training which is tailored to the individual (up to 3 years under exceptional circumstances) and each participant is assigned a caseworker. The programme officially started in September 2004 (Hernes et al. 2019). Whilst several Norwegian municipalities had their own rules and integration programme before NIP, the act itself formalised the rights, obligations, and contents of integration programmes across the country. An overview of how the programme has evolved since 2004 is found in Djuve and Kavli (2019 see table 1).

NIP is delivered by municipalities which in turn receive an integration grant to cover relevant costs. There are 356 municipalities (2020)[5] in Norway, and municipalities vary in size. The largest is Oslo with around 692,000 inhabitants, and the smallest is the island of Utsira with 192 inhabitants (2021). Migrants could reside anywhere in Norway. However, to receive Introduksjonsstønad, migrants must as a rule stay within their settlement municipality. For refugees (including successful asylum seekers), the settlement municipality is designated by the Norwegian government (Directorate for Integration and Diversity, IMDi) whilst each municipality can decide how many refugees they want to host in given year. Potential settlement municipalities are dispersed across Norway to avoid concentrating refugees in certain areas. The government's main consideration is to settle refugees as quickly as possible to begin the integration process(Tønnessen and Andersen

[4] A more detailed specification of immigrants who have or do not have the right and duty to participate in the Introduction program is described in § 2 of the Introduction Act.

[5] The number of municipalities in Norway has change over time mainly due to merges.

2019). Eligible migrants have a right and duty to request entry to NIP after settling into a local municipality. The municipality then must offer a place to eligible participants within three months after application. The duty and responsibility to provide the programme exists for up to five years after eligible participants settle in the municipality (Norwegian Government 2003). Each municipality has the right to test language proficiency beforehand to assess eligibility. The funding for the local settlements comes from the central government and is allocated per settled immigrant to cover the various settlement and integration costs (Norwegian Government 2003).

Participants exit the programme once they have accepted an offer of work or education; used up their available time (a one-year extension is possible under the 2003 Act) or – in rare instances – been kicked out of the programme due to poor attendance / other reasons (Kavli, Hagelund, and Bråthen, M 2007). Most participants leave the programme in these ways (p.12, ibid). Technically participants may simply leave without getting work or education; we know of no other punitive actions other than the denial of Introduction benefits[6]. Participants can interrupt the programme due to illness, leave, or relocation, in a similar manner to employees in Norway. Most eligible migrants who do not participate in NIP do not receive any special benefits instead with two exceptions. First, refugees who do not attend NIP, but who attend primary or secondary education instead, can get a refugee scholarship. Second, refugees who had accrued rights to unemployment or sickness benefits *prior* to starting NIP (e.g. previously employed in Norway), will get allowances from the programme reduced, correspondingly[7]. In theory, since NIP is aimed at those who need basic qualifications, refugees who attend further education should not qualify for NIP. However, in practice, due to a lack of information on refugees' prior education and incentives to participate, refugees in NIP may have higher than basic education[8].

Introduksjonsstønad (Introductory Benefit)

To the best of our knowledge, NIP is one of the most expensive and substantial migrant integration programmes in the world (OECD 2022, 8) and this is in part due to the Introduksjonsstønad. As mentioned earlier, the benefit is calculated using hours of attendance to participants (Hagelund 2005, 677–78). The incentives for programme attendance are not means-tested and the annual benefit for full attendance is double basic national insurance. The rate is only 2/3 for those under 25s – the increase in benefits upon

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[6] See however, a comment on page 23, section 2.2, and remarks to §2 https://www.regjeringen.no/globalassets/upload/bld/ima/lov_om_introduk_ordning_feb_13/246392-rundskriv_h20-05.pdf (read: 28.11.2022]

[7] See §12 in the Introduction law for more details on the coordination of benefits (applicable until 1.1.2021).

[8] A detailed explanation of the content of Introduction Act and who has the right and duty to participate in the overall program which includes benefits is found here : https://www.regjeringen.no/globalassets/upload/bld/ima/lov_om_introduk_ordning_feb_13/246392-rundskriv_h20-05.pdf [read: 25.11.2022]

reaching 25 is substantial (≈€6,430 p.a). The benefit is paid in arrears but the exact schedule of payment is up to municipalities (e.g. monthly, weekly). This means that as soon as a participant turns 25, their next hours of participation are paid at the full benefit rate (2G). People may nonetheless exit the programme due to the opportunity cost of other activities (e.g. work). It is also important that migrants are active participants, in contrast to being passive recipients.

Aside from being an incentive, the Introduksjonsstønad was designed to fill a gap in welfare provision: most eligible migrants have not lived or worked long enough in Norway to qualify for rights-based benefits (e.g. unemployment benefits). Migrants can access means-tested social assistance, but these are meant to be short-term (Hagelund 2005, 674). The Introduksjonsstønad is a type of Conditional Cash Transfer (CCT) designed to simultaneously encourage programme participation and alleviate poverty. CCTs are commonly used to incentive poor households to participate in education or healthcare in developing countries (particularly in Latin America, World Bank 2015). The Introduksjonsstønad, along with similar programmes in other Nordic countries, are uncommon example of CCTs being used to encourage integration in Europe.

Due to its key role in Norwegian policy, the extent of the intervention and its costs, and the novelty of its design (Denmark and Sweden have similar programmes Hernes et al. 2019); knowledge about the effectiveness of NIP and the Introduksjonsstønad is of considerable interest to national and international policy audiences. This project is specifically interested in the CCT element of NIP: does offering higher Introduksjonsstønad payments lead to increased NIP participation and integration outcomes? To measure this effect, we exploit the Introduksjonsstønad rules and Norway's extensive administrative data.

As mentioned before, Introduksjonsstønad payments increase significantly once a participant turns 25 and, under ordinary circumstances, NIP is a two-year programme. Therefore, the expected lifetime earnings from NIP change depending on age. If increasing CCTs were effective, then there would be a gradual increase in programme participation and outcomes for migrants who were settled in Norway between the ages 23 and 25.

