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Immigrant Responses to Social Insurance Generosity ☆

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ABSTRACT

Immigrants from low-income source countries tend to be underrepresented in employment and overrepresented in social insurance programs. Based on administrative data from Norway, we examine whether such gaps reflect systematic differences in impacts of social insurance benefits on work incentives. Drawing on a reform of the temporary disability insurance program, we identify negative employment and earnings effects of higher benefits. While behavioral responses are significantly larger for immigrants, these immigrant-native differentials are largely explained by differences in earnings prospects, family structure, and job opportunities. We also uncover cross effects of benefits on the labor supply of spouses, amplifying the adverse effects on family earnings.

1. Introduction

In many European welfare states, immigrants from low-income source countries are overrepresented in social insurance (SI) programs and underrepresented in employment; see, e.g., Bratsberg et al. (2010; 2017), Sarvimäki (2011), Hansen and Lofstrom (2011), Riphahn and Wunder (2013), OECD (2013, Table 3.6), and Schultz-Nielsen (2017). Generous social insurance weakens the economic incentives to search for jobs and take up work. This paper examines whether immigrant-native participation gaps can be attributed to disincentive effects embedded in SI programs.[1] We study the impacts of SI generosity among participants in one of the largest social insurance programs in Norway, the temporary disability insurance (TDI) program. A major reform of the benefit computation formula in 2002 generated considerable variation in benefits across observationally similar participants entering just before or just after the reform, enabling us to identify the causal effects of the TDI benefit level on transitions to employment and future earnings.

Why would we expect social insurance generosity to affect immigrants from low-income countries differently from natives? In our analyses, we explore three potential mechanisms. First, immigrants tend to enjoy less consumption gain from employment, due to a combination of relatively low labor earnings prospects and a progressive social insurance benefit schedule. If social insurance claims rise with the replacement ratio due to moral hazard, as indicated by the literature (see, e.g., Krueger and Meyer, 2002; Johansson and Palme, 2002; Røed and Zhang, 2003; Henrekson and Persson, 2004), this would yield higher social insurance take-up (and lower employment) rates among immigrants. Second, non-pecuniary costs of working may be higher for immigrants if they face less attractive employment opportunities and have a higher valuation of non-employment. More valuable time outside paid work relates to family structure and larger households in particular, and also to attachment to a foreign country with lower living expenses. Together with the higher social insurance replacement ratios, this implies that the utility difference between employment and non-employment may be systematically smaller for immigrants than for natives. Based on a simple theoretical framework – where individuals are allowed to choose freely between employment and social-insurance-supported non-employment in a frictionless labor market – we point out that a marginal change in benefits will affect the realized state only for individuals who initially were on the fence between the alternatives of employment and social insurance. Consequently, we expect the behavioral responses to marginal benefit variations to be heterogeneous, with particularly large effects for individuals who are close to indifference between employment and non-employment.

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[1] This perspective complements existing evidence on employment gaps. Immigrants are more exposed to precarious employment relationships and job loss caused by adverse shocks to the employer (Bratsberg et al., 2018a). There is also evidence on economic downturns in general and involuntary job loss in particular, showing that consequences are more adverse for immigrants than for natives, in part because of their poorer general labor market and majority language skills (Røed and Zhang, 2003; Barth et al., 2004; Bratsberg et al., 2010; Dustmann et al., 2010).

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The third potential mechanism relates to the availability of realistic job opportunities. Social insurance generosity can influence individual employment and earnings patterns only insofar as such opportunities exist. For the TDI claimants studied in this paper, this is not necessarily the case. Health impairments may effectively limit the range of feasible employment opportunities, particularly in thin local labor markets. This restriction from the demand side may be more or less binding for immigrants than for natives. The fact that immigrants from low-income countries tend to have lower education, less host-country work experience, and limited majority language skills, points to larger employment barriers for immigrants. However, the fact that they are heavily overrepresented in the central parts of Norway with considerably thicker and more diverse labor markets, points in the other direction.

A supplementary mechanism relates to the value of joint leisure within households and the impact that benefit generosity may have on the labor supply of the spouse of the claimant. If higher benefits raise the total income of the claimant him/herself – also taking into account any negative labor supply responses – we expect the effect on the spouse's labor supply to be negative, both through a direct income effect and through a substitution effect caused by leisure complementarities.

To our knowledge, no prior study has explicitly examined the differences between immigrants and natives in their labor supply responses to benefit generosity. Our choice of the temporary disability insurance program as the foundation for the empirical analysis is motivated by importance as well as possibility. TDI is by far the largest among the Norwegian programs compensating for temporary income loss, with a caseload more than twice as large as the unemployment insurance program. And even though participants in this program have health impairments that undermine their current work capacity, they are still considered to have a possibility of returning to some form of regular work in the (near) future. The second reason for focusing on the TDI program relates to a major reform in 2002, that generated quasi-experimental variation in benefit generosity across participants, such that entrants with exactly the same individual characteristics could obtain quite different benefit levels due to small differences in the timing of the health impairment. Because we can calculate the exact pre and post-reform benefits for each individual, even for the counterfactual state, we can use a difference-in-differences type strategy where both the actual and the hypothetical benefit levels are allowed to correlate with outcomes at all times, but where the causal effect is identified as the "extra" statistical association stemming from the periods where each benefit-schedule actually applies.[2] This way, we exploit the quasi-experimental features of the reform to examine the effects of the benefit level on various outcomes of program participants, including the transition rate to employment and subsequent earnings