MONASH University

MONASH BUSINESS SCHOOL

CENTRE FOR HEALTH ECONOMICS

WORKING PAPERS

Welfare Reform and Migrant's Long-term Labor Market Integration

Discussion Paper no. 2023-05

Johannes Kunz and Anna Zhu

Keywords: Welfare reform, labor market outcomes, migration, job quality

JEL Classification: E64, I30, J60

Johannes Kunz: Monash University (email: johannes.kunz@monash.edu); Anna Zhu: RMIT (email: anna.zhu@rmit.edu.au).

Welfare Reform and Migrant's Long-term Labor Market Integration*

Johannes S. Kunz
Monash University

Anna Zhu
RMIT University, IZA

June 28, 2023

Abstract

We study the effect of reducing welfare assistance on migrants' long-term integration in Australia. The policy postponed a migrant's eligibility for benefits during their first two years in the country. It mainly affected mothers and was announced after their arrival. Using a regression discontinuity design and 21 years of administrative welfare data, we find significant reductions in welfare receipt, where the gap widened over time, and stabilized in the long run. Benefit receipt amounts reduced by 28%, and time-on-benefits by 19%, particularly in the unemployment and disability categories. We observe larger treatment effects for mothers from disadvantaged backgrounds.

Keywords: Welfare reform, labor market outcomes, migration, job quality.
JEL classification: E64, I30, J60

**Addresses for correspondence*: Kunz: Monash University, 900 Dandenong Road, 3145 Caulfield East, Vic Australia, johannes.kunz@monash.edu and Zhu: RMIT University, Level 10, Building 80, 445 Swanston Street, 3000 Melbourne, Vic, Australia, anna.zhu@rmit.edu.au
Acknowledgments: We thank Daniel Auer, Melisa Bubonya, Christian Dustmann, Jennifer Hunt, Tim Moore, David Johnston, Umair Khalil, Claudio Labanca, and conference participants of Alife 2021 (Canberra), as well as seminar participants at Melbourne Institute, Monash University, University of Western Australia, UCL for thoughtful comments. We also thank Tessa Loriggio for her excellent Research Assistance.
The paper uses administrative data from the Data On Multiple INdividual Occurrences" (DOMINO) dataset provided by the Australian Government Department of Social Services (DSS). The findings and views reported in this paper are those of the authors alone and should not be attributed to DSS.
Zhu acknowledges the support of the Australian Research Council Linkage Project (LP170100472).
Neither author has any conflict of interest to declare.

1 Introduction

Governments in developed counties are taking an increasingly harder stance against migrants receiving welfare benefits. As a result, welfare benefits are highly restricted under most migration programs around the world (Ruhs, 2013). In particular, many countries have policies that limit the access to welfare for migrants in the first few years after they arrive in the host country. One aim of limiting migrants' welfare benefits early on is to encourage earlier employment uptake with the view that this may improve economic assimilation in the long run.[1] Inherent in this argument is that such policies can create a culture against the use of welfare, which may limit subsequent use (Dahl, Løken and Mogstad, 2014). Alternatively, policies that postpone the timing of welfare receipt may simply intensify receipt later, changing the timing but not the extent to which a migrant relies on welfare. To date, almost no evidence exists on the long-run implications of policies that postpone the eligibility of welfare-benefits for migrants.

In this paper, we examine the validity of the long-run assimilation claim by assessing how migrant mothers fare for up to two decades after arrival in the host country as a result of a stricter welfare policy. Specifically, we analyze an Australian policy that reduced potential welfare benefits by up to roughly 50% for the two years post-arrival for all mothers arriving on or after 1 April 1996. After the first two years, these mothers were eligible for the same types and amounts of benefits as those arriving before this date. Notably, the policy change was announced ex-post and applied retrospectively, i.e., after migrants arrived in Australia, eliminating any selective sorting around the policy cut-off date. This, coupled with

[1] Other justifications are based on fairness (lack of prior contribution to the welfare state) and to shape the type of migrants a country attracts (Agersnap, Jensen and Kleven, 2020). However, finding employment (or matching with a suitable job) upon arrival in a host country may be challenging for migrants, who often need access to job referral networks and country-specific signaling credentials. In particular, migrants who arrive with children may face additional vulnerabilities if they are restricted in their time availability to (look for) work. As such, the work incentives could lead to unintended consequences for migrants' well-being and may have knock-on effects on their long-term employment and job-quality outcomes.

administrative data on the universe of welfare recipients and exact arrival information, allows us to exploit the discontinuity based on the arrival date within a Regression Discontinuity Design (RDD) framework. We assess how the initial benefit reduction impacted migrants' long-run welfare receipt and employment assimilation patterns (over much of the focal individual's working life and over a 21-year window).

Australia is an ideal setting to study the long-run assimilation claim, as it is a country with high migration rates. In fact, it has the third highest intake per capita, only lower than Switzerland and Luxembourg, which are, however, much smaller countries (Brell and Dustmann, 2019). Further, the migrant intake in the year of the policy - 1996 - was prior to the boom in skilled-based migration, making the migrant intake compositions similar to those of other Anglo-Saxon countries today. It is important to note that Australia's welfare system follows the *Anglo-Saxon model*, which contrasts with other systems, such as the *Nordic model*. Thus the evidence that we provide here complements the literature on the causal impact of welfare reforms on migrants, which has almost exclusively focused on the Scandinavian setting - a setting characterized by large welfare states and relatively small migrant populations.[2]

We begin by validating our RDD approach. Specifically, we show that: (1) the pre- and post-arrival migrants are empirically indistinguishable based on observable characteristics; (2) there is no measurable change in the number of arrivals around the cut-off; and (3) there is unlikely to be compositional differences between the treated and control groups (through exits from the welfare system or through return migration). Our results point to significant long-run impacts from the reform: treatment and control groups' welfare receipt patterns differ in the length of time on welfare (by 10 months from a mean of 48 months – a 19% reduction –

[2]It is important to note for context, that in the Australian policy setting, migrants remained eligible for a small amount of welfare payments (roughly 50% of the pre-refrom benefits) and, relative to the US, were fully eligible for medical coverage.

over 15 years) and by the amount of receipt (A$15,000 from A$54,000, or 28%) over the same period. The reform reduced benefit access in the first two years of arrival by 18 months which subsequently reduced long-term welfare receipt - occurring at a margin where there was low dependence on welfare overall. Adjustments in unemployment and disability benefits drive these differences and correspond mainly to the intensive (amount and length of receipt) rather than the extensive (ever being on welfare) margin.

The policy design results in a disparity between the treated group mothers and the control group mothers regarding total months of access to welfare payments, with the former experiencing lower levels throughout the analysis window. Notably, the pronounced concentration of restricted welfare access within the initial two years after arrival is a key factor contributing to the long-term impact. For example, the treatment of making migrant mothers wait for welfare upon arrival impacts the long-run welfare patterns independent of (or holding constant) the total potential receipt length. Thus, we conclude that the dynamics of welfare restrictions in the initial period after arrival matter rather than the total access to welfare reduction. The treatment effects grow over time in both payment categories (disability and unemployment benefits) and stabilize in the long run in all types. A similar picture emerges for job quality, where benefits appear to materialize in the medium run (11 years after arrival), consistent with a stepping-stone interpretation and different from the optimal unemployment insurance relationships of extended benefits and better job quality (Nekoei and Weber, 2017). Lastly, we document heterogeneity in the treatment effects by pre-migration characteristics. We find all migrant mothers reduce their long-run welfare receipt due to the reform. However, the reductions are most pronounced among the most disadvantaged mothers.

The potential long-term consequences of waiting times for welfare benefits to migrants have so far been the subject of few direct empirical investigations. A small number of papers examine the impact of stricter welfare policies on refugees (i.e. Andersen, Dustmann and Landersø, 2019; Foged, Hasager and Peri, 2022). We contribute to this literature on four fronts. First, we examine the effects of temporary unavailability of welfare while these other papers look at a permanent reduction in welfare payments to refugees. Second, we assess a broader group than refugees but who are also a potentially vulnerable population: migrant mothers with children. Third, we examine impacts over a longer time frame post arrival (up to two decades after) in terms of migrants' welfare benefit trajectories. Fourth, our analysis of migrants' long-term employment, earnings, and job-quality outcomes is unique but an essential step toward understanding how migrants' long-term economic success may depend on the policies they face when they first migrate.

Another unique aspect of our paper is that it quantifies the causal impact of a welfare design element that is receiving renewed attention in the broader welfare evaluation literature: the timing of the intervention. Economists have long debated the merits of expansions and contractions in the unemployment insurance program (see Lalive 2007, 2008 and Nekoei and Weber 2017 for a detailed discussion). The longer the duration of support provided to an individual after they lose their job, the higher the chance they will reduce their effort to search for a new job because the hardship of unemployment lessens. More generous support will likely erode human capital as people stay out of the workforce longer. In other words, the timing and duration of welfare provisions are important because duration dependence may apply. Empirical findings show that shifting more support forward (and reducing the amount of support later, holding constant the total amount of support) can be beneficial (Lindner and Reizer, 2020, in Hungary), as is the case with shifting the timing of intense mandated job search periods forward (Bolhaar, Ketel and van Der Klaauw, 2019, in the Netherlands), as well as the provision re-employment bonuses in the initial stages of unemployment (Ahn, 2018, in South Korea). Yet, in other circumstances, Kolsrud et al. (2018, in Sweden) find the moral hazard cost of benefits is larger when paid earlier in the spell.

We depart from this literature by examining how the timing of receipt matters for migrants. By contrast, the existing literature has almost exclusively assessed the impact on native or resident males that have held jobs for an extended period. More evidence is needed on diverse populations' experiences of such reforms. New migrants likely differ from newly unemployed non-migrants in one crucial dimension, and that is related to their country-specific human capital. Migrants often arrive with little to no experience in the host country's labor market or networks. In contrast, the newly unemployed hold stock of such human capital upon job loss. Low (or minimal) initial stock of country-specific human capital means that new migrants may likely start with a job from the lower end of the skills distribution. Even if they wait longer to enter the labor market – and engage in a longer search buoyed by welfare benefits – it may be unlikely to procure better job matches for lower-skilled migrants. Thus, the benefits of starting work earlier may help migrants accumulate more skills over time and form a stronger attachment to the host country's labor market. This means that it may work to improve future employment prospects and outweigh the benefit of a longer potential search period. The counter argument is that cutting benefits in a sensitive time period, such as upon arrival to a host country, risks pushing vulnerable migrant subgroups, such as female migrants with young children and especially those with low previous labor market attachment, into poverty.[3] Furthermore, the initial period influences migrants' expectations and human capital investments (Adda, Dustmann and Görlach, 2022). We contribute to this nascent literature by examining a configuration in the timing of welfare support that is the reverse of the front-loading profile discussed above. The 1996 Australian policy meant that a group of new migrants was given less support earlier on – upon migration – compared to those who migrated to Australia before the policy was implemented. Thus, we ask whether delaying benefit access and reducing support affects migrants' likelihood of success

[3] Filomena and Picchio (2021) show, in a meta-study that assesses whether entry-level jobs are a stepping-stone or a dead-end, that the evidence for the latter seems slightly greater. But in general, the evidence is somewhat mixed and context-specific.

in the long term. Due to the aforementioned reasons, this is an empirical question that warrants a unique focus as new migrants may respond differently to the newly unemployed.

Related Literature

Our study relates, first, to the literature on optimal welfare provisions and the timing of interventions. Most studies focus on the length and amount of welfare and how they affect the speed of re-employment and job quality (Nekoei and Weber, 2017). We add to this literature by presenting evidence on the effect of waiting periods, a distinct feature that governs access to welfare. The design choices in the time profiles of welfare provision and time-dependent policy designs include, among others, front-loading i.e. keeping the benefit amount the same but paying out earlier (Lindner and Reizer, 2020), early job search periods (Bolhaar, Ketel and van Der Klaauw, 2019), or re-employment bonuses paid out at the beginning of the period (Ahn, 2018). Generally, these find that the initial unemployment period is highly influential. For example, Bolhaar, Ketel and van Der Klaauw (2019) assess mandatory job search periods, which is a waiting period (of 4 months in their case, in the Netherlands) where the applicant is encouraged to search for work, substantially reducing benefit take-up. Consistent with this, Krueger (1990) finds that waiting periods reduce disability benefits receipt, based on survey data in the US.

Extending this line of inquiry, we assess how waiting periods may affect labor market entrants. This is important because their behavioral response will likely differ from those previously well-attached to the labor force. For example, recent evidence suggests that job search strategies are a crucial determinant of labor market success (Marinescu and Skandalis, 2021; DellaVigna et al., 2022), and that migrants' job search strategies might differ from natives (Frijters, Shields and Price, 2005). Migrant mothers with children are a sub-population with low labor force participation. Thus, we are likely to provide an upper bound on the effectiveness of such policies. In addition, it raises the concern that the policy can heighten poverty since the intended improvement in employment is less likely to eventuate for those with weak prior labor force attachment. Thus, timing interventions correctly might be even more critical for this group of labor market entrants. We further contribute to this literature by assessing effects over a longer period and assessing more detailed measures of job quality than the prior literature (for example, we look at the stability of employment and the contract type, such as non-zero hour and continuous employment contracts).

By focusing on migrants, our study also contributes to the large literature on the assimilation of migrants over the long run. Recent surveys are provided by Brell, Dustmann and Preston (2020) for refugees in general, Dorn and Zweimüller (2021) for migrants in Europe, and Brell and Dustmann (2019) for migrants in Australia. However, policy-based evidence in welfare eligibility is still largely lacking.[4] We add to this by assessing the very long-run outcomes and by focusing on a sub-population with very low labor force attachment. In this respect, Andersen, Dustmann and Landersø (2019) assessment of refugee assistance is the closest study to our setting, which uses the same design and a similar population. In their words, "hardly any analysis exists that studies the immediate and longer-term consequences of such reforms on migrants and their families".[5] They find that women are likelier to drop out of the labor market due to the household means testing for refugees. In comparison, our study captures all migrants and specifically focuses on women. Critically, the policy we study was announced ex-

[4] Arendt, Dustmann and Ku (2022) summarise findings on welfare access for refugees and how they might interact with local skill demand. Overall, their effects appear short-lived (they fade within five years in low-demand settings).

[5] Outside of Scandinavian countries, there is very little administrative data evidence. Scandinavian evidence is highly valuable, yet they might not be as representative of other countries due to their large welfare state, heavily colony-biased migration, and their migrants' relative size. Migration to a country with an international language environment, a relatively highly-skilled labor force, and a very large migration intake can provide alternative benchmarks for international comparisons (Beerli, Indergand and Kunz, 2023).

post. Thus, we see our results as highly complementary to their analysis. Finally, our policy is time-dependent, affecting the initial period after arrival and restoring welfare after the waiting period.