The Norwegian Legislation committee consultation document discusses the rationale for giving young people lower benefits (Norwegian Government 2002). These reasons were: 1) younger people have lower expenses and 2) to avoid disincentivising young people from continuing on NIP instead of (ordinary) education (Norwegian Government 2002 s 13.7). Age 25 is used as the cut-off because most young people would finish further education by age 25 (presumably based on statistics from non-migrants). Despite concerns, the Government decided to make age 25 the only consideration in calculating the Introduksjonsstønad to simplify and strengthen the NIP incentive structure. As far as we can tell, the choice of 25 as the exact cut-off age – instead of 23, 24 or 26 – is arbitrary. At the time, the committee consultation could find no other national benefits that age-differentiates around age 25. From desk-based research and other investigations, we could also find no other age-related discontinuities around age 25.

Aside from all the reasons mentioned above, any increase in participation due to the Introduksjonsstønad is of interest domestically. Lower CCTs for young people disadvantages young parents or those with other expenditure pressures which can have

unintended negative consequences such as less engagement with NIP, higher usage of means-tested benefits, and long-term effects on the children of migrants. Some of these issues were raised by the association of municipalities (KS) and Oslo municipality Government (2002). This has led to a minority of municipalities giving higher benefits to those under 25 (Kavli, Hagelund, and Bråthen, M 2007). On the other hand, if increasing benefits does not have any impacts (positive or negative) then there is room for considerable savings by tweaking the Introduksjonsstønad.

As far as we are aware, our approach to evaluating the effects of NIP is novel and in general, clearly addresses an evidence gap in the literature for robust evaluations of migrant integration programmes (OECD 2022).

Related literature

In this subsection, we start with a short overview of the theoretical basis behind programmes like NIP and then focus on empirical studies of NIP and other similar programmes.

Throughout this paper, we describe the Introduksjonsstønad as a CCT however the term is rarely used in other studies (Hernes et al. 2019; Åslund and Engdahl 2018). This is likely because the Introduksjonsstønad predates the rapid growth of CCT programmes in the 2000s: the number of CCTs worldwide increased from 27 in 2008 to 64 by 2014 (World Bank 2015). Theoretically, CCTs are designed to induce positive outcomes through two main effects (Kabeer and Waddington 2015). First, there is a substitution effect where CCTs incentivises participation in NIP over other activities (e.g. low paid work). Assuming that NIP training is effective, greater participation should lead to positive outcomes. Second, there is an income effect whereby the CCT itself allows households to increase expenditure and reduce poverty: both of which may lead to better labour market outcomes years later. For example, new migrants are more likely to be in poverty than the native population and higher benefits might lessen pressures such as childcare, housing, travel and so forth.

Aside from the Introduksjonsstønad, NIP itself is a training programme that focuses on cultural integration and labour market outcomes and these programmes are exist in other Nordic countries (Hernes et al. 2019). The Nordic literature promotes at least two theoretical reasons for rewarding certain types of behaviour in these programmes: paternalism and externalities (Åslund and Engdahl, 2018). The rationale is that migrants do not know their own good, and individual utility will be increased if they invest more in their education. The public good or externality argument is well known when explaining why one should have public provision of education. The social benefits of education are higher than the individual private benefits, so for efficiency reasons, governments should subsidize education. However, generous economic support may weaken the economic incentives to finish education, searching for and taking jobs (Bratsberg, Raaum, and Røed 2020). According to Bratsberg et al. (2020) three mechanisms could potentially weaken the incentives for immigrants. First, the availability of suitable and attractive jobs could be very small given low average qualifications amongst migrants after arrival in a new country. The expected economic gain from employment could be lower for refugees, at least in comparison to prospects of relatively prosperous universal welfare benefits. Finally, non-

pecuniary elements (e.g. time for childcare) could be relevant for immigrants, and consequently, there may not be any positive difference in utility between working and not working, which on average contrasts with the situation for natives (Bratsberg, Raaum, and Røed 2020).

A range of evaluations have studied various elements of NIP but, to our knowledge, there are no studies using experiments– the gold standard for measuring causal impact. Nonetheless, using Norway's rich population register data (see data section), several observational studies have studied the proportion of former NIP participants who are in employment or education years later. We summarise studies that focus on NIP as well as similar interventions in other Nordic countries (primarily Sweden and Denmark). These countries face similar challenges, in particular concerning the potential conflict of maintaining relatively generous universal social welfare programmes, in combination with a high number of arriving refugees over time (Hernes et al. 2019). Although we do summarize some descriptive findings, we will mainly focus on research that intend to measure causal effects. An overview of broader also non-causal studies of migrant integration programmes could be found elsewhere (Djuve and Kavli 2019; Hernes et al. 2019; 2020).

Djuve and Kavli (2019) present empirical research that has studied the outcomes of various components of integration programmes such as NIP. Their summary shows that there seems to be an agreement in the literature that language training is beneficial to integration, at least in the longer run. However, the outcomes vary with the quality of the programmes and with the education level of participants. When it comes to the effect of economic incentives directed towards refugee immigrants and relevant integrational outcomes, the evidence base is weaker. Concerning educational outcomes, results are highly mixed. Some studies show positive, some negative and some insignificant effects on educational achievements of participating students (Åslund and Engdahl 2018).

Rosholm and Vejlin (2010) use a quasi-experimental design and study the reduction in income transfers to refugee immigrants and the impact this may have on employment in Denmark. Refugees get (reduced) economic transfers until they find employment or become self-supported, conditional on participation in the Danish integration programme. The Danish programme is similar to NIP and lasts for three years. Important motivations for their study are that refugee immigrants are perceived as having low employability, and they gain economic incentives shortly after their arrival. In these early phases of the integration process, it could be very difficult for them to act upon the incentives. Shortly after arrival (two years), there is no evidence that the reduction in the transfers show any effect on employment outcomes. After two years, however, there is a large and positive increase in employment status, mainly so for those who are expected to have high employability (men, those aged 30-50 and those with the highest education). However, they question if this effect could be due to the integration programme. For UNCHR refugees, women and the low educated, the sensitivity to economic incentives is the lowest. For women, the incentives may disincentive engagement in the labour market. Åslund and Engdahl (2018) study the introduction of a differentiated bonus which was depending on the completion of language training programmes for refugees within 15 months after arriving in Sweden, and 12 months after the start of the programme. Those finishing the

highest academic levels of the course gained the highest bonuses. On average there were no effects of the bonuses for the studied refugees. In a comparative study, Hernes et al (2019) compare the outcomes of NIP participants to the participants of other integration programmes in Denmark and Sweden. Their descriptive analysis shows that there is a negative relationship between hours attending language courses and employment rates (also see Tronstad 2015).

Since NIP is a statutory duty for many migrants and there is a strong cash incentive, there is almost certainly selection bias affecting NIP eligibility and participation. Most studies do not leverage any design-based strategies to overcome selection bias. One exception is Ugreninov and Turner (2021) who leverages the timing of the 2003 Introduction Act and compares female migrants who arrived shortly 18 months before and after the introduction of NIP. We believe that their causal design is plausible since there is empirical evidence that the two migrant cohorts were comparable and that timing of migration is unrelated to the introduction of NIP. They found that later female cohorts had slightly higher employment rates but found no evidence of an effect on earnings. Compared to our study, their study has a different causal estimand (effect of NIP compared to historical interventions); a more limited sample (women only) and is only relevant to the early year of NIPs (e.g. may reflect early implementation issues). As a trade-off, our design has less statistical power and its own sample limitations (e.g. limited to younger people, see discussion section).

Finally, several qualitative studies have explored and documented the NIP (Hagelund 2005; Nyrud 2019). These studies are invaluable in describing the implementation of the intervention, the experiences of stakeholders (e.g. local government, migrants, NIP staff), and other information. These studies highlight that NIP is full-time and has an emphasis on labour market participation afterwards. This can be a challenge, especially for those with children and women from traditional households who never expected to work outside the home (Nyrud 2019). Whilst NIP training is structured around individual needs, there is a limit on contact with coordinators and individualised training due to resources (Hagelund 2005; Nyrud 2019).

3. Research design

As an overview, we frame our quasi-experimental design as an attempt to emulate an ideal trial (henceforth known as the Target Trial Hernán and Robins 2016). Our causal question of interest is whether increasing CCTs through the Introduksjonsstønad would increase participation in NIP and subsequent integration outcomes. In the fictional Target Trial, we could create multiple treatment arms and vary the CCT amounts across treatment arms. The minimum rate would be 1.33G whilst the maximum is 2G. For later estimation, we create an intended exposure variable T where $T = 0$ if the intended Introduksjonsstønad rate is 1.33G and $T = 1$ if the rate is 2G. We would then randomly assign eligible migrants to treatment arms. For outcomes, we can use administrative data to measure programme participation and labour market participation several years after arrival in Norway. We can estimate the intention to treat effect (ITT) by regressing our outcome Y onto T using linear

regression model. As part of the trial procedure, we would preregister our protocol before data collection. Post data collection, we would conduct tests to check the internal validity of the trial including randomness of treatment allocation, compliance in treatment arms and so forth. For example, whilst the intended benefit rate in a treatment arm might be $T = 0.5$, the actual rate offered could be different due to non-compliance from those implementing the treatment (e.g. municipalities).

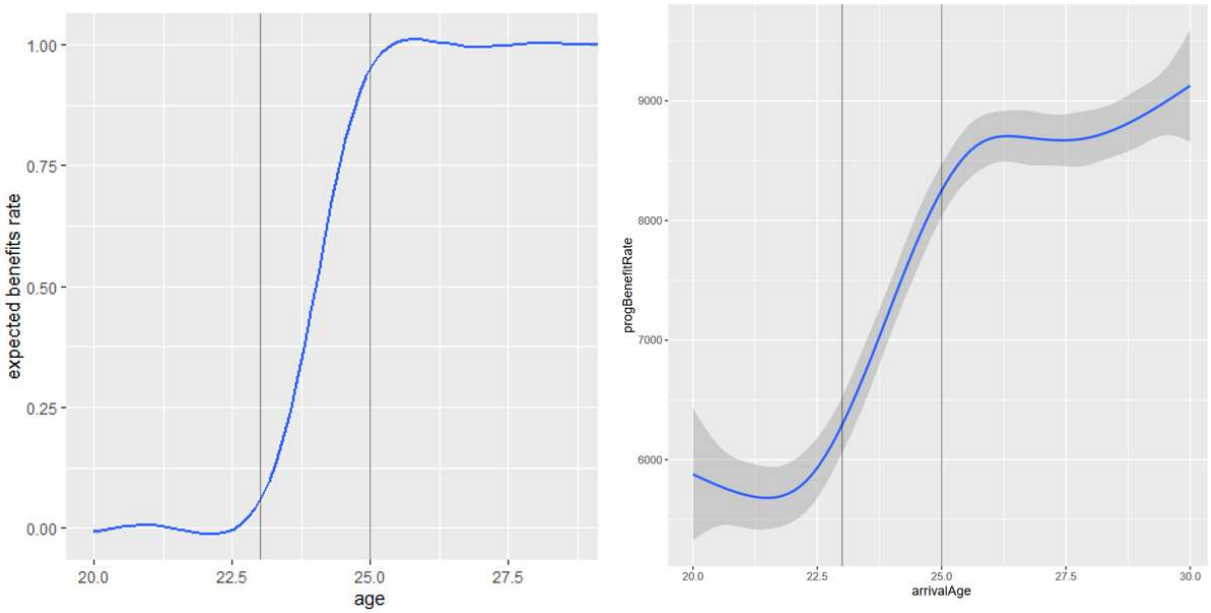


Figure 1: Loess curve (with error ribbon) showing the relationship between age upon legal residence and NIP incentives. (i) Intended expected benefit rate (simulated data based on study protocol) and (ii) actual average monthly benefit rates calculated from migrants in the Norwegian data.

For our quasi-experiment, we can emulate almost every element of the above Target Trial with a few exceptions. Instead of random assignment to treatment arms, the sharp increase in hourly rates at age 25 and NIP's maximum two-year programme length creates a situation where the *expected* potential earnings from the Introduksjonsstønad changes based on migrant age upon legal settlement in Norway. For example, assuming immediate registration in NIP upon gaining legal residence in Norway, migrants aged exactly 25 can expect to earn more money from NIP compared to migrants aged exactly 24. Assuming full-time uninterrupted attendance for two years, the former spends the entirety of the programme earning the highest rate (2G, $T = 1$) whilst the latter would spend the first year earning the lower rate and the second year earning the highest rate (average $T = 0.5$). We conjectured that this would results in a gradual discontinuity or 'kink' in the relationship between age and the expected incentives for NIP participation between ages 23-25 (see Figure 1). In Figure 1, the real relationship between benefit rates (averaged per month) strongly resembles our original conjecture.