Our results comment on welfare benefits for families more generally. For example, in the US, Aid to Families with Dependent Children was abolished in 1996 and replaced with Temporary Assistance for Needy Families (Hartley, Lamarche and Ziliak, 2022), which predominantly affected single mothers and tended to restrict program access overall through new payment caps, time limits, sanctions, and work requirements. Blank (2006) summarises the findings: As a result of the welfare reform "[...] a high share of single mothers with younger children, when given some support for job searches, seem able to find and retain some employment. The rate for welfare leaving and job finding was much higher than I would have predicted, with real gains in income." Earlier literature assessing labor supply effects of means-tested transfer payments include (i.e., Eissa and Liebman, 1996; Eissa and Hoynes, 2004; Moffitt, 2002; Saez, 2002) among mothers (Mogstad and Pronzato, 2012) and migrants (Borjas and Hilton, 1996).

Finally, a smaller but growing literature assesses the role of peer effects (Nicoletti, Salvanes and Tominey, 2018; Bursztyn, González and Yanagizawa-Drott, 2020) and cultural values on maternal labor supply (Boelmann, Raute and Schönberg, 2021), we address these and various other channels in our heterogeneity analyses.

2 Institutional setting, estimation, and data

2.1 Migrant welfare and policy changes in Australia

Like in many other countries, the 1990s policy landscape in Australia was characterized by tightening welfare access. Australia's reform philosophy was similar to the US's 1996 Welfare Reform – the move from Aid to Families with Dependent Children to the Temporary Assistance for Needy Families scheme – (extensively summarized in, for example, Hartley, Lamarche and Ziliak, 2022; Blank, 2002; Ziliak, 2015). Post-1990s welfare was meant to be means-tested, restricted, and reduced for migrants and the overall population. The focus shifted from supporting (single) mothers to stay at home with their children to encouraging employment and preventing welfare traps.

Minimum waiting-period policies were motivated by a growing perception of migrants' reliance on welfare payments. Birrell and Evans (1996) estimated that the expenditure paid out benefits for 1996 was around A$133 million. To counteract this concern and to promote greater self-sufficiency among new migrants, governments introduced a reform restricting migrants' access to welfare payments. Throughout the 1990s, migrants' access to this payment was substantially reduced and restricted. By the end of the 1990s, newly arrived migrants had to satisfy a two-year waiting period before they became eligible for relatively generous payment schemes. In Table 1, we present the timeline of restrictions.

Table 1: Policy time line

1 January 1993	26 week waiting period was introduced for selected payments (Parenting Allowance and Unemployment Benefits)
23 May 1996	**104 weeks for Parenting Allowance, for those arriving after 1 April 1996**
4 March 1997	104 weeks were introduced for a wider range of payments
1 January 2019	the NARWP was further extended to 208 weeks for various working age payments and concession cards; and new waiting periods of 208[6] weeks, 104 weeks[7] and 52 weeks[8] were introduced for a range of other payments

The Newly Arrived Resident's Waiting Period (NARWP) policy,[9] was implemented in two rounds: the first was implemented in 1993[10] and the second, which is the reform of interest for this paper, was implemented in 1996. This reform

[9]The NARWP is a period migrants must have been an Australian resident and in Australia before receiving certain payments (DSS 2021).

[10]The 1993 NARWP policy introduced a waiting time of six months for new migrants wanting to receive Parenting Allowance or Unemployment benefits.

extended the waiting period for newly arrived migrants from six months to two years (see Appendix A for a full description, and Appendix Figure B.1.1 for a visualization).

We focus on welfare policy directed at low-income migrants who were primary carers of children and who were eligible to receive *Parenting Allowances* upon arrival in Australia. The policy affected prospective recipients of Parenting Allowance. As a result, migrants' arrival time affected the type and timing of benefit receipt. Specifically, migrants who had arrived after 1 April 1996 were eligible to receive roughly 50% lower payments six months after arrival; migrants who had arrived between 1 April 1996 and 4 March 1997 could only receive Unemployment Benefits six months after arrival and were eligible to receive Parenting Allowance (or continue receiving Unemployment benefits) two years after arrival.[11] Migrants who had arrived after 4 March 1997 were neither eligible for Parenting Allowance nor Unemployment Benefits but were eligible for both payments two years after arrival. The reform was further extended to four years in 2019, showing the timeliness of the issue assessed here.

The policy changed three major welfare design features. The largest and most important change was the size of the benefit as it essentially halved the maximum benefit amount.[12] Figure 1 visualizes the policy change by date of arrival, where the top panel depicts the maximum benefit before the policy change and the bottom panel depicts it after the policy change. The maximum loss of benefits amounts to A$150 per week. For comparison, in 1996-97 the average weekly income of couple families was A$890, A$432 for one-parent families, and A$391 for single persons. The relative size of the reduction was, therefore, considerable, totaling roughly A$10,800 over the 18-month extended waiting period. It is im-

[11]Parenting Allowance is a more desirable payment than Unemployment Benefit for prospective recipients because they are not required to fulfill any activity requirements as discussed below.

[12]`https://www.abs.gov.au/AUSSTATS/abs@.nsf/mediareleasesbyReleaseDate/C68FDE8DCA48ABCECA2568A9001362A0?OpenDocument`.

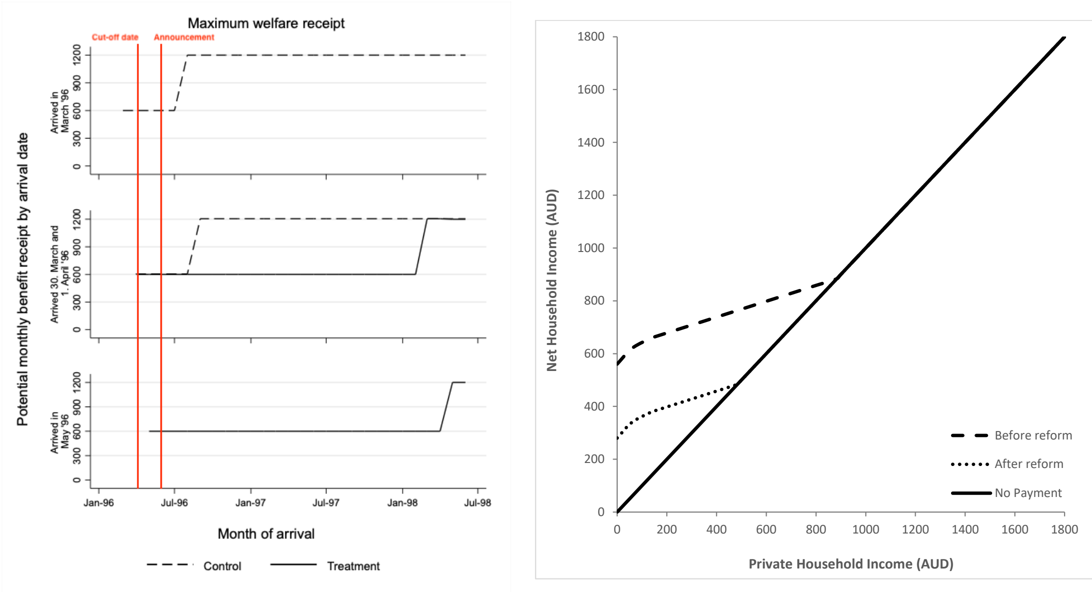

Figure 1: Policy change on maximum welfare receipt by arrival month and on taper rates

Note: The first panel visualizes an exemplary case of a migrant eligible for maximum receipt who entered before the policy change (control group) and a month after (treatment group). The second panel displays the tax schedule for migrants affected by the reform before and after the cut-off date.
Source: Australian government information, own visualization.

portant to note that migrant mothers still had access to some payments (again at maximum 50% lower than previously) and were fully eligible for health care coverage.

Second, the policy changed the taper rates, and third, the job-search requirements. The second panel of Figure 1 illustrates the effect on the taper rate at one point during the waiting period (for example, six months after arrival). Mothers arriving on or after 1 April 1996 were made worse off by the policy change. This is because primary carers were made ineligible for the relatively generous *Parenting Allowance*. A family earning no private income faced a reduction of 50 percent of disposable income. Apart from an income effect, families also faced a substitution effect. Between the income range of A$500 to A$900 per fortnight, families under the old policy (arrived before 1 April 1996) faced higher taper rates because, in that income range, they were still entitled to benefits, which was not the case for those subject to the new policy. The income and substitution effects both encourage a greater labor supply for the primary carer and the partner, as did the job search requirements, which only affected those that arrived after the cut-off.

Third, the policy changed the job-search requirements. Unlike with Parenting Allowance, prospective recipients of Unemployment Benefits were required to fulfill activity requirements. This involved filling in a JobSeekers' Diary (JSD), which needed to list the details of all job applications (the employer name and contact details, job description, and the job search method to find the vacancy) for each fortnight over a three-month period. Moreover, each JSD participant was instructed on the minimum number of jobs per fortnight for which they must apply based on local labor market conditions.

Notably, apart from these changes, rules for other welfare payments remained unchanged at the 1 April 1996 cut-off. There were also no other changes affecting natives on this day, rendering any general equilibrium effects on this specific day unlikely. However, some migrant groups were exempt from the NARWP policies, and those migrants did not face any waiting times.[13]

2.2 Data and representation

Our primary data comprises the universe of all welfare recipients. The Department of Social Services holds the administrative records and is called "Data On Multiple INdividual Occurrences" (DOMINO). These data have several key benefits. First, from 2001 to 2017, the event data enable a detailed analysis of welfare receipt patterns. Second, they include both welfare recipients and individuals receiving other non-means-tested government transfers, so they also include individuals who are income-disadvantaged and those who are relatively advantaged. This means

[13] These include those on humanitarian visas; New Zealanders; Chinese persons who entered under designated temporary entry permit; holders of specific visa classes such as 820 (Spouse), 826 (Interdependency), 832 (Close ties), 833 (Certain unlawful citizens). Migrants who were single mothers were eligible to receive Single Parent Payments upon arrival in Australia. Similarly, migrants who could not work post-arrival in Australia due to severe health issues could receive the Disability Support Pension or Disability Wage Supplement without a waiting period.

our data capture nearly all families with children.[14] Third, they include all migrants who had any contact with the social security system anytime from 2001 to 2017, therefore representing a large data set ideal for estimating RDD regressions. Fourth, we observe various information on the quality of the job held by the individual.

However, two dimensions of the data are potentially problematic for our analysis. First, our data began in 2001 (cf. Appendix Figure B.1.1),[15] six years after policy enactment, creating potential selection into out-migration. For example, migrants from less favorable backgrounds may be policy-induced to leave the country due to the smaller assistance payments. We perform balance tests to check visible discontinuities in the characteristics and density tests. In addition, we use another data set that extends back to when the policy was enacted to show the immediate and short-run responses to the policy (cf. Appendix Figure C.1.1). The Longitudinal DataSet (LDS) is a smaller administrative-based data set, which only includes a 1 percent random sample of the total welfare population. We present the immediate effects of the policy in Appendix C. Our primary analysis relies on the DOMINO data because of its larger sample.

Second, by using welfare data, there is the potential for policy-induced selection into welfare payments. As we include all individuals that receive either welfare payments 'or' family tax benefits, or childcare benefits. Thus, by using their union, we include nearly all (migrant) mothers in Australia.

In Figure 2, we provide the country of origin distributions in the welfare data (red/bright) and Census data (blue/dark) both from 2016.[16] In the first panel,

[14] Family Payments assist families with the cost of raising children and are provided to approximately 81% of families with children (Australian Bureau of Statistics [ABS], 2007).

[15] Arrival date is not collected until 2001, six years after policy-enactment.

[16] We use the 2016 Census and compare migrant women who arrived in 1995, 1996, and 1997 and then compare this to the same population in the welfare data in 2017 (on Census night) that arrived in the same years. We use these year-based all-female cohorts as we do not observe exact arrival dates in the Census or any children that left home before the Census date.

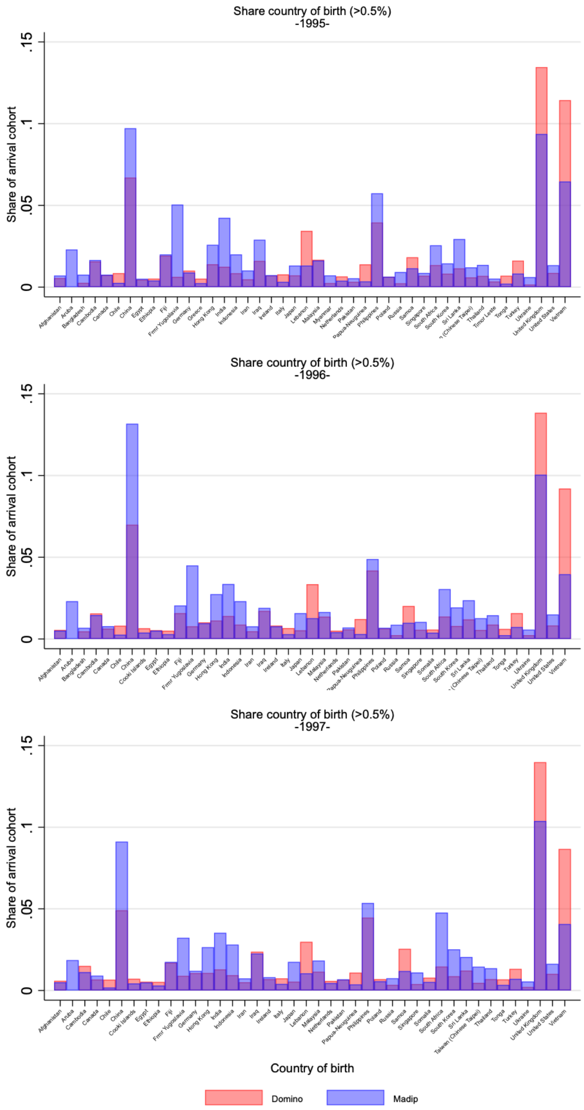

Figure 2: Aggregate country of birth distribution and selection into welfare

Note: The Figure displays the share of female migrants in the 2016 Census population (red/light) that migrated in 1995 - left, 1996 - middle, and 1997 - right-panel and equally for the 2016 (for comparison) welfare data (blue/dark) that migrated in the same years. We omit countries with population shares (among migrants) smaller than 0.5%.
Source: DSS-Domino Data 2016 and MADIP Census 2016, own calculations.

we show female migrants in 1995 (pre-reform). The distributions are very similar. There are, however, some notable origins with a higher share of people on any welfare payments, including Vietnam, Great Britain, Lebanon, and Turkey, although the differences are relatively small. For example, in the 2016 Census, 9% of all migrants that came to Australia in 1995 were from Great Britain, yet their respective share in the welfare distribution was 14%. More relevant for our setting, these patterns are constant across all three arrival cohorts. Among those that arrived in 1997 (post-reform), 10% were from Great Britain and, again, they made up 14% of the welfare distribution. Thus, we expect this difference to remain stable close to the reform cut-off date. In the appendix, we use a continuous variable (1995 GDP per capita) of the country of origin instead of raw country shares to show similar consistency across years (cf. B.3.1). This is noteworthy as this was a period of transition from family migration to skill-biased migration (cf. B.2.1).