The existence of this kink defines our Regression Kink (RK) design. Since there is a substantial kink in expected incentives between 23 and 25 there should also be a kink in participation rates between 23 and 25 if increasing CCTs were effective. Furthermore, CCTs should also improve later integration outcomes as a result of greater NIP participation and through increased CCT itself. This would result in another kink in the relationship between age and later outcomes.

We do not assume that migrants' characteristics and outcomes are unconfounded with the assignment variable: age upon legal settlement in Norway. Instead, our RK design assumes that in the absence of any intervention, the relationship between age and our outcomes is predictable and approximately linear in our eligible sample (Card et al. 2017). We also assume that the age upon settlement in Norway cannot be perfectly manipulated (Lee and Lemieux 2010). For refugees and family migrants, the exact date that migrants are offered legal residence in Norway is subject to administrative and practical considerations that cannot be manipulated (Tønnessen and Andersen 2019). We can empirically test these assumptions (see study validity subsection).

Data

Our data on migrants come from Norwegian administrative data which was available to us by the Norwegian national statistical agency: Statics Norway (SSB). This data is available via protected access to Norwegian researchers. The administrative database contains several data tables with information on labour market outcomes, NIP attendance and so forth. The key linkage table is the National Population Register (NPR, Folkeregisteret) which contains information for everyone (past and present) with a legal residence in Norway. Every individual born in Norway is assigned a unique personal id number whilst migrants are assigned a unique personal id upon legal residence. The personal id number is used in practically all transactions between individuals and government authorities, as well as some private service providers. The personal id number allows data linkage across different administrative records.

We link data from the NPR to NIP participation information, which is also available from SSB. The participation data contains start and end dates for programme participation as well as the total Introduksjonsstønad earned during a year. Our NIP data covers the years 2005 to 2020; NIP data is not available for 2004.

For the baseline period, we use information recorded when migrants gained legal residence in Norway. For the follow-up periods, we use information recorded three, five and seven years after the baseline (or as close as possible to those dates).

For baseline characteristics, we measure sex; age upon settlement; settlement municipality; migrant type (quota refugee/ asylum seeker/ family reunion/ family expansion); marital status; country of origin; household status (couple/ couple with children/ single/ single parent); and the number of children under 18. We did not record previous education data as this is known to be unreliable and missing in many cases.

Our measures of NIP participation and integration are:

- Total Introduksjonsstønad received (three years after legal residence only).
- Total active months of NIP attendance (three years after legal residence only). A migrant is recorded as being 'active' in a month if they attended NIP. The exact hours of attendance are not recorded.
- Labour market outcomes: employment status and total earnings. Self-employed individuals paid through a VAT code are considered employed. Total earnings are calculated based on the entire follow-up year without taking into account hours worked. The earnings include wages as well as income from self-employment.

We do not have direct information on allocated settlement municipality or age upon settlement. For settlement municipality, we use information on where migrants lived when they gained legal residence as a substitute measure. For age, we use age upon gaining legal residence in Norway.

Sample

The eligible sample are migrants who have all the following characteristics:

- are refugees (including former asylum seekers), spouses of refugees or family reunion migrants,
- gained legal residence in Norway between January 2005 and January 2013,
- aged between 23 - 30 upon legal residence
- not from Somalia (see adhere to protocol subsection)

This results in 10,696 eligible migrants of whom 68.9% attended NIP at some point. On average, our sample spent 15.7 months in NIP. The majority of these are asylum seekers (53%) whilst family-related migration makes up about 36%. The biggest migrant groups are those from Eritrea (19.5%) and Iraq (12.7%). Other large migrant groups include those from Afghanistan and Ethiopia. If Somalians were included, they would constitute the biggest migrant group (n = 3,300).

At three years after arrival, the employment rate is 43.0% and the average earned income is 131,000 NOK (€12,890). After seven years, the employment rate rises to 61.0% whilst earned income increases to 207,000 NOK (€20,370, net of inflation).

Variable	Statistic	Full sample	Asylum seeker	Family expansion	Family reunion	Quota refugee
Baseline (N = 10696)						
Ever attended NIP	Never	31.1%	14.5%	79.3%	32.9%	2.8%
	At least once	68.9%	85.5%	20.7%	67.1%	97.2%
Marital status	Married	53.9%	31.9%	94.6%	82.4%	52.8%
	Not married	45.8%	67.9%	4.8%	17.0%	47.1%

	Missing	0.3%	0.2%	0.6%	0.6%	0.1%
Sex	Female	51.6%	36.3%	72.6%	87.4%	56.1%
	Male	48.4%	63.7%	27.4%	12.6%	43.9%
Household status	Couple	19.5%	3.0%	59.2%	25.2%	8.8%
	Couple w/children	24.1%	15.0%	26.2%	57.2%	43.2%
	Single	49.7%	74.0%	13.3%	13.8%	33.4%
	Single parent	6.4%	7.9%	0.9%	3.2%	14.6%
	Missing	0.2%	0.1%	0.4%	0.6%	0.1%
Arrival age	Mean (SD)	26.54 (1.97)	26.68 (1.96)	26.24 (1.95)	26.61 (1.98)	26.47 (1.96)
Arrival year	Mean (SD)	2008.64 (2.23)	2008.92 (2.13)	2008.13 (2.28)	2008.40 (2.29)	2008.63 (2.32)
N. children	Mean (SD)	0.50 (0.91)	0.39 (0.82)	0.35 (0.67)	1.16 (1.23)	0.94 (1.19)
Country (grouped)	Eritrea	19.5%	31.2%	1.2%	12.2%	6.4%
	Iraq	12.7%	10.9%	15.9%	20.9%	7.8%
	Rest of world	67.8%	57.9%	82.9%	66.9%	85.8%
Follow-up (3/5/7 years later) Earned Wages (000s NOK)						
3 years	Mean (SD)	131.28 (149.55)	142.67 (148.43)	137.94 (163.32)	82.78 (131.07)	90.13 (115.64)
5 years	Mean (SD)	199.14 (176.16)	224.21 (176.85)	175.38 (184.02)	136.33 (152.37)	168.66 (141.76)
7 years	Mean (SD)	207.02 (198.09)	228.54 (206.19)	183.04 (192.75)	153.59 (167.10)	189.25 (169.73)
Employed						
3 years	Employed	43.0%	48.0%	40.0%	26.0%	35.0%
5 years	Employed	55.0%	61.0%	46.0%	40.0%	55.0%
7 years	Employed	61.0%	66.0%	53.0%	51.0%	64.0%
Total active NIP months	Mean (SD)	15.70 (12.05)	19.13 (10.12)	4.65 (9.65)	15.55 (12.59)	24.13 (7.94)
Total Introduksjonsstønad (000s NOK)	Mean (SD)	190.91 (150.81)	235.41 (130.68)	54.33 (114.54)	187.25 (155.95)	285.21 (103.30)