Finally, it is reassuring that all countries in the Census are represented as expected in the welfare data, as our welfare definition includes a near-universal payment for mothers. Thus, due to the common support, external validity is likely high in this sample and treatment regime.

Regarding our primary estimation sample, Table 2 shows descriptive statistics by the arrival date, all variable definitions are presented in Appendix Table B.1.1. The 'Pre' group consists of mothers who migrated before the policy was enacted, and the 'Post' group represents those who arrived after. Columns (1)-(3) cover the overall sample: all migrants that arrived with children from 1995 to 1996. Columns (4)-(5) cover the sample close to the cut-off (three months before and two months after), roughly corresponding to the sample selected by the optimal bandwidth selection from our main specification. Standard deviations are presented in brackets, and standard errors of the differences are shown in parentheses. Columns (1)-(3) include the post-period mothers that arrived after the announcement date (on 23 May 1996). However, the sample based on the (data-driven) optimal bandwidth restrictions mean that we effectively do not include mothers who arrived after the announcement date (23 May).

On average, mothers were 32 years old at arrival, and their youngest child was around 2.5 years old. On average, 60% of mothers arriving in 1996 came from

Table 2: Descriptive Statistics: Means of Pre-arrival Variables Pre- and Post-Reform

Descriptive statistics and mean differences by reform status	Overall sample (2 year window)			Five-month window of reform		
	Pre	Post	Δ	Pre	Post	Δ
	(1)	(2)	(3)	(4)	(5)	(6)
Mother age at arrival	32.198	32.512	0.314	32.423	32.214	-0.209
	[8.015]	[8.082]	(0.104)	[8.013]	[7.886]	(0.168)
N	13681	8136	21817	2580	2921	5501
Child age at arrival	2.405	2.815	0.410	2.788	2.595	-0.194
	[6.220]	[6.375]	(0.119)	[6.435]	[6.210]	(0.215)
N	13681	8136	21817	2580	2921	5501
Partner age at arrival	36.203	36.309	0.107	36.390	35.916	-0.474
	[8.015]	[8.082]	(0.142)	[8.013]	[7.886]	(0.193)
N	13681	8136	21817	2580	2921	5501
English main language in origin country	0.367	0.376	0.009	0.372	0.372	0.000
	[0.482]	[0.484]	(0.012)	[0.483]	[0.483]	(0.018)
N	13681	8136	21817	2580	2921	5501
GDP per Capita (PPP) in 1995 in origin country	8244.2	8081.8	-162.34	8080.1	8198.4	118.36
	[8805.3]	[8675.9]	(252.34)	[8869.8]	[8798.8]	(331.10)
N	13681	8136	21817	2580	2921	5501
Share of females no education in origin country	18.281	18.245	-0.035	18.402	17.960	-0.441
	[19.637]	[20.030]	(0.390)	[19.698]	[19.975]	(0.705)
N	11489	6612	18101	2173	2347	4520
Average years of schooling in origin country	6.998	7.032	0.034	6.978	7.079	0.101
	[2.546]	[2.546]	(0.052)	[2.529]	[2.542]	(0.088)
N	11489	6612	18101	2173	2347	4520
Share agreeing with men preferred when jobs are rare in origin country	0.576	0.573	-0.003	0.574	0.566	-0.009
	[0.190]	[0.191]	(0.004)	[0.187]	[0.189]	(0.006)
N	11136	6517	17653	2080	2342	4422
Share agreeing with children suffer when mothers are working in origin country	0.566	0.571	0.006	0.564	0.563	-0.001
	[0.176]	[0.172]	(0.005)	[0.179]	[0.170]	(0.007)
N	8182	4615	12797	1525	1614	3139
Co-ethnic arrivals in 1995	3698.4	3766.2	67.76	3675.5	3847.2	171.75
	[3483.9]	[3564.2]	(75.79)	[3442.2]	[3589.0]	(135.70)
N	13153	7763	20916	2498	2788	5286
Conflict in childhood of mother in origin country	0.390	0.367	-0.024	0.395	0.366	-0.029
	[0.488]	[0.482]	(0.010)	[0.489]	[0.482]	(0.016)
N	13681	8136	21817	2580	2921	5501
Degree of vulnerability index	46.589	47.158	0.570	46.548	46.054	-0.494
	[26.490]	[26.765]	(0.578)	[26.193]	[26.254]	(0.872)
N	13681	8136	21817	2580	2921	5501

Notes: Table presents descriptive means and standard errors by arrival date. Columns (1)-(3) for the two-year window and (4)-(6) for the five-month window (similar to the optimal bandwidth window of three months before and two months after the reform). Columns (1) and (4) are pre-policy, (2) and (5) are post-policy, and (3) and (6) show the difference and test for significance in observables. Variable definitions are presented in Appendix Table B.1.1.

Source: DSS-Domino Data 2001-17, own calculations.

countries where English is neither the official nor a common language used and with relatively low levels of general education (where on average 20% of females have no primary education). They also tended to arrive from countries with relatively modest economic development, around A$8,000 GDP per capita on average, which corresponds in this time to countries such as Lebanon, Bulgaria, and Iran. The majority (57%) of mothers migrated from countries with traditional gender-role values, which we capture through two World Value Survey (WVS) questions about 1) preferences for men to work rather than women when jobs are scarce and 2) beliefs that children are negatively impacted when mothers work. Migrants arrive with a co-ethnic network size of around 3,700 people on average, based on the number of existing migrants from the same country that arrived in 1995 (pre-policy). However, the standard deviation is almost as large as the average. Approximately 38% of migrants experienced conflict in their origin country during their childhood. The degree of vulnerability faced by migrants, based on our own summary index measure[17], is around 47 (out of 100). From the average characteristics of our sample, it is clear that we are analyzing a group of relatively vulnerable mothers whose opportunities to transition into the labor market in Australia upon arrival are likely to be highly constrained.

Significant differences in the two-year window (Column 3) include the mother's age, the (upon arrival) youngest child's age, and conflict during the mother's childhood. For the two-year sample (Columns 1 - 3), the pre-reform group of mothers was slightly younger (32.2 years) than the post-group (32.5 years), but the difference vanishes for the sample that is closer to the cut-off (Columns 4 - 6). Similarly, children were slightly younger (2.4 years) in the pre-group than in the post-group (2.8 years). The proportion experiencing conflict during childhood was somewhat higher in the pre-group (39%) than in the post-group (37%). Significant differences in the five-month window (Column 6) appear only (marginally for) the partner's age, which was slightly higher for the pre-group (36.4 years) than the post-group (35.9 years).

[17] We follow Lindner and Reizer (2020) and use pre-reform mothers (1995) that are not part of the sample (i.e., arrived before the chosen bandwidths) and regress their monthly welfare receipt on background characteristics (own, partner's, and youngest child age and country of origin indicators). The predicted score is our measure of the degree of vulnerability.

Overall, the differences are minor in magnitude. Based on origin country characteristics, there is no difference in the economic development, English background, or traditional gender norms across pre- and post-reform samples. Based on labor market variables, we find no difference in education or co-ethnic network size. Lastly, there is no difference between the pre and post-groups in our summary index measure of vulnerability. This reassures us that running an RDD on samples closer to the cut-off can lessen the bias arising from pre-existing differences between pre-and post-reform mothers. We are, consequently, confident that our pre- and post-samples are balanced in their background characteristics.

2.3 Estimation Strategy

The welfare reform extended migrants' residency requirements based on a sharp cut-off date and reduced their benefits substantially for 18 months (from 6 months to 24 months); the reform was announced ex-post (while the migrants were already in the country), alleviating selection concerns. This setup allows us to assess the reform effect using a regression discontinuity in arrival time design (RDD) (Lee and Lemieux, 2010), which compares migrants that arrived just before the reform date to migrants that arrived just after the reform date on 1 April 1996. More specifically, to estimate the local intent-to-treat (ITT) effect, we use the following model

$$y_{it} = \alpha + \tau \times \text{reform}_i + f(T_i) + x'_{it_0}\beta + \varepsilon_{it}, \tag{1}$$

where y_{it} are various long-run outcomes, i.e., months on welfare receipt, $reform_i$ is a dummy indicator of whether i arrived on/after the reform date, $f(T_i)$ is the running variable counting days between the date of entry and the reform date to flexibly account for differential trends before and after the reform, for some specifications we include pre-arrival covariates x_{it_0}, and ε_{it} is an error term.

The main coefficient of interest is τ. We use various estimators to assess its sensitivity. Our main approach uses local linear regressions and optimal (and possibly asymmetric) bandwidth selection (Imbens and Kalyanaraman, 2012)[18] and robust non-parametric bias reduction following Calonico, Cattaneo and Farrell (2018). We amend these with a fixed bandwidth local linear regression approach and linear OLS with a third-order polynomial in arrival time. We assess additional robustness by using placebo dates and groups and including covariates. In addition, we test for heterogeneous impacts to assess mechanisms by running subsample RDDs.

Under the assumption of no manipulation, ex-changeability, and the outcome probability being continuous at the cut-off in the absence of the reform – that we will assess next – the RDD will identify the ITT of reducing welfare benefits upon arrival. We are confined to the ITT because we do not observe the arrival period directly in our main data.[19] Using the LDS, a data set that is much smaller and thus needs to be interpreted cautiously, we find an initial reduction of receipt by 2.4 months on average (out of a claim eligibility of 18 months). It is worth pointing out that the ITT might be more attractive than the take-up weighted ATT, as the option value of having insurance might induce behavioral change over and above the actual take-up of benefits such as a piece of mind and a sense of economic security.

2.4 Identification

In this section, we establish the validity of the RDD in our setting. As mentioned previously, Australia shifted towards skill-based migration in the late 1990s. As such, we test whether there were any discontinuities in the density of arrivals, such as bunching before the cut-off date. Note that this is highly unlikely as the policy was announced ex-post.

[18]We use two different MSE-optimal bandwidth selectors (below and above the cut-off).

[19]We are also confined to the ITT also since we do not observe the initial arrival visa category and thus can not distinguish humanitarian migrants who were exempt from the policy reform. Appendix Figure B.2.1 suggests their limited impact on the overall population.

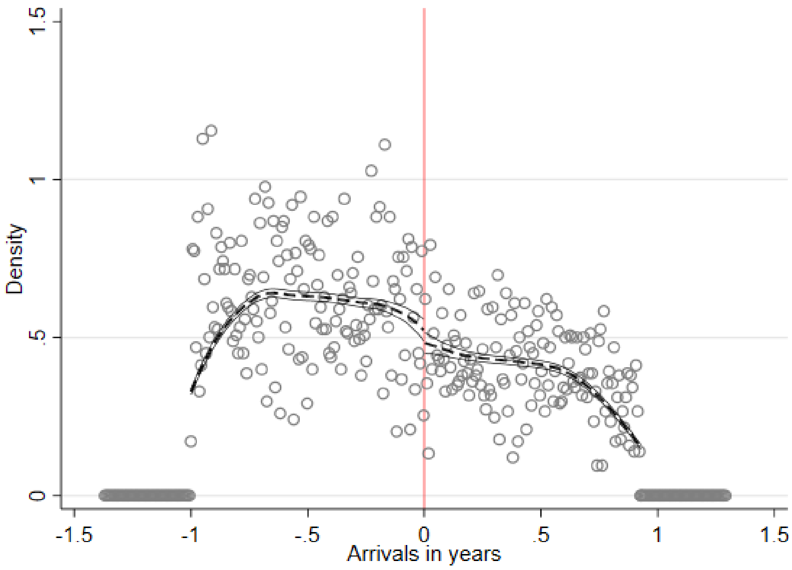

Figure 3: DENSITY TEST

Note: The Figure displays the McCrary (2008) density test. It plots the number of observations around the cut-off date. The selected bin size is 0.006, and the bandwidth is 0.33. The test can not reject any imbalances.

Source: DOMINO 2001-17, own calculations.

In Figure 3, the McCrary (2008) density test confirms that there is no break in the density around the cut-off.[20] Additionally, the alternative test by Cattaneo, Jansson and Ma (2020) shows analogous p-values over 10% for both the total sample and for the selection excluding missing countries of origin (in Appendix Table C.2.1 we show that there is no difference in missing country information at the policy cut-off).

Despite the overall smoothness of the timing-of-arrivals profile, assessing any compositional changes that might have occurred is essential. This is also related to potential out- or out-of-welfare-migration that may have happened between the policy enactment and the start of the data collection. As noted above, the relative frequencies by country of origin stayed constant (Figure 2). However, this aggregate comparison might not hold at the cut-off. Thus, we assess imbalances in the migrant's background by estimating equation (1) and using various pre-migration

[20]The implied discontinuity estimate (log difference in height) is -0.057 with a standard error of 0.040, and the corresponding t-statistic is roughly 1.489. The discontinuity is, therefore, insignificant.