Table 1: Sample summary table

Statistical analysis

For the RK design estimator, we start by assuming that the expected outcome ($E(Y)$) has a linear relationship with age (Z) which we centre on age 25 ($Z = 0$ at age 25). Linearity is only an approximation, and we can check if this approximation is correct using descriptive statistics (see Figure 1). We create an exposure variable T which is equal to i) 0 if the age is 23 and under ii) 1 if the age is 25 or over and iii) otherwise is equal to $\frac{25-23}{2}$. As described in the section overview, T represents the anticipated incentives for NIP participation and is equivalent to the intended incentives variable in the Target Trial.

Equation 1:

$$E(Y) = \beta_{0Y} + \beta_{1Y}Z + \beta_{2Y}\mathrm{T}$$

The parameter β_{2Y} indicates the effect of the exposure on Y – this is our estimate of the ITT effect. For example, β_{2Y} is the causal effect of starting the programme at age 25 compared to age K_{-1} (e.g. 23 in this example). We purposefully restrict the sample's age range to limit bias due to model extrapolation based on linear functional form.

We test for statistically significant treatment effects by subgroup using heteroscedasticity-robust F-tests (Wooldridge 2003, 253). We first estimate a full model based on equation 1 but with interactions with subgroup membership G (dummy variable) and every other covariate. We then estimate a restricted model without an interaction term between G and T (i.e. model assumes equal subgroup effects). We use an F-test to check the model fit between the full and restricted models. If the F-test is statistically significant then we reject the null hypothesis that there is no subgroup effect. We can then estimate subgroup effect sizes by estimating equation 1 for individual subgroups (e.g. males only). We focus on migrant subgroups by:

- Sex
- Presence of children. Parents may be more or less sensitive to economic incentives. Parents of dependents may also be eligible for other benefits.
- Migrant type. Different migrant groups have different rights and obligations. These groups also differ in several characteristics and refugees are eligible for alternative grants. Migrant type is a potential confounder if age profiles differ across migrant groups.
- Country of Birth: The demography of migrants changes over time to reflect Norwegian immigration policy and international developments (e.g. mass migration events due to civil war). Treatment effects by country of birth allow us to forecast the efficacy of the Introduction Programme for future migrant cohorts.

Throughout all our analyses, we use heteroscedasticity-robust standard errors. There is very little missing data in our data and where missing cases exist we use case-wise deletion.

Study validity

Our results fundamentally rely on the validity of our design. We first turn to our RK design and investigate the extent to which it eliminates bias (or at least reduces it to negligible levels). First, we test whether or not T is correlated with observed and unobserved confounders. For observed confounders, we can test for a relationship between our T and baseline characteristics observed upon legal residence in Norway using an F-test. For the F test, we compare the following models:

Equation(s) 2:

$$E(T) = \delta_0 + \delta_Z Z$$

$$\mathrm{E(T)} = \delta_0 + \delta_Z Z + \sum \delta_U U$$

Where U is our confounder of interest and we use multiple dummy variables in the case of categorical covariates. The results are shown in Table 2. Out of nine tests, only one is statistically significant at the usual threshold (Number of Children) whilst two others are significant at a higher $p \leq 0.10$ threshold (Sex and Martial Status).

Confounder	df	Statistic	p-value
Sex	1	3.137	0.077
Migrant Type	3	0.292	0.832
Marital status	1	2.761	0.097
Country of origin	26	1.294	0.144
Settlement municipality	324	0.912	0.869
Household status	3	1.220	0.301
Number of children	1	5.547	0.019
Arrival year	7	1.614	0.126
All covariates	366	0.954	0.723

Table 2: F-tests for observed confounders

For statistically significant F-tests, we can further test the strength of confounding by effectively reversing equation 2 and estimating the following regression model:

Equation 3:

$$U = \beta_{0U} + \beta_{1U} Z + \beta_{2U} T$$

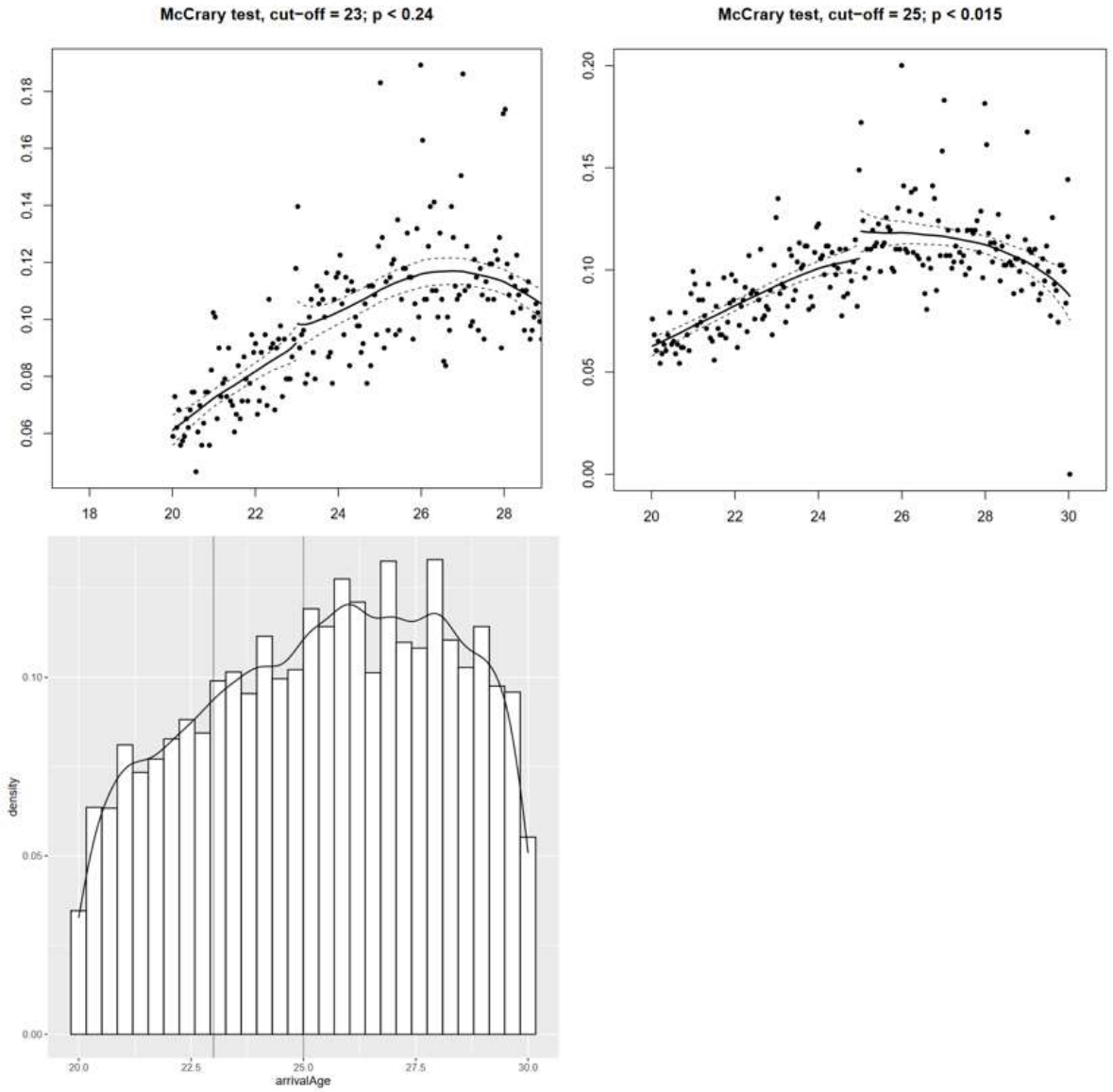
Where β_{2U} is our estimate of interest and indicates the association between our T and confounders (e.g. what is the predicted average difference in U when $T = 0$ and $T = 1$). We found only weak relationships between our T and other baseline characteristics: the treatment indicator is associated with having 0.11 fewer children at baseline; a 4.7 per cent higher chance of being male; and a 4.4 per cent lower chance of being married. We anticipate that this will produce a negligible amount of selection bias. For example, amongst

all baseline characteristics, sex has the strongest relationship with labour market outcomes: male migrants on average earn 20,000 NOK more per year than females (three-year follow-up). Since our treatment indicator is associated with a 4.7 increased chance of being male, this results in regression bias equivalent to 1,970 NOK (€92). Whilst we cannot directly test for unobserved confounders, based on observed confounders there is unlikely to be any severe selection bias in our results.

A critical assumption of RK designs is that the distribution of the running variable (i.e. age upon legal residence) cannot be perfectly manipulated by participants. From personal correspondence with NIP staff, the extra rise in Introduksjonsstønad after age 25 is known to migrants. From desk-based research[9], we have not encountered any evidence of age manipulation. However, we can also indirectly check for age manipulation by looking for discontinuities in the age distribution of migrants. For example, the number of migrants who settled in Norway at 25 should not be unexpectedly high or low. We can test this assumption by looking at a histogram of age and conducting a McCrary test (McCrary (2008)). We do not find any statistically significant discontinuity in the distribution of refugees at age 23. However, the number of migrants aged just under 25 is lower than anticipated and statistically significant (figure 2). This may be related to the issue of inaccuracies in recording birth dates; it is a known issue that refugees and asylum seekers often lack official documentation. Therefore officials are reliant in some cases on information from migrants themselves. This issue is also evidenced by the high numbers of migrants whose day and date of legal residence is the same as their birthdate. A minority of migrants may be motivated to claim to be 25 if they were aware of NIP. Since RK and other discontinuity based designs are robust in the absence of perfect manipulation, we do not believe this will bias our results (also see Lee and Lemieux 2010, 295).

Figure 2: Age distribution. (i) McCrary test at cut-off 23 (top-left), (ii) McCrary test at cut-off 25 (top-right), (iii) histogram of age (bottom-left).

[9] Our main sources were policy documents, government evaluation, peer-reviewed papers, historical web archives (e.g. official websites outlining policy at certain dates), and unpublished masters and PhD theses. These sources were usually in Norwegian. Author MZ translated documents into English using google translate. Translations were further checked by authors LO and HA who are both fluent in Norwegian.



To fact-check our design (i.e. things occurred as we expected), we reviewed documents on NIP and spoke to local municipality staff working with refugees. From past studies, we found evidence that some municipalities offered higher Introduksjonsstønad to under 25s under certain circumstances. This would be equivalent to a failure to comply with the treatment strategy in a Target Trial. To test this, we check the association between age and benefit rates. If municipalities followed the national NIP rules, there should be a roughly linear relationship between CCT rates and ages between 23 - 25. Earlier in this section, we show in figure 1 that the kink relationship is almost perfectly linear, as predicted. Whilst we do not doubt that there may be some exceptions to the Introduksjonsstønad rules based on previous research (Kavli, Hagelund, and Bråthen, M 2007), these exceptions are not frequent enough to affect our results.

Adherence to study protocol

A study pre-registration and protocol was published before the data was received in November 2021 (Zhang, Andersen, and Osland 2021). In this working paper, we made the following changes:

- Restricted the sample to those aged 23 - 30. Initially, we restricted the sample to those aged 20 - 30 because we expected confounders, such as the number of dependents, to change linearly between ages 20 - and 30. This was based on speculation and what we thought was reasonable at the time. Instead, we found that the average number of children baseline is static between ages 20 – 22 and then increases linearly from ages 22-30 onwards. As such, we reduced our age range to 23 - 30; this contingency was anticipated in the study protocol.
- Excluded Somalians from our eligible sample. This was because the age distribution of Somalians between 20-30 was different from the rest of the migrant sample: Somalians were more likely to be 25 and older.
- Age upon legal residence is used instead of the age under which participants entered NIP. This is due to an oversight in the protocol; our design isn't logically possible with the NIP entry age since not all migrants are enrolled in NIP.
- A variable named tenure status was changed from a binary status ('cohabiting' or not) to a more detailed household status variable.

Our per-protocol results (e.g. including Somalians and 20-30 year olds) are included in the results section of this paper. We find no substantive difference in our results due to deviations from the study protocol. Other elements such as our estimator, design, hypotheses and forth remain unaltered. In addition, our early results only use a subset of our data to address some of the research questions specified in our protocol.

4. Results

Figure 3: Relationship between age (20 – 30) and selected outcomes (loess curve (with error ribbon). Vertical lines at ages 23 and 25. Outcomes: (i) total active months in NIP (left); (iii) age and earnings (7 years follow-up, right)

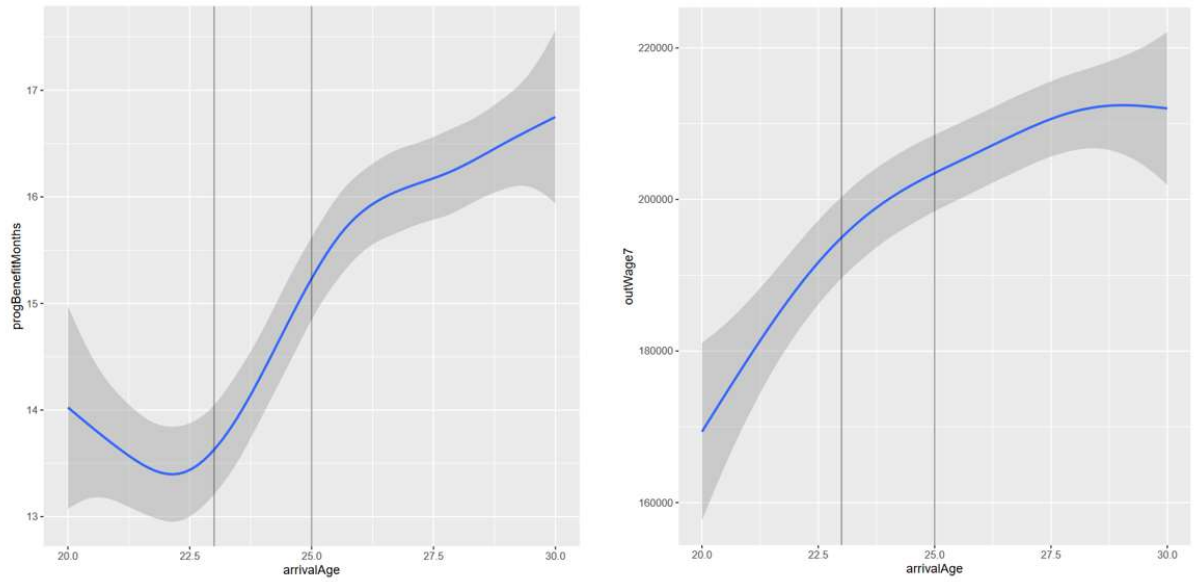


Figure 3: Relationship between age (20 – 30) and selected outcomes (loess curve (with error ribbon). Vertical lines at ages 23 and 25. Outcomes: (i) total active months in NIP (left); (iii) age and earnings (7 years follow-up, right)

Our overall results are succinctly explained in figure 3: we observe a kink in participation between ages 23-25. However, we do not observe any signs of improved outcomes at these ages in the follow-up periods (e.g. no kink when participants initially aged 20-30 become 30-40). Using statistical models to estimate the effect sizes, the ITT effect (β_{2Y}) on total Introduksjonsstønad earned (50,000 NOK or approximately €4,920, $p < 0.001$). By the follow-up period, the ITT effect results in 1.8 more months of NIP participation ($p < 0.01$). There is an increase of 4.8% participation in NIP but this effect is not statistically significant at the usual threshold. Whilst these effects are statistically significant, the effect sizes are modest relative to the population variance. For example, the ITT effect is equivalent to a 0.15 standard deviation (sd) increase in months of attendance (based on Table 1). They are also not particularly strong especially given the substantial increase in cost as a result of the Introduksjonsstønad.

We find no statistically significant ITT effect on integration outcomes during any follow-up period. The effect sizes for earnings and employment rate are effectively zero. Aside from not being statistically significant, these effects are not particularly substantial considering NIP's flagship role in Norwegian integration policy.

Our statistical tests are not underpowered: for example, we can detect an effect size of 20,400 NOK or €2,007 on annual earnings seven years later. This is equivalent to 0.10 sd which is equivalent to a small effect size in the trial literature. Our study power in this working paper is only marginally worse than our anticipated power (0.06sd) based on our original protocol.

Outcome	Estimates (SE)			
	(1) Main	(2) Main + covariates	(3) Protocol	(4) 23 - 27 no integer
Any NIP attendance	0.048 (0.025)	0.028 (0.035)	0.073 (0.016) ***	0.025 (0.042)
Months of attendance	1.808 (0.637) **	1.099 (0.975)	2.104 (0.429) ***	1.596 (1.096)
Total Intro. Benefits	49.748 (7.621) ***	42.414 (11.772) ***	64.497 (5.305) ***	50.563 (13.400) ***
Wages (3 years)	4.991 (7.981)	-15.175 (15.114)	-3.057 (5.145)	-2.963 (14.045)
Wages (5 years)	4.594 (9.012)	-16.969 (17.611)	-4.052 (6.216)	0.434 (16.109)
Wages (7 years)	-5.445 (10.226)	-32.676 (17.906)	-19.506 (6.977) **	-7.250 (17.751)
Employed (3 years)	0.043 (0.026)	-0.008 (0.05)	-0.002 (0.018)	0.032 (0.044)
Employed (5 years)	0.005 (0.026)	-0.031 (0.051)	0 (0.018)	0.008 (0.045)
Employed (7 years)	0.031 (0.026)	0.011 (0.05)	0.012 (0.018)	0.04 (0.044)
Additional covariates	No	Yes	No	No
N [min, max]	[10680,10696]	[10625,10641]	[18343,18373]	[5926,5934]

Table 3: Table of ITT effects on outcome

We check the sensitivity of our results to observed confounding and alternative model specification. Our results are in table 3. For model 2, we re-estimate our main RK estimator and add additional covariates to the regression model. These covariates were previously used in our checks for confounding (see table 2). We also include a polynomial term for age to account for nonlinearity. For model 3, we use our original protocol sample which includes those aged 20-30 and migrants from Somalia (i.e. to check for sensitivity due to deviance from protocol). For model 4, we shrink our age bandwidth to those between 23-27 and filter out migrants whose recorded birth date is their date of arrival (i.e. migrants with suspected missing birth records). All our effect sizes are substantively the same but statistical significance will fluctuate due to changing statistical power. This is particularly true for model 2 where the age polynomial term greatly reduces power. Using our original protocol sample, the precision of our estimates improve which results in statistically significant increases in participation (measured in participation months and any attendance at all) and decreased earnings at seven years follow-up (19,500 NOK , equivalent to €1,900). None of the alternative estimators differ substantively from our main estimator.