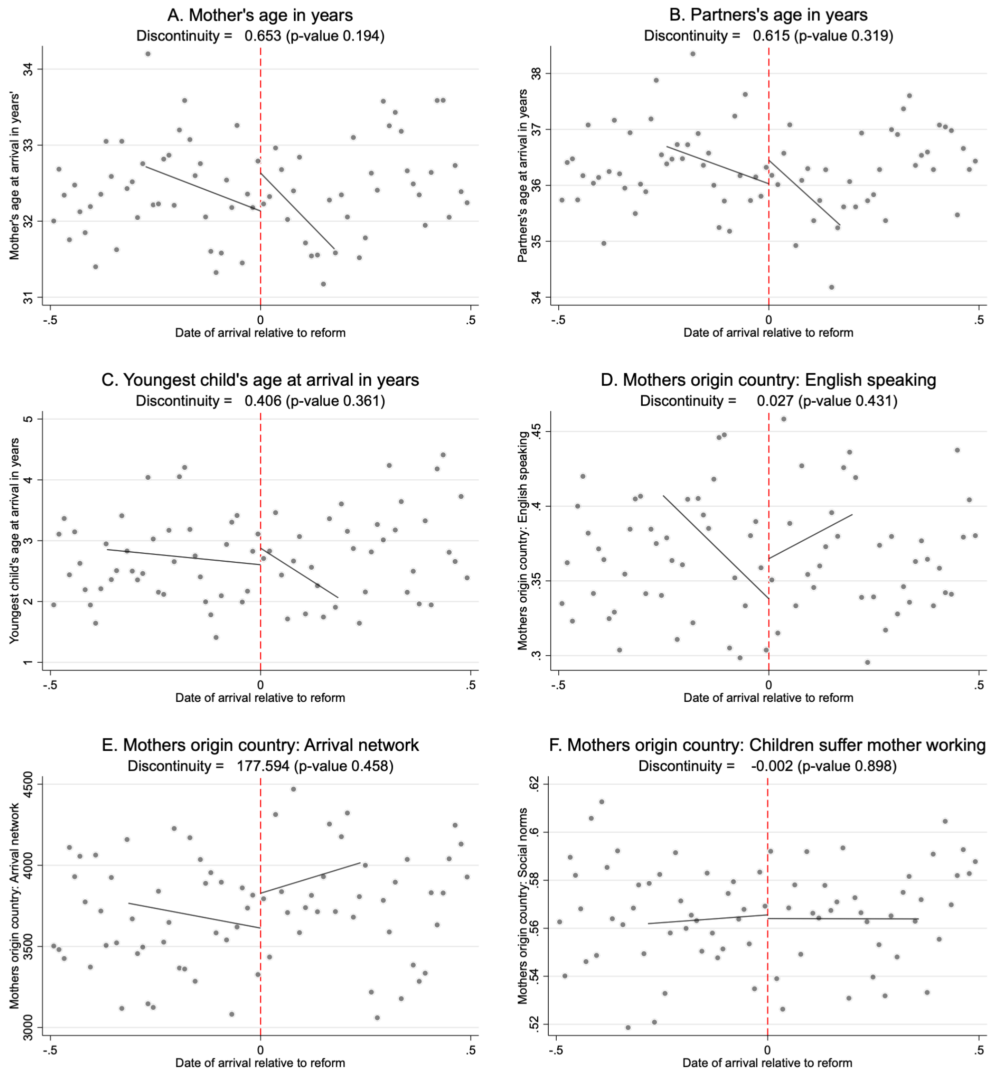

Figure 4: Selected covariate balance: pre-migration characteristics

Note: The Figure displays discontinuity plots using outcome-specific optimal bandwidths analogous to our main specification. Appendix Table C.2.1 presents the full set of covariates and additional statistics.
Source: DOMINO 2001-17, own calculation.

characteristics as outcome variables (using outcome-specific and possibly asymmetric optimal bandwidths). Figure 4 shows the RDD balance plots for selected pre-migration covariates (Table C.2.1 in the Appendix presents the full battery of tests).

We start by testing whether there are any differences in the age-at-arrival of either the focal mother (Panel A), her partner (B), or her youngest child (C). None of these show any differences close to the threshold. The partner's age was marginally significant in the mean comparison (cf. Table 2) and highly insignificant in the local linear estimation. Turning to the background characteristics of the mother's country of origin, we find very large p-values for the language spoken (English vs. not-English, Panel D, see Bleakley and Chin, 2004; Auer, 2018; Isphording and Otten, 2014), the size of the co-ethnic arrival cohort just before the mother arrived (Panel E, see Beaman, 2012; Egger, Auer and Kunz, 2021), or whether a large share in the country agrees with the statement that children suffer if the mother works (Panel F, see Steinhauer, 2013).

In the Appendix Table C.2.1, we further assess the country-of-birth income (GDP per capita in 1995). We find no evidence of a discontinuity in economic development at the cut-off by the migrants' origin country. Next, we assess the discontinuity in trade relations with Australia. For example, migrants might have easier labor market access if their country of birth is a sizable trading partner (Parsons and Vézina, 2018). We find a small and marginally significant discontinuity in overall trade (p-value 0.09), yet, there are no discontinuities by the size of exports and separately for the size of imports. Around the cut-off, migrants come from countries with similar cultural backgrounds (Spolaore and Wacziarg, 2017; Islam and Raschky, 2015) and female education levels (Barro and Lee, 2013). Their partners are just as likely to come from English-speaking countries and countries that tend to agree with the statement that: "when jobs are scarce, men should have more right to a job than women" (Guiso et al., 2008). Lastly, we find no discontinuities in whether migrants experienced war or significant conflict in their country of origin during their childhood – which takes the age of the mother and her migration time into account – which is related to trauma (Couttenier et al., 2019).

We check whether the propensity to leave Australia differs between pre- and post-reform mothers. Although this would not introduce a bias in our estimates, it would affect how we interpret the observed differences in long-run welfare take-

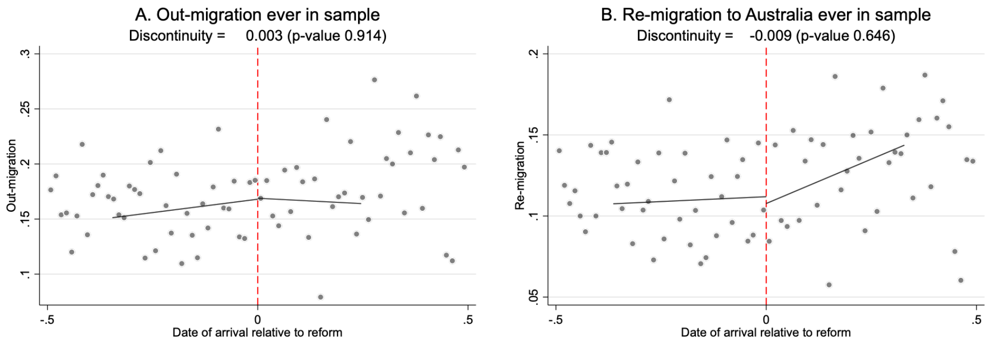

Figure 5: Post-arrival selection: Out-migration and re-migration to Australia

Note: The Figure displays estimates from (1) using indicators for *ever out-migrated* or *re-migrated* in the sample period.
Source: DOMINO 2001-17, own visualization.

up. We can not test out-migration directly due to the discrepancy in the reform time and the data collection window. Yet, as documented above, migrants in the welfare date are similar to migrants in the Census data, suggesting no selection into out-migration close to the reform date. We validate this further in Figure 5, which shows no discontinuity among those who left Australia or those who returned to Australia. Meaning that the propensity to leave Australia is the same between the pre-and post-groups, as is the propensity to return to Australia after leaving the country.

To summarise, a multitude of tests confirm the validity of our estimation approach. Overall based on a comparison with Census data, we show that the mothers in our sample are similar to the overall resident population. There are some slight differences in the country-of-origin composition, but overall they exhibit identical trends across the years and very similar distribution of their origin country's economic status. We show no break in the density by the arrival date, and the background characteristics are balanced at the cut-off, especially once we adopt the optimal selected bandwidth. The only exception is trade, which appears to be unbalanced at the cut-off with a p-value of 0.09, using the local linear approach with optimal asymmetric (robust and bias-adjusted). Given that we assessed 24 indicators, we are reassured by these results.

3 Main results

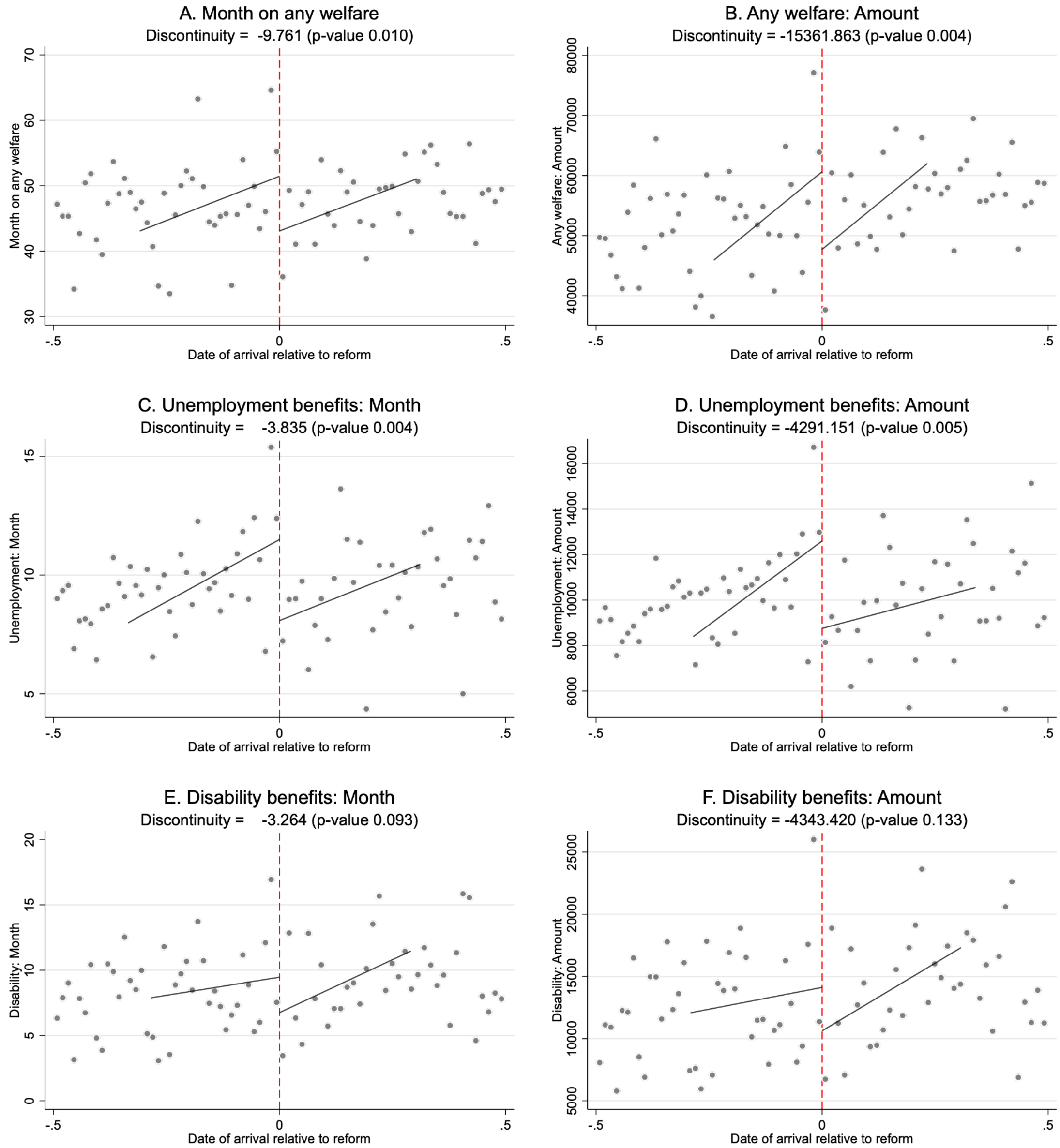

Figure 6: RDD estimation: Arriving after policy change on any income receipt in months

Note: The Figure displays our main RDD design motivated in (1). We present six panels, first the count of months on any welfare (A), and the amount of welfare (B). We then assess these outcomes for the two main welfare payments, unemployment benefits (C and D) and disability benefits (E and F). All are based on local linear regressions with optimal bandwidth and bias-corrected robust standard errors. Bandwidths are allowed to differ pre- and post-reform for each outcome.
Source: Domino 2001-17, own visualization.

We now turn to our main results. As mentioned above, our data allow us to estimate the ITT. The RDD estimates in Figure 6 summarise our main results. We look at welfare receipt for up to 21 years after policy enactment.[21] The left panel shows the total number of months on welfare payments. The right panels show the amount received for the respective welfare payments. There is an apparent discontinuity at the arrival date for both time on welfare and amount of welfare.

Panel A shows that the number of months on any welfare payment dropped by roughly ten months on average, from a baseline of about 50 months, or about 20%. Figure B shows the amount of any welfare received decreased by A$15,000, from a baseline of A$60,000, or about 25%. These are driven by the two largest payment categories where adjustments are the most likely:[22] Panel C: number of months on unemployment benefits dropped by four months on average (from a baseline of 12 months or 33%), and the amount of unemployment benefits decreased by A$4,000 (from a baseline of A$12,000, or about 33%) and Panel D: the number of months on disability benefits dropped by three months on average (from a baseline of 10 months, or about 30%), and the corresponding amount of disability benefits dropped by A$4,000 (from a baseline of A$14,000, or about 29%). We see an economic and statistically significant decline in receipt across welfare payment types, even those not directly targeted by the welfare reform of interest - both at the intensive and extensive margin in the long run.

Table 3 presents the corresponding coefficient estimates of our main analysis and assesses the sensitivity of our design (similar to Lindner and Reizer, 2020). Columns (1) and (2) show the OLS estimates. The bandwidth is restricted to 6 months before and after the reform. Columns (3)–our prefered specification– and (4) restrict the bandwidth based on optimal selection. Columns (5) and (6) use local linear regression and manually restrict the bandwidth to 5 months, three months before, and two months after the reform. Panels A and B focus on any

[21]We further assess the extensive margin (ever being on benefits), however, a large part of our sample is unemployed at least once during the 21 year period, thus, we focus on the length of time on benefits.

[22]The residual category of all other payments is unaffected by the reform, results available upon request.

welfare payment, showing the duration and amount, respectively. Panels C and D focus on duration, showing unemployment and disability benefits, respectively. In sum, the results suggest a sizable drop in long-run welfare receipt that appears to affect the extensive margin (i.e., time on unemployment benefits), and thereby the intensive margin. In other words, similar welfare payment amounts are received when on benefits, which suggests mothers are engaging in part-time work and top-up welfare or higher household income plays a lesser role. We, therefore, focus most of our discussion on the time-on-benefits rather than the amounts.

Overall, the results are consistent across the three specifications. The OLS estimates are slightly larger than the RDD estimates. We thus rely on the RDD specifications as they provide more conservative results. The RDD estimates show a highly statistically significant reduction in the total number of months on welfare (of 10 months from a control mean of 48 months) over the course of 21 years after arrival. This amounts to an 18.5% decrease in welfare dependence. Regarding the amount of welfare, the RDD specifications suggest that post-reform mothers received A$15,000 less in total benefits (from a baseline amount of A$54,000), corresponding to a 27.6% reduction. With regards to the two most relevant welfare benefits for incoming migrants, unemployment benefits and disability benefits, we find a 3.8-month reduction in unemployment benefits duration (40% reduction from a relatively low base of 10 months) and a A$4,000 reduction in unemployment benefit amounts (40% compared to a base of A$10,000 over 21 years). For disability benefits, we find a 3.3-month reduction in duration (40% reduction from a control mean of 8 months) and a A$4,300 reduction in the amount received (30% reduction from a base of A$13,000). It is important to note that the overall welfare receipt is very low; the two largest categories of unemployment and disability are only taken up 10 and 8 months out of a potential receipt length of 180 months. Yet, the reform reduced months on receipt even further from this already low-dependence margin.

Next, we present two placebo tests of our results. Appendix Table C.3.1 (Cols. 1 and 2) shows the results of a placebo test using migrants who arrived without children and subsequently became mothers after arriving in Australia. Since the reform only affected families with children, these women were not subject to the reform upon arrival. Indeed, we find no difference in the outcomes between the pre and post-groups across all of our specifications. Analogously, Cols 3 and 4, show the results of the placebo test using a different cut-off of 1 April 1995, i.e., the year prior to the actual implementation. Again, we find no difference in the outcomes between the pre-and post-groups. The exception is the duration of any welfare in the RDD specification without covariates (Panel A, Columns 3 and 5). Since there is only one significant coefficient out of 16, that disappears once controlling for covariates. We attribute this to random noise.

Our results imply a long-run elasticity of roughly 55% (reducing payments by 18 months reduces long-run receipt by ten months). One potential interpretation for this result is that longer receipt of welfare benefits in those initial years upon arrival increases reliance as a whole because individuals gain a better understanding of how to navigate the system. Alternatively, less initial support may prompt earlier entry into work, which brings long-term benefits through the stepping stone phenomena or improved health.

3.1 Amount or timing?

Next, we focus on the time profile of our estimates. We estimate the reform effect when the welfare payment's potential duration is fixed. This means we can isolate the impact of the timing of entry into the system upon migration (as a result of the waiting time policy). Categorical ineligibility (for example, once the youngest child reaches 16 years) means that the timing-of-receipt effects can be conflated with the total duration-on-welfare effects. For example, a migrant mother arriving with a 12-year-old before the policy reform can expect 3.5 years of generous welfare benefits (4 years till the youngest child reaches 16 minus the six-month waiting period), a post-reform mother arriving with a 12-year-old after April 1st, has to wait for two years, thus receives at most two years of benefits. To isolate the effect, we look at mothers with the same potential time on benefits, i.e. in the example, a post-reform mother with the youngest child being 10.5.[23]

In Appendix Table C.4.2, we compare the estimates from the total effect and those that hold constant the potential duration. Our results are largely unaffected. One exception is that we find the reform led to a larger reduction in unemployment benefits once we hold the potential duration fixed, but the differences are minor.

The importance of limited benefits in worker behavior has been recognized for a long time; see, for instance, Katz and Meyer (1990), Van Ours and Vodopivec (2006), and Card, Chetty and Weber (2007). More recent studies have focused on policies explicitly targeting the initial period. In conjunction, our results suggest that this critical period is not only highly influential in unemployment spells but also in the initial period of labor market entry, relevant for young people and re-entrants such as carers that return after periods of leave.

3.2 Over-time profiles

Assessing the time path of the differences in effects is important to show whether differences might vanish between treated and control (Andersen, Dustmann and Landersø, 2019). Recall that after two years, the treated and control mothers faced the same welfare regime and access to support payments.

Table 4 shows the number of months on welfare over three periods: 6-10 years, 11-15 years, and 16-21 years. The results suggest a substantial widening in the effect over time. That is, the impact of the policy grows, with the largest decline

[23]Since the difference between arriving with substantially younger children might change the adjustments, we estimate two versions of this excluding children younger than 5. Here, we expect a mother may be more time-constrained than a mother with older children. However, we find that the results are very similar.

occurring 16-21 years post-arrival at 4.2 months.[24] Similarly, for unemployment and disability benefits, the average duration grows over time. However, the overall dependence relative to the base stays constant at around 40% for both payments. This reflects changes in the level of reliance among those affected in the long term compared to those affected in the short and medium term. In contrast to migrant mothers who arrived before the cut-off date, the treated women appear to exhibit a greater level of protection or insulation. This is noteworthy, particularly as employment disruptions or disabilities are increasing in prevalence over such a long assessment window. One explanation may be that the treated group retains employment or becomes re-employed more quickly. An alternative explanation is that the treated group is less equipped to navigate the welfare system since they were excluded initially. This would most likely affect disability payments. Given that one group is not necessarily more likely to develop a disability than other benefit categories, disability benefits receipt requires much more paperwork and navigation of the system. Thus, these effects might, in fact, reflect a barrier to entry into the system (Bertrand, Luttmer and Mullainathan, 2000; Aizer and Currie, 2004).

Our results are in line with those in the literature. For example, Andersen, Dustmann and Landersø (2019) find a 30% reduction in transfer payments, and a 17% decrease in the unemployment rate over the first five years, although these effects fall over time, whether-as ours increase for the longer run. In our case, both treatment and control groups are eligible for the same benefits after the two-year waiting period.

3.3 Job quality

Given that the reform led to a long-run reduction in welfare receipt, which grew and stabilized over time, it is natural to ask whether affected migrants achieved

[24] Note, the length of the windows are different so the relative-to-mean effect is more comparable over time.

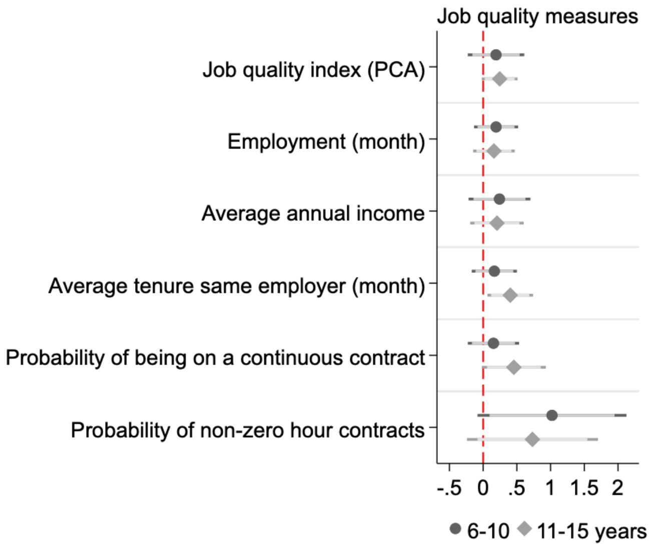

Figure 7: RDD estimation: Standardized job quality measures

Note: The Figure displays coefficients for our job quality index, employment, annual income, average tenure with the same employer, and the probability of being on a continuous contract and a non-zero-hour contract, standardized –for comparison– relative to their respective control means. See Appendix Table C.4.3 for coefficient estimates in numerical and non-relative form.
Source: Domino 2001-17, own visualization.

better job quality (Nekoei and Weber, 2017). Few studies focus on direct measures of re-employment job quality instead of on re-employment wages (one of the few notable exceptions include Farooq, Kugler and Muratori, 2020, also see references therein). However, job quality is a multidimensional construct that may or may not be summarised well with wage levels. Making use of our rich welfare receipt data, we assess dimensions that have often been overlooked in the literature.

Figure 7 shows the impact of the reform on job quality in the short-term (6-10 years) and medium-term (11-15 years) standardized relative-to-mean, Appendix Table C.4.3 presents these in numerical and non-standardized from.[25] Overall, there is some suggestive evidence of a positive effect on job quality. We analyze multiple dimensions of job quality, including, from top to bottom, a measure of job quality using principal component analysis, employment, tenure with the same employer, the chance of being on a 'continuous' contract, and the chance of being

[25]We confine our analysis to the 15-year window because children can become independent welfare recipients at age 16. This means that parents could lose eligibility for family tax benefits and might disappear from our sample due to being categorically ineligible, cf. Section 2.2.

on a non-zero hour contract. The results suggest that job quality improved in the long-term, employment probabilities were not significantly affected, tenure with the same employer significantly enhanced, and the chance of being on a ‘continuous’ contract and a non-zero hour contract both increased. Despite the imprecision in these estimates, they generally tend to point to improvements in job quality, which grow over time and become statistically significant.

The results of a suggestive lift in job quality in the longer term align with the stepping stone theory. However, as our results are relatively imprecise in terms of their level of statistical significance, we only draw conclusions that are suggestive of this phenomenon. Yet, it seems clear that it is not the case that these migrants are stuck in worse employment due to the initial push to work, the potentially shorter search period, and worse job matches.

3.4 Heterogeneity and mechanisms

We assess whether differential effects emerge across the population of migrants. We are primarily interested in whether migrants who are ’highly employable’ are more likely to respond to policy variation. In other words, we assess whether the policy made some migrants better off at the detriment of others.

Figure 8 reveals which groups were affected most by the reform (Appendix Figure C.4.1 presents the results, including selected job quality indicators). We define the disadvantaged group as anyone with children under 5, those older than 35, originating from a non-English speaking country, originating from a country with a low share of educated women, originating from a country that has below median trade with Australia, originating from a country with traditional gender norms, arriving with below-median co-ethnic network size, and has an above median degree of vulnerability. We define advantage as anyone with the opposite of these characteristics. We analyze the effect of the number of months on 1) any welfare benefits, 2) unemployment benefits, and 3) disability benefits. The coefficient re-

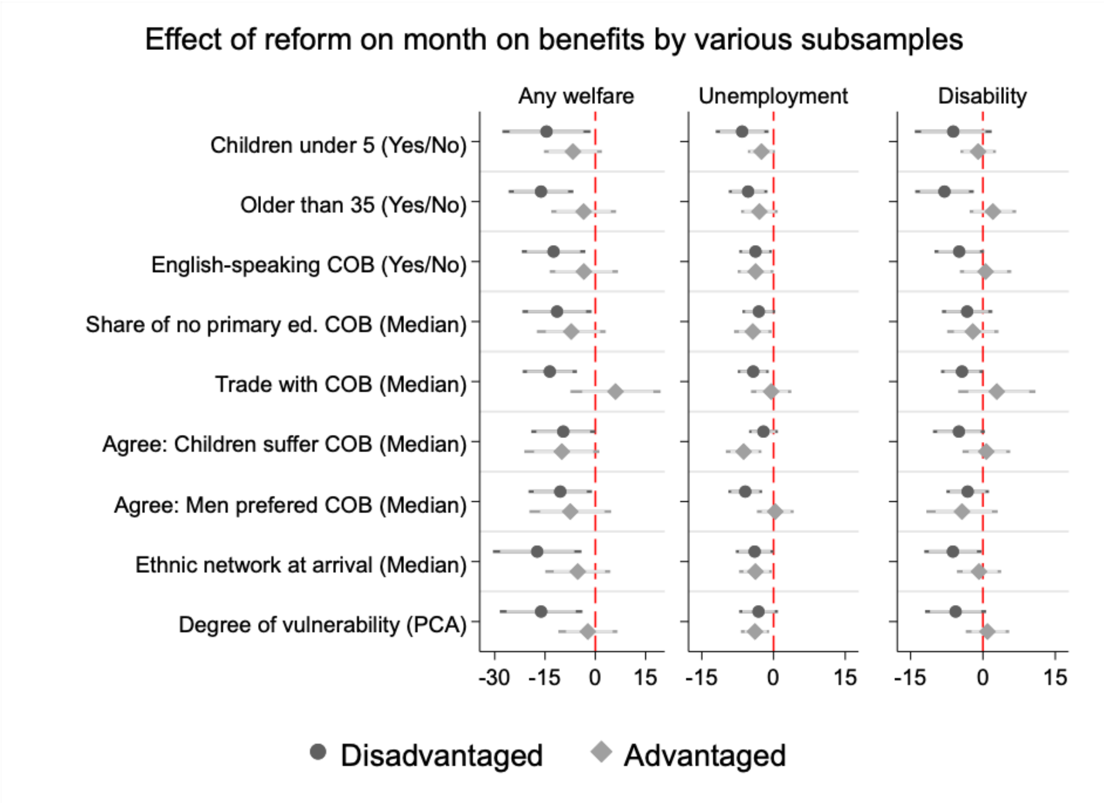

Figure 8: RDD estimation: Effect on months on any income receipt by characteristic

Note: The Figure displays coefficients of sub-sample regressions using our preferred specification. COB denotes the Country of Birth. Dots denote disadvantaged mothers who have the following characteristics: arrived with the youngest child under 5, older than 35, originated from a non-English speaking country, originated from a low-educated country, originated from a country that makes little trade with Australia, originated from a country with traditional gender norms, arrived with a small co-ethnic network, has a high degree of vulnerability. Diamonds denote advantaged mothers who have the opposite of these characteristics. Bandwidths are allowed to vary by the group to ensure we compare the true causal effects by the group. See Appendix Figure C.4.1 for the individual plots.
Source: Domino 2001-17, own calculations.

flects the difference in the months between the treated and control groups, where the treated group is anyone who arrived in Australia after the policy was enacted. We estimate two confidence intervals: the darker bounds represent 95% CIs and the lighter bounds represent 90% CIs.

Almost all of the coefficients are large and negative. Among the disadvantaged group, all of the results are statistically significantly negative. Overall, the underprivileged groups had lower long-run welfare receipts, as they experienced larger reductions in duration on welfare compared to the advantaged groups. For example, for affected migrants with children under 5, there is a 17-month reduction in time on any welfare benefit. This is large given a mean of 4 months reduction in the overall sample (cf. Table 3). For affected migrants with children older than 5, there is a seven-month time reduction on any welfare benefit.

We also estimate whether differences between the advantaged and disadvantaged groups are statistically significant. There is no accepted way in the literature to do this in a local linear regression framework. We take a highly conservative approach and use the upper and lower bound of the p-values to determine significance.[26] This is a much higher significance bar than applying the mid-range of the p-value bounds. For example, the lower bound p-value for trade with the country of origin is 0.00, and the upper bound is 0.07. As such, the treatment effects are significantly different between those from countries with low trade and those from countries with high trade. Despite this extreme approach, some groups have statistically significant differences (Appendix Figure C.4.1 presents all the bounds). The largest differences are by age, language, co-ethnic network size, and degree of vulnerability. For example, affected migrants older than 35, from non-English speaking countries, with small co-ethnic networks and a high degree of vulnerability, have significantly lower welfare duration than advantaged groups. It is a similar picture for months on unemployment benefits but with smaller differences overall. Notably, when it comes to gender norms, the difference is reversed: those from advantaged countries (with progressive views who think mothers should work) show a larger effect (fewer months on unemployment benefits) compared to those from disadvantaged countries (with conservative views). The overall effects are similar for disability benefits: those from disadvantaged countries experience larger reductions in welfare duration. The differences are larger for disability benefits than unemployment benefits, yet, few are significantly different across the

[26]The issue is that $\frac{X-Y}{se(X-Y)}$ where $se(X-Y) = \sqrt{var(X) + var(Y) - 2cov(X,Y)}$ depends on the covariance $cov(X,Y)$, which is unknown. However, $-1 \leq \frac{cov(X,Y)}{\sqrt{var(X)var(Y)}} \leq 1$ can be used to bound the corresponding p-values. Additionally, we use the third-polynomial linear specification from the main Table and test the difference directly; results are broadly confirmed (and available upon request).

groups – with our conservative bounds. Significant differences occur by age, with both upper and lower bound p-values less than 10%.

4 Conclusion

Excluding new migrants from welfare is a common feature of most labor migration programs worldwide. Understanding the implications of these policies on migrants' welfare and employment trajectories has been understudied. The 1996 Australian policy that extended the waiting period for new migrant mothers with children to access welfare provides an ideal setting to study this question. We apply regression discontinuity design to 21 years of administrative data to assess the long-term economic effects of welfare restrictions on migrants.

Our findings suggest a substantial reduction in long-run welfare receipt over much of the focal mothers' working life. We find that these differences grow over time and stabilize in the long run. This is mirrored in job quality, which is consistent with a stepping-stone hypothesis. Finally, it appears that all mothers exhibit receipt reductions, but more disadvantaged mothers reacted stronger. It appears that the timing of benefits eligibility is an important feature rather than the overall amount. Suggesting that the arrival period is a critical time to affect migrants' future labor market success.

Our results are consistent with those found for similar US reforms, which were very successful at reducing benefits, welfare caseloads, welfare duration, and payment amounts (e.g., Blank, 2006). In addition, we find some suggestive evidence of improved job quality. Heterogeneity suggests that very disadvantaged mothers benefited from the reform more than relatively advantaged mothers. An important open question is whether there are non-linear effects of this policy. The Australian government recently extended this waiting period to four years, and once the data becomes available, it will be interesting to see whether the effects we found extend over this much longer time horizon. Finally, we cannot say whether the reform was a success on non-economic variables as we have no information on migrants' financial security or well-being. This is an important question for future research as governments continue to tighten access.

Table 3: Reform effect on 15-year long-run welfare dependence

	Full sample		Optimal bandwidth		Constrained Bandwidth	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Months on any means-tested welfare payment (in 15-years / 180 months)						
After	-12.439	-13.455	-9.761	-10.939	-10.774	-10.884
	(4.705)	(4.627)	(3.774)	(3.987)	(3.743)	(3.672)
P-value:			0.01	0.01	0.00	0.00
BW before:	0.50	0.50	0.30	0.24	0.30	0.30
BW after:	0.50	0.50	0.31	0.28	0.20	0.20
Observations	11,353	11,353	21,817	21,817	21,817	21,817
Mean control	46.87	46.87	47.24	47.56	48.85	48.85
Panel B. Amount of means-tested welfare payment (in 15-years / 180 months)						
After	-18,426.17	-18,734.13	-15,361.86	-16,790.19	-14,687.60	-14,517.11
	(5,938.24)	(5,884.32)	(5,328.60)	(5,436.35)	(4,699.75)	(4,626.75)
P-value:			0.00	0.00	0.00	0.00
BW before:	0.50	0.50	0.23	0.22	0.30	0.30
BW after:	0.50	0.50	0.24	0.22	0.20	0.20
Observations	11,353	11,353	21,817	21,817	21,817	21,817
Mean control	52,108.41	52,108.41	54,319.94	54610.93	54,051.47	54,051.47
Panel C. Unemployment payments (in 15-years / 180 months)						
After	-4.264	-4.358	-3.835	-3.930	-4.004	-3.984
	(1.681)	(1.674)	(1.332)	(1.340)	(1.362)	(1.357)
P-value:			0.00	0.00	0.00	0.00
BW before:	0.50	0.50	0.31	0.31	0.30	0.30
BW after:	0.50	0.50	0.34	0.33	0.20	0.20
Observations	11,353	11,353	21,817	21,817	21,817	21,817
Mean control	9.44	9.44	9.88	9.86	10.38	10.38
Panel D. Disability payments (in 15-years / 180 months)						
After	-3.589	-4.229	-3.264	-3.360	-3.463	-3.491
	(2.306)	(2.267)	(1.942)	(1.968)	(1.842)	(1.798)
P-value:			0.09	0.09	0.04	0.04
BW before:	0.50	0.50	0.29	0.23	0.30	0.30
BW after:	0.50	0.50	0.28	0.30	0.20	0.20
Observations	11,353	11,353	21,817	21,817	21,817	21,817
Mean control	8.27	8.27	8.56	8.42	8.92	8.92
Covariates		✓		✓		✓
$f(T_i)$	No	3rd poly	Kernel	Kernel	Kernel	Kernel

Notes: The table presents coefficient estimates of the reform effect on the number of months on any benefits (Panel A), the amount received (Panel B), the number of months on unemployment (Panel C) and disability (Panel D) benefits. We present 6 model specifications. Column 1 is OLS with a third-order polynomial restricted to half a year pre and post the reform. Column 2 replicates Column 1 but also includes mother's pre-arrival characteristics (age of mother, partner and child). Column 3 is our main specification, local linear RDD, with optimal bandwidth that is allowed to vary on either side. Column 4 replicates Column 3 but also includes covariates. Column 5 fixes the bandwidth to 3 months prior and 2 months post reform. Column 6 replicates Column 5 but also includes covariates. We present coefficients, standard errors in parentheses, bias-corrected robust p-values (for the optimal bandwidth selection), the chosen bandwidth [BW], number of observations, and the control mean. See Appendix Table C.4.1 for the remaining estimates.

Source: DOMINO 2001-17, own calculations.

Table 4: Reform effect on 15-year long-run welfare dependence: Over time

	6-10 yrs	11-15 yrs	16-21 yrs
	(1)	(2)	(3)
Panel A. Month on any means-tested welfare payment			
After	-2.267	-3.616	-4.167
	(1.312)	(1.424)	(1.580)
Relative-to-mean	-0.15	-0.24	-0.25
P-value	0.08	0.01	0.01
BW before:	0.25	0.30	0.32
BW after:	0.31	0.31	0.34
Observations	21,817	21,817	21,817
Mean control	15.62	15.33	16.35
Panel B. Month on Unemployment payments			
After	0.049	-1.085	-3.042
	(0.277)	(0.528)	(1.061)
Relative-to-mean	0.06	-0.45	-0.45
P-value	0.86	0.04	0.00
BW before:	0.32	0.26	0.26
BW after:	0.25	0.36	0.27
Observations	21,817	21,817	21,817
Mean control	0.79	2.43	6.77
Panel C. Month on Disability payments			
After	-0.406	-1.019	-1.836
	(0.414)	(0.673)	(1.102)
Relative-to-mean	-0.05	-0.41	-0.36
P-value	0.33	0.13	0.10
BW before:	0.26	0.29	0.32
BW after:	0.31	0.31	0.29
Observations	21,817	21,817	21,817
Mean control	8.88	2.46	5.16

Notes: See Table 3 notes. Here we disaggregate the effects by 4/5-year brackets; since the number of potential receipts vary, we show additionally the relative to mean effect.

Source: DOMINO 2001-17, own calculations.

References

ABS. 2007. 2007 Australian Bureau of Statistics: Labour Force, Australia. Technical report Australian Bureau of Statistics, Canberra.
`Accessedfromwebsite:http://www.abs.gov.au/AUSSTATS/abs@.nsf/DetailsPage/`

Adda, Jérôme, Christian Dustmann and Joseph-Simon Görlach. 2022. "The Dynamics of Return Migration, Human Capital Accumulation, and Wage Assimilation." *Review of Economic Studies* 89(6):2841–2871.
`https://doi.org/10.1093/restud/rdac003`

Agersnap, Ole, Amalie Sofie Jensen and Henrik Kleven. 2020. "The Welfare Magnet Hypothesis: Evidence From an Immigrant Welfare Scheme in Denmark." *American Economic Review: Insights* 2(4):527–542.

Ahn, Taehyun. 2018. "Assessing the effects of reemployment bonuses on job search: a regression discontinuity approach." *Journal of Public Economics* 165:82–100.

Aizer, Anna and Janet Currie. 2004. "Networks or neighborhoods? Correlations in the use of publicly-funded maternity care in California." *Journal of Public Economics* 88(12):2573–2585.

Andersen, Lars Højsgaard, Christian Dustmann and Rasmus Landersø. 2019. "Lowering Welfare Benefits: Intended and Unintended Consequences for Migrants and their Families." *Discussion Paper Series CDP 05/19* .

Arendt, Jacob Nielsen, Christian Dustmann and Hyejin Ku. 2022. "Refugee migration and the labour market: lessons from 40 years of post-arrival policies in Denmark." *Oxford Review of Economic Policy* 38(3):531–556.

Auer, Daniel. 2018. "Language roulette–the effect of random placement on refugees' labour market integration." *Journal of Ethnic and Migration Studies* 44(3):341–362.

Barro, Robert J. and Jong Wha Lee. 2013. "A New Data Set of Educational Attainment in the World, 1950 to 2010." *Journal of Development Economics* 104(0):184 – 198.

Beaman, Lori A. 2012. "Social networks and the dynamics of labour market outcomes: Evidence from refugees resettled in the US." *Review of Economic Studies* 79(1):128–161.

Beerli, Andreas, Ronald Indergand and Johannes S Kunz. 2023. "The supply of foreign talent: how skill-biased technology drives the location choice and skills of new immigrants." *Journal of Population Economics* 36(2):681–718.

Bertrand, Marianne, Erzo FP Luttmer and Sendhil Mullainathan. 2000. "Network effects and welfare cultures." *The Quarterly Journal of Economics* 115(3):1019–1055.

Birrell, Bob and Samantha Evans. 1996. "Recently arrived migrants and social welfare." *People and Place* 4(2):1–11.

Blank, Rebecca M. 2002. "Evaluating welfare reform in the United States." *Journal of Economic Literature* 40(4):1105–1166.

Blank, Rebecca M. 2006. "Was welfare reform successful?" *The Economists' Voice* 3(4).

Bleakley, Hoyt and Aimee Chin. 2004. "Language skills and earnings: Evidence from childhood immigrants." *Review of Economics and Statistics* 86(2):481–496.

Boelmann, Barbara, Anna Raute and Uta Schönberg. 2021. "Wind of Change? Cultural Determinants of Maternal Labor Supply." *CESifo Wp* .

Bolhaar, Jonneke, Nadine Ketel and Bas van Der Klaauw. 2019. "Job search periods for welfare applicants: Evidence from a randomized experiment." *American Economic Journal: Applied Economics* 11(1):92–125.

Borjas, George J and Lynette Hilton. 1996. "Immigration and the welfare state:

Immigrant participation in means-tested entitlement programs." *Quarterly Journal of Economics* 111(2):575–604.

Brell, Courtney and Christian Dustmann. 2019. Immigration and wage growth: the case of Australia. In *RBA Annual Conference Papers*. Vol. 41 Reserve Bank of Australia.

Brell, Courtney, Christian Dustmann and Ian Preston. 2020. "The labor market integration of refugee migrants in high-income countries." *Journal of Economic Perspectives* 34(1):94–121.

Bursztyn, Leonardo, Alessandra L González and David Yanagizawa-Drott. 2020. "Misperceived social norms: Women working outside the home in saudi arabia." *American Economic Review* 110(10):2997–3029.

Calonico, Sebastian, Matias D Cattaneo and Max H Farrell. 2018. "On the effect of bias estimation on coverage accuracy in nonparametric inference." *Journal of the American Statistical Association* 113(522):767–779.

Card, David, Raj Chetty and Andrea Weber. 2007. "The spike at benefit exhaustion: Leaving the unemployment system or starting a new job?" *American Economic Review* 97(2):113–118.

Cattaneo, Matias D, Michael Jansson and Xinwei Ma. 2020. "Simple local polynomial density estimators." *Journal of the American Statistical Association* 115(531):1449–1455.

Couttenier, Mathieu, Veronica Preotu, Dominic Rohner and Mathias Thoenig. 2019. "The Violent Legacy of Victimization: Post-Conflict Evidence on Asylum Seekers, Crimes and Public Policy in Switzerland." *American Economic Review* 109(12):4378–4425.

Dahl, Gordon B, Katrine V Løken and Magne Mogstad. 2014. "Peer effects in program participation." *American Economic Review* 104(7):2049–74.

DellaVigna, Stefano, Jörg Heining, Johannes F Schmieder and Simon Trenkle. 2022. "Evidence on job search models from a survey of unemployed workers in germany." *Quarterly Journal of Economics* 137(2):1181–1232.

Dorn, David and Josef Zweimüller. 2021. "Migration and labor market integration in Europe." *Journal of Economic Perspectives* 35(2):49–76.

Egger, Dennis, Daniel Auer and Johannes S. Kunz. 2021. "Effects of Migrant Networks on Labor Market Integration, Local Firms and Employees." *Mimeo* .

Eissa, Nada and Hilary Williamson Hoynes. 2004. "Taxes and the labor market participation of married couples: the earned income tax credit." *Journal of Public Economics* 88(9-10):1931–1958.

Eissa, Nada and Jeffrey B Liebman. 1996. "Labor supply response to the earned income tax credit." *Quarterly Journal of Economics* 111(2):605–637.

Farooq, Ammar, Adriana D Kugler and Umberto Muratori. 2020. "Do Unemployment Insurance Benefits Improve Match and Employer Quality? Evidence from Recent US Recessions." *National Bureau of Economic Research wp27574* .

Filomena, Mattia and Matteo Picchio. 2021. "Are Temporary Jobs Stepping Stones or Dead Ends? A Meta-Analytical Review of the Literature." *IZA Discussion Papers No 14367* .

Foged, Mette, Linea Hasager and Giovanni Peri. 2022. "Comparing the Effects of Policies for the Labor Market Integration of Refugees." *National Bureau of Economic Research wp30534* .

Frijters, Paul, Michael A Shields and Stephen Wheatley Price. 2005. "Job search methods and their success: a comparison of immigrants and natives in the UK." *Economic Journal* 115(507):F359–F376.

Guiso, Luigi, Ferdinando Monte, Paola Sapienza and Luigi Zingales. 2008. "Culture, gender, and math." *Science* 320(5880):1164–1165.

Hartley, Robert Paul, Carlos Lamarche and James P Ziliak. 2022. "Welfare reform and the intergenerational transmission of dependence." *Journal of Political Economy* 130(3):523–565.

Imbens, Guido and Karthik Kalyanaraman. 2012. "Optimal bandwidth choice for the regression discontinuity estimator." *Review of Economic Studies* 79(3):933–959.

Islam, Asadul and Paul A Raschky. 2015. "Genetic distance, immigrants' identity, and labor market outcomes." *Journal of Population Economics* 28(3):845–868.

Isphording, Ingo E. and Sebastian Otten. 2014. "Linguistic barriers in the destination language acquisition of immigrants." *Journal of Economic Behavior & Organization* 105:30–50.

Katz, Lawrence F and Bruce D Meyer. 1990. "The impact of the potential duration of unemployment benefits on the duration of unemployment." *Journal of Public Economics* 41(1):45–72.

Kolsrud, Jonas, Camille Landais, Peter Nilsson and Johannes Spinnewijn. 2018. "The optimal timing of unemployment benefits: Theory and evidence from sweden." *American Economic Review* 108(4-5):985–1033.

Krueger, Alan B. 1990. "Incentive effects of workers' compensation insurance." *Journal of Public Economics* 41(1):73–99.

Lalive, Rafael. 2007. "Unemployment benefits, unemployment duration, and post-unemployment jobs: A regression discontinuity approach." *American Economic Review* 97(2):108–112.