Subgroup	Outcome	df	F-statistic	p-value
Sex	Prog. active months	1	0.173	0.678
	Wages (7 years later)	1	1.526	0.217
Migrant type	Prog. active months	3	0.646	0.586
	Wages (7 years later)	3	0.780	0.505
Country (top 3 + rest of world)	Prog. active months	2	0.097	0.907
	Wages (7 years later)	2	1.029	0.358

Children or no child	Prog. active months	1	0.062	0.803
	Wages (7 years later)	1	0.451	0.502

Table 4: F-test for subgroup effects

For subgroup effects, we look at months of active programme attendance and earnings (seven years follow-up) in table 4. We do not find any heterogeneity in the ITT effect.

5. Discussion and concluding remarks

Based on our early results, our evidence has implications for policy in Norway and other countries with similar CCT programmes for migrants. Whilst we find a statistically significant effect for programme attendance, these effects are not substantial compared to the cost of the Introduksjonsstønad. From a policy perspective, this is important given ongoing concerns in Norway about the cost of the Introduksjonsstønad and its potential to disincentivise engagement in other forms of education and work (Norwegian Government 2003). Due to these concerns, the Introduction Act was further revised in 2020 and the Introduksjonsstønad was reduced for those under 25 and living with a parent (1/3 of the rate paid to 25 and over, Norwegian Government 2020). In addition, academics, municipalities and other public bodies have raised the potential for reduced Introduksjonsstønad to harm integration. However, we do not find any effects on labour market outcomes at the three, five and seven-year follow-up.

The discovery of our quasi-experiment is beneficial and timely for evidence-based policy in Norway. First, our evidence covers recent migrant cohorts who arrived up until 2016 whilst the only other quasi-experimental study can only examine migrant cohorts in the early years of NIPs (Ugreninov and Turner 2021). Second, the age 25 rule is still in effect with only minor revisions and our design will continue to be valid for more recent cohorts with little adjustment. For future studies, spouse-related migrations must be omitted due to a change in migration law in 2016. Finally, continuing to study and exploit this quasi-experiment using Norwegian administrative data is extremely cost-effective. Our study cost is a fraction of the cost of a randomised control trial, and we will make our code available to reduce costs for future studies. Secondary data analysis also has other advantages such as ethical approval and maturation time (e.g. if we started a trial in 2023, we'd collect the earliest results by 2026).

On the other hand, whilst we believe quasi-experimental have advantages, there is no substitute for actual experiments where these are possible and cost-effective. In the Norwegian context, there are powers under Norwegian law to suspend normal NIP operations to experiment with alternative interventions. We believe a future experiment should replicate the finding from this paper and test the effects of higher Introduksjonsstønad for those under 25. This was mentioned as a policy option in the earliest consultations for the Introduction Act and is in effect in some municipalities already (Kavli, Hagelund, and Bråthen, M 2007).

There may be several reasons why we do not find any effects on integration outcomes. First, the additional 1.8 months of NIP attendance may not yield any additional benefits given that average participation is already 15.7 months. Second, the additional Introduksjonsstønad earnings may have an effect that cancels any benefits from NIP itself. For example, extra benefits could disincentivise future labour market participation whilst NIPs training components improves future labour market outcomes. This is a post hoc explanation and we did not find this explanation plausible when we designed our study protocol. Our follow-up periods (up to seven years) occur years after the Introduksjonsstønad ends. Based on previous studies and the original rationale behind NIPs itself, we expected both NIP training and the Introduksjonsstønad to individually have positive effects on later integration outcomes. However, we have only examined labour market outcomes in this paper and increased earnings from the Introduksjonsstønad may have positive effects on other outcomes or the children of migrants (OECD 2022).

There are some limitations to our design. Due to our design, we can only identify the effect of the Introduksjonsstønad on young people (23 -30). However, a substantial proportion of migrants fall within or near this age range. We do not calculate the NIP effect for Somalians, who are a considerable proportion of the migrant community and, due to lower education, are more likely to participate in NIP. We also calculate the effects of increasing benefits, which is only one component of NIP, on NIP attendance and other integration outcomes. We cannot isolate the impact of individual NIP components. We have looked at primary integration outcomes as defined used by Statistics Norway. However, the increased programme benefits may impact other unmeasured outcomes such as Norwegian language fluency. In this paper, we also concentrate on the migrant cohorts that gained legal residence between January 2005 – January 2013 due to a change in recording NIP participation that occurred from 2017 onwards. We are working to harmonise this data. Furthermore, a key criticism of decreased benefits for under 25s is the potential negative impact this may have on children with young parents. This is a potential avenue for future research.

Finally, we intend to expand these results according to our study protocol to look at a larger sample, more outcomes and more research questions (Zhang, Andersen, and Osland 2021). This includes non-labour market outcomes, such as educational attainment and language fluency, and testing whether there are heterogeneous effects by municipality (since the delivery of NIP and the Introduksjonsstønad can vary across regions). There is also another age-cut off where those over 55 have a right to receive training under NIP but not an obligation. This has the potential for another quasi-experiment measuring the effect of NIP on an older migrant sample. Compared to our initial expectations, the current quasi-experimental design so far is not quite perfect: there is some evidence of minor confounding. Based on our results, we would describe the design as not perfect (compared to initial expectation) but good enough: our sensitivity analysis shows that our substantive results are not affected by any observed confounding. We've made minor adjustments to the original study protocol, many of which were originally anticipated, but otherwise left our original hypotheses and analysis plans unchanged. For the benefit of other researchers, we are determined to show how our initial research design worked out. Results from planned studies are often not reported because the original quasi-experimental design did

not work, null findings are never published, or the study is changed after seeing the results. This leads to selection bias in publication and diminishes the scientific value of economic research (Leamer 1983). In this working paper, we have focussed strongly on reducing bias at the expense of precision. For practical purposes (i.e. cost-benefit analyses) it is important to consider the bias-variance trade-off and we may explore estimators that increase precision at the bias.

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