Lalive, Rafael. 2008. "How do extended benefits affect unemployment duration? A regression discontinuity approach." *Journal of Econometrics* 142(2):785–806.

Lee, David S and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48(2):281–355.

Lindner, Attila and Balázs Reizer. 2020. "Front-Loading the Unemployment Benefit: An Empirical Assessment." *American Economic Journal: Applied Economics* 12(3):140–74.

Marinescu, Ioana and Daphné Skandalis. 2021. "Unemployment insurance and job search behavior." *Quarterly Journal of Economics* 136(2):887–931.

McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics* 142(2):698–714.

Moffitt, Robert. 2002. "Economic effects of means-tested transfers in the US." *Tax Policy and the Economy* 16:1–35.

Mogstad, Magne and Chiara Pronzato. 2012. "Are lone mothers responsive to policy changes? Evidence from a workfare reform in a generous welfare state." *Scandinavian Journal of Economics* 114(4):1129–1159.

Nekoei, Arash and Andrea Weber. 2017. "Does extending unemployment benefits improve job quality?" *American Economic Review* 107(2):527–61.

Nicoletti, Cheti, Kjell G Salvanes and Emma Tominey. 2018. "The family peer effect on mothers' labor supply." *American Economic Journal: Applied Economics* 10(3):206–34.

Parsons, Christopher and Pierre-Louis Vézina. 2018. "Migrant networks and trade: The Vietnamese boat people as a natural experiment." *Economic Journal* 128(612):F210–F234.

Ruhs, Martin. 2013. The price of rights. In *The Price of Rights*. Princeton University Press.

Saez, Emmanuel. 2002. "Optimal income transfer programs: intensive versus extensive labor supply responses." *Quarterly Journal of Economics* 117(3):1039–1073.

Spolaore, Enrico and Romain Wacziarg. 2017. "Ancestry and Development: New Evidence." *Dataset* .

Steinhauer, Andreas. 2013. "Identity, working moms, and childlessness: Evidence from Switzerland." *University of Zurich discussion paper* .

Van Ours, Jan C and Milan Vodopivec. 2006. "How shortening the potential duration of unemployment benefits affects the duration of unemployment: Evidence from a natural experiment." *Journal of Labor Economics* 24(2):351–378.

Ziliak, James P. 2015. Temporary assistance for needy families. In *Economics of Means-Tested Transfer Programs in the United States, Volume 1.* University of Chicago Press pp. 303–393.

For online publication only

A Welfare reform details

The 1997 policy reform extended the waiting period for newly arrived migrants from six months to two years. The 1997 reform to NARWP was first announced and proposed to Parliament on 23 May 1996 by the Howard government soon after it was elected in March 1996. The legislation was passed in parliament (and royal assent was received) on 4 March 1997. The policy affected prospective recipients of Parenting Allowance by requiring migrants who had arrived in Australia or who had been granted permanent residency (whichever was the later date) on or after 1 April 1996 to serve a new two-year waiting period before becoming eligible for payments. Migrants arriving before this date were exempt from the new policy, i.e., they were only required to wait six months after arrival before they were eligible to receive Parenting Allowance. Since the policy was announced (23 May 1996) after the targeted migrants (those arriving after 1 April 1996) had arrived, it made it impossible for migrants to alter the arrival date to Australia strategically.

The government's NARWP bill came into force on 4 March 1997. The announcement of this reform was delivered before the bill was passed. Specifically, the government had sent out a directive to caseworkers in all welfare agencies around the country to notify prospective welfare recipients arriving on or after 1 April 1996 that they would need to wait two years before they could access payments. The directive was worded as follows "It will not be possible for Parliament to pass the legislation before the implementation date of 1 April 1996. However, the Government intends to enact this legislation retrospectively. This means that staff will have to inform new migrant customers about the two years waiting period from 1 April even though the Act will not have changed".[27]

The government's original NARWP bill was also meant to apply to another welfare program, Unemployment benefits. The government's directive to case workers (described above) to all welfare agencies around the country included 'both' Parenting Allowance and Unemployment Benefits.However, including unemployment benefits in the bill was met with strong opposition in the Senate. As a result, the government amended the bill by pushing back the arrival date threshold for new migrants wanting to receive Unemployment benefits. Specifically, migrants arriving on or after 4 March 1997 had to wait two years before receiving Unemployment benefits.

[27] Theoretically, this means that a migrant arriving on 1 April 1996 could receive Parenting Allowance after six months of arrival and receive the payment for approximately five months before becoming ineligible again on 4 March 1997 for the payment. Realistically, this did not happen - migrants arriving on or after 1 April 1996 did not bother to go onto Parenting Allowance for the short period of eligibility.

B Additional data information

B.1 Data information

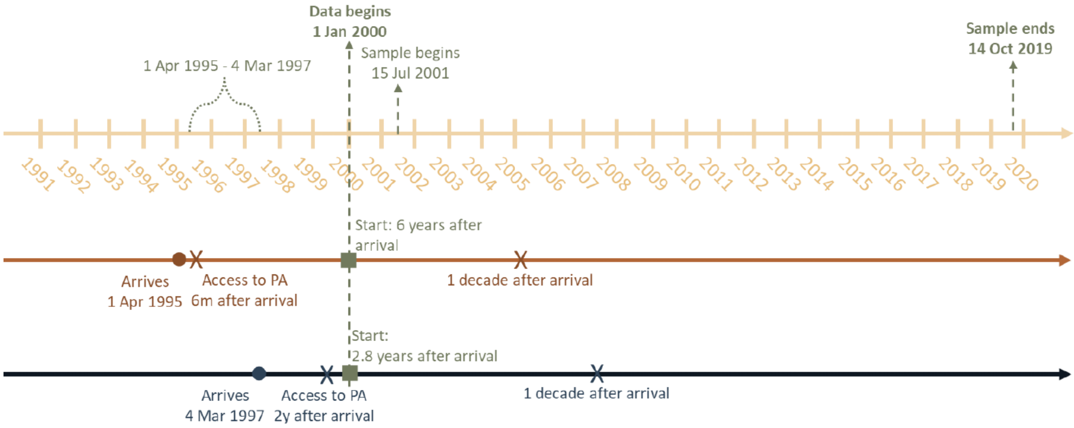

Figure B.1.1: Policy and data timeline

Note: The Figure displays a timeline of the policy and data that we employ.
Source: Australian government information, own visualization.

Table B.1.1: Variable overview

	Description	Source	Accessed
Date of arrival	Exact date of arrival	Domino	
Month on any welfare	Number of months on any welfare payment from 2006-2017	Domino	
Amount of any welfare	Amount of welfare payment received from 2006-2017	Domino	
Month on unemployment benefits	Number of months on unemployment benefits from 2006-2017	Domino	
Amount of unemployment benefits	Amount of unemployment payment received from 2006-2017	Domino	
Month on disability benefits	Number of months on disability benefits from 2006-2017	Domino	
Amount of disability benefits	Amount of disability payments received from 2006-2017	Domino	
Job quality PCA	Principal component of the following employment variables:	Constructed	
Employment	Month employed in the 15-year span	Domino	
Average annual income	Average annual income of those employed in the 15-year span	Domino	
Average tenure	Average years at same employer in the 15-year span	Domino	
Continuing contract	Indicator whether the individual has a continuing as opposed to fixed-term contract	Domino	
Non-zero-hour contract	Indicator whether individual has a non-zero-hour contract	Domino	
Age at arrival	Mothers' age at arrival in years	Domino	
Partner's age at arrival	Youngest child age at arrival in years	Domino	
Youngest child age at arrival	Partners age at arrival in years	Domino	
Country of origin	Indicators for various countries of origin	Domino	
English speaking	Indicator for official language English in the country of origin (CEPII)	link	15.02.21
GDP per Capita (PPP adjusted)	World Bank's estimate of 1995 GDP per capita-PPP adjusted in the country of origin	link	15.02.21
Total trade	Total bilateral trade country of origin with Australia in 1995 UNCOMTRADE	link	15.02.21
Total import	Total bilateral imports to Australia in 1995 from the country of origin	link	15.02.21
Total export	Total bilateral export from Australia to country of origin in 1995	link	15.02.21
Weighted FST distance	Country of origin bilateral genetic distance to Australia Pemberton et al., from Spolaore and Wacziarg.	link	17.10.18
Female share 15+ no primary	Barro Lee 1995's estimate of the share of females with no primary education in the country of origin	link	30.10.20
Female share 15+ tertiary	Barro Lee 1995's estimate of the share of females with completed tertiary education in the country of origin	link	30.10.20
Men preferred to work	Country of origin share agree in the world value survey statement: Jobs scarce: Men should have more right to a job than women all years.	link	15.02.21
Children suffer mothers working	Country of origin share agree in the world value survey statement: Pre-school child suffers with working mother all years.	link	15.02.21
The inflow of co-nationals	Total arrivals from the country of origin in 1995, Department of Homeland Affairs		
Conflict 1000 casualties	Country of origin experienced a war (more than 1,000 casualties) in 1995. POW: PRIO Battle Deaths Dataset 3.1	link	03.11.20
War in childhood	Indicator whether in the country of origin of the migrant mother was a war when they were in critical period 0-12	Constructed	
Degree of vulnerability	Regression prediction of migrant mothers characteristics of pre-treatment controls (of the month on any welfare on age own, partner's age, youngest child age, origin indicators)	Constructed	

B.2 Long run trend in type of migration

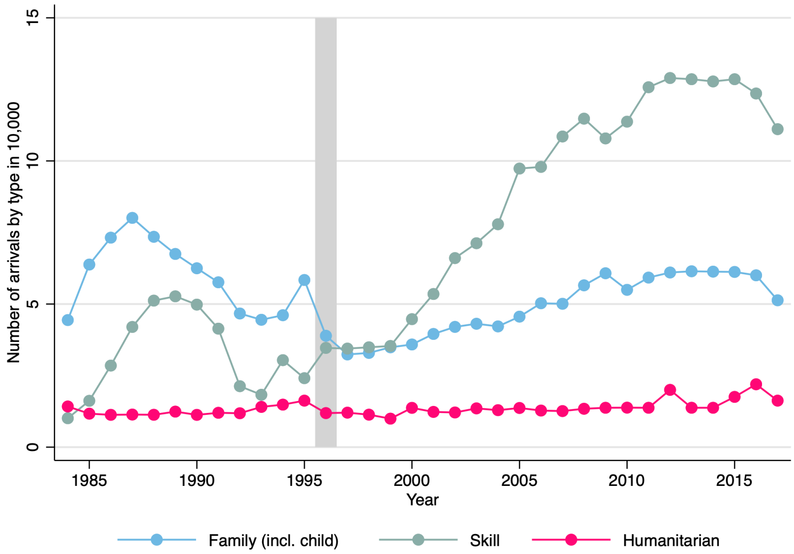

Figure B.2.1: AGGREGATE MIGRATION NUMBERS OVER TIME BY MIGRANT VISA STATUS

Note: The Figure displays aggregate migration numbers by visa stream and arrival year.
Source: Australian government information, own visualization.

B.3 Population comparison: Census 2016 and Welfare data 2016

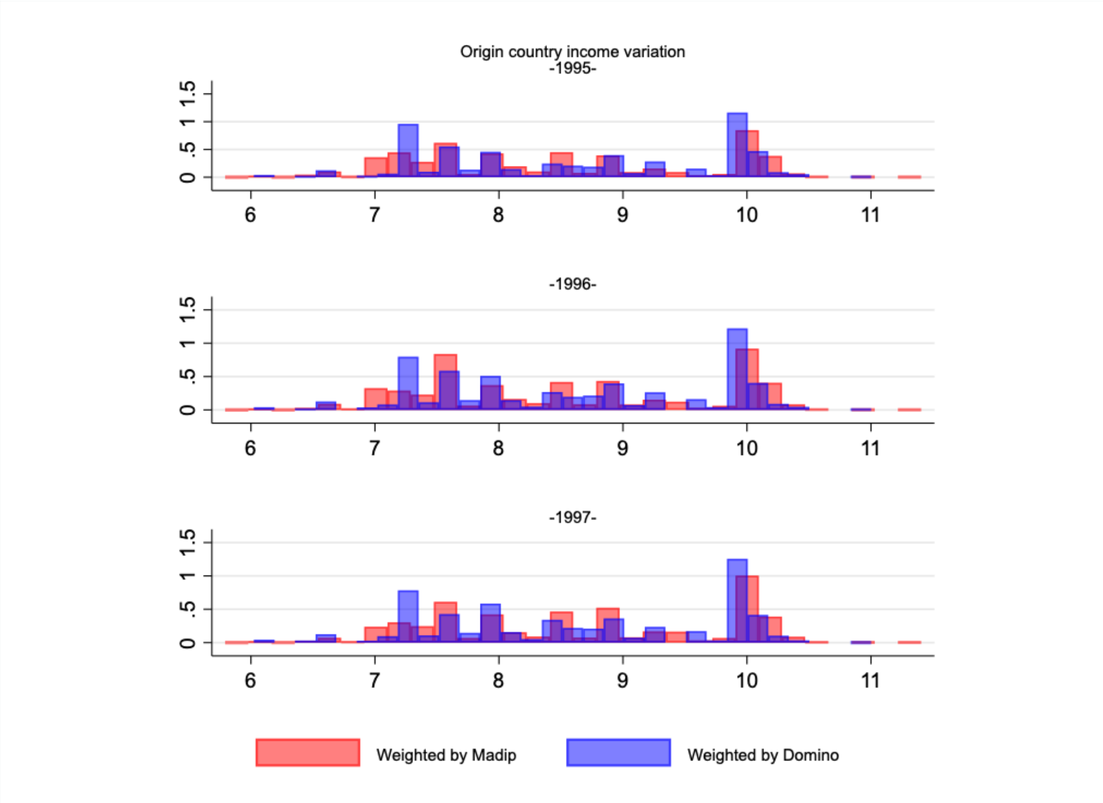

Figure B.3.1: COUNTRY OF ORIGIN GDP DISTRIBUTION

Note: The Figure displays the share of migrants from high/low-income countries using the income level in 1995. For either data-set Census-MADIP and welfare-DOMINO, we extract the share of migrants from country X (cf. Figure 2) and then merge countries X GDP per capita (logged) information to get a comparable source of information, and plot these across time, i.e., using different arrival cohorts (1995 - pre- reform, 1996 -reform year, 1997 - post-reform) but fixing the countries' GDP information in 1995.
Source: MADIP 2016, DOMINO 2016, Data World Bank, own visualization.

C Additional results

C.1 Short term: Survey

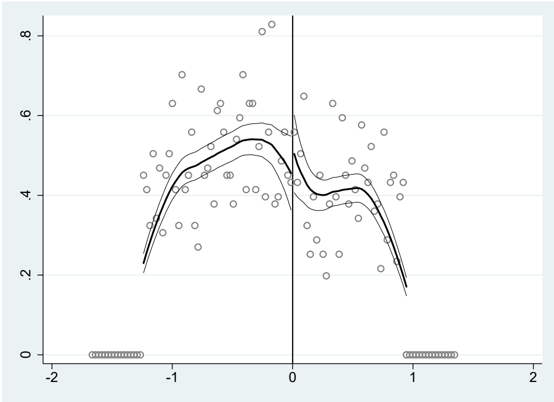

Figure C.1.1: DENSITY TEST: LDS

Note: The Figure displays the density test of the reform date for the survey results covering the initial arrival time.
Source: LDS, own calculation.

Table C.1.1: LDS results

	RDD Est. (P-values)
Primary carer of child	
Average # of months on any welfare payment in 7 to 24 months after arrival	-2.549
	(0.012)
Average # of months on Parenting Allowance in 7 to 24 months after arrival	-2.284
	(0.002)
Average # of months on Unemployment Benefits in 7 to 24 months after arrival	-0.161
	(0.591)
Sample size	1,364

C.2 Identification

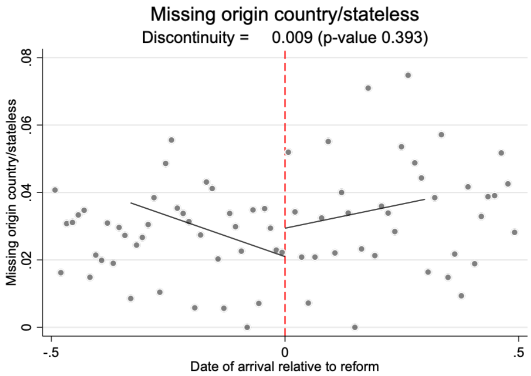

Figure C.2.1: Selection: Stateless and missing country of origin infromation

Note: The Figure displays estimates from (1) using indicators for stateless or missings in the country of origin in the sample period.
Source: DOMINO 2001-17, own visualization.

Table C.2.1: Covariate smoothness

	(1)	(2)	(3)	(4)
	GDP per Capita	Trade value in million USD total	import	export
After	767.094	483.380	243.522	184.836
	(640.316)	(282.688)	(162.150)	(149.900)
P-value	0.23	0.09	0.13	0.22
BW before:	0.19	0.32	0.33	0.30
BW after:	0.19	0.31	0.29	0.37
Observations	21,817	12,025	12,024	11,950
Mean control	8094.21	3494.89	1703.30	1800.05
	Genetic distance	English Speaking	Language Distance	Partner English
After	-0.001	0.027	-0.005	0.006
	(0.001)	(0.035)	(0.004)	(0.027)
P-value	0.25	0.43	0.19	0.81
BW before:	0.31	0.20	0.28	0.18
BW after:	0.25	0.25	0.31	0.31
Observations	19,588	21,817	20,376	21,817
Mean control	0.02	0.37	0.05	0.16
	Female Years of Schooling	Share of females no-schooling	WVS: Men preferred for jobs	WVS: children suffer when mothers work
After	0.253	-0.197	-0.001	-0.002
	(0.217)	(1.822)	(0.014)	(0.014)
P-value	0.24	0.91	0.92	0.90
BW before:	0.19	0.16	0.25	0.36
BW after:	0.21	0.25	0.29	0.28
Observations	18,101	18,101	17,653	12,797
Mean control	7.03	17.93	0.57	0.56
	Children's age at arrival	Own age	Partner's age	Degree of Vulnerability
After	0.406	0.653	0.615	-2.041
	(0.444)	(0.503)	(0.618)	(1.941)
P-value	0.36	0.19	0.32	0.29
BW before:	0.18	0.18	0.17	0.22
BW after:	0.37	0.27	0.24	0.24
Observations	21,817	21,817	21,817	21,817
Mean control	2.65	32.36	36.23	46.19
	Concurrent war	War & Conflict 3yr window war	3yr window conflict	Network size co-nationality arrivals
After	-0.010	-0.016	0.022	177.594
	(0.021)	(0.021)	(0.034)	(239.217)
P-value	0.63	0.45	0.52	0.46
BW before:	0.29	0.29	0.19	0.24
BW after:	0.27	0.30	0.40	0.31
Observations	21,117	21,117	21,817	20,916
Mean control	0.11	0.12	0.39	3747.62

Notes: The table presents coefficient estimates of the RDD model regression, using pre-arrival background characteristics.

Source: DOMINO 2001-17, own calculations.

C.3 Placebos

Table C.3.1: Placebo tests: Reform effect on 15-year long-run welfare dependence

	Women (no children)		Year prior (1 April. 1995)	
	(1)	(2)	(3)	(4)
Panel A. Month on any means-tested welfare payment (in 15-years/180 months)				
After	4.141	4.598	8.527	4.901
	(5.441)	(5.481)	(4.182)	(3.905)
P-value:	0.45	0.40	0.04	0.21
BW before:	0.32	0.30	0.25	0.29
BW after:	0.27	0.26	0.18	0.21
Observations	11,697	11,697	21,795	21,795
Mean control	43.02	42.86	46.93	45.98
Panel B. Amount of means-tested welfare payment (in 15-years/180 months)				
After	-2732.034	-1707.747	4459.830	2385.719
	(6498.835)	(6458.939)	(4880.955)	(4624.813)
P-value:	0.67	0.79	0.36	0.61
BW before:	0.35	0.34	0.29	0.34
BW after:	0.24	0.25	0.24	0.25
Observations	11,697	11,697	21,795	21,795
Mean control	45805.30	45689.23	51080.47	51078.28
Panel C. Unemployment payments (in 15-years/180 months)				
After	1.359	1.499	0.539	0.127
	(1.560)	(1.580)	(1.373)	(1.356)
P-value:	0.38	0.34	0.69	0.93
BW before:	0.29	0.28	0.25	0.27
BW after:	0.35	0.35	0.35	0.34
Observations	11,697	11,697	21,795	21,795
Mean control	7.20	7.20	9.31	9.34
Panel D. Disability payments (in 15-years / 180 months)				
After	-2.185	-2.019	1.616	-0.592
	(1.941)	(1.897)	(1.954)	(1.793)
P-value:	0.26	0.29	0.41	0.74
BW before:	0.36	0.36	0.32	0.40
BW after:	0.22	0.23	0.20	0.23
Observations	11,697	11,697	21,795	21,795
Mean control	2.56	2.64	8.29	8.20
Covariates		✓		✓
$f(T_i)$	Kernel	Kernel	Kernel	Kernel

Notes: See Table 3 notes.

Source: DOMINO 2001-17, own calculations.

C.4 Additional results

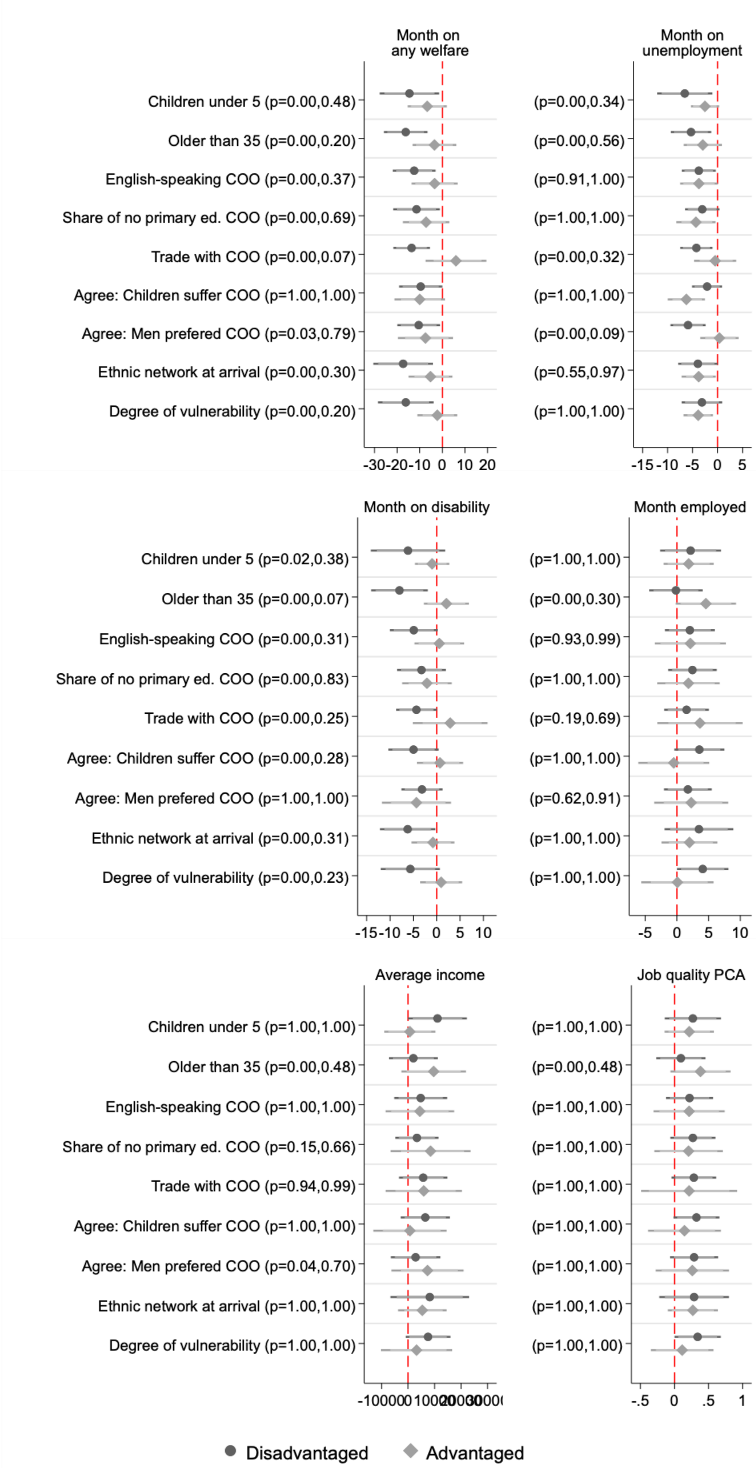

Figure C.4.1: RDD estimation: Effect on months on any income receipt by characteristic

Note: See Figure 8 notes. Significance bounds are presented in the variable labels $p = (lowerbound, upperbound)$.
Source: Domino 2001-17, own visualization.

Table C.4.1: MAIN RESULTS: AMOUNT

	Full sample		Optimal bandwidth		Constrained Bandwidth	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Amount of Unemployment payments (in 15-years / 180 months)						
After	-4854.653	-4929.506	-4291.151	-4338.764	-4204.764	-4181.598
	(1867.807)	(1860.901)	(1532.579)	(1540.749)	(1511.121)	(1504.340)
P-value:			0.01	0.00	0.01	0.01
BW before:	0.50	0.50	0.34	0.34	0.30	0.30
BW after:	0.50	0.50	0.28	0.28	0.20	0.20
Observations	11,353	11,353	21,817	21,817	21,817	21,817
Mean control	10019.66	10019.66	10461.77	10606.92	11060.12	11060.12
Panel B. Amount of Disability payments (in 15-years / 180 months)						
After	-4923.794	-5795.237	-4343.420	-4503.168	-4934.437	-4945.625
	(3508.202)	(3459.710)	(2891.703)	(2908.828)	(2785.921)	(2727.816)
P-value:			0.13	0.12	0.08	0.07
BW before:	0.50	0.50	0.31	0.26	0.30	0.30
BW after:	0.50	0.50	0.29	0.31	0.20	0.20
Observations	11,353	11,353	21,817	21,817	21,817	21,817
Mean control	12533.12	12533.12	13010.71	12928.72	13372.13	13372.13
Covariates		✓		✓		✓
$f(T_i)$	No	3rd poly	Kernel	Kernel	Kernel	Kernel

Notes: See Table 3 notes.
Source: DOMINO 2001-17, own calculations.

Table C.4.2: Reform effect on 15-year long-run welfare dependence: Amount held constant

	Months		Amount	
	Baseline	Potential amount constant	Baseline	Potential amount constant
	(1)	(2)	(3)	(4)
Panel A. Any means-tested welfare payment				
After	-9.761	-9.879	-15,361.86	-15,019.78
	(3.774)	(3.803)	(5,328.60)	(5,252.60)
P-value	0.01	0.01	0.00	0.00
BW before:	0.30	0.29	0.23	0.23
BW after:	0.31	0.32	0.24	0.26
Observations	21,817	21,437	21,817	21,437
Mean control	47.24	47.26	54,319.94	53,583.05
Panel B. Unemployment payments				
After	-3.835	-4.943	-4,291.15	-5,426.42
	(1.332)	(1.393)	(1,532.58)	(1,610.18)
P-value	0.00	0.00	0.01	0.00
BW before:	0.31	0.26	0.34	0.28
BW after:	0.34	0.31	0.28	0.26
Observations	21,817	21,437	21,817	21,437
Mean control	9.88	9.90	10,461.77	10,615.05
Panel C. Disability payments				
After	-3.264	-3.302	-4,343.42	-4,481.20
	(1.942)	(1.908)	(2,891.70)	(2,830.39)
P-value	0.09	0.08	0.13	0.11
BW before:	0.29	0.27	0.31	0.31
BW after:	0.28	0.27	0.29	0.28
Observations	21,817	21,437	21,817	21,437
Mean control	8.56	8.75	13010.71	13075.03

Notes: See Table 3 notes. Columns (2) and (4) restrict the receipt by conditioning the youngest child's age to be two years younger in the pre-treatment sample.

Source: DOMINO 2001-17, own calculations.

Table C.4.3: Reform effect on job quality over time

	6-10 yrs	11-15 yrs
	(1)	(2)
Panel A. Principal Component Job Quality		
After	0.134	0.270
	(0.149)	(0.147)
P-value	0.37	0.07
Mean control	0.007	-.011
Panel B. Employment		
After	0.091	0.082
	(0.078)	(0.082)
P-value	0.25	0.31
Mean control	0.474	0.513
Panel D. Average Annual Income		
After	2,201.46	2,448.40
	(2,118.62)	(2,397.15)
P-value	0.30	0.31
Mean control	9,075.6	11,897.5
Panel E. Average tenure with same employer (month)		
After	26.955	76.787
	(27.450)	(33.528)
P-value	0.33	0.02
Mean control	161.98	190.94
Panel G. Probability in continuing contract		
After	0.057	0.138
	(0.072)	(0.074)
P-value	0.43	0.06
Mean control	0.373	0.303
Panel J. Probability in non-zero-hour contracts		
After	0.048	0.068
	(0.026)	(0.046)
P-value	0.07	0.14
Mean control	0.047	0.093

Notes: See Table 3 notes. We estimate the model by two-period brackets and for various measures of job quality.

Source: DOMINO 2001-2017, own calculations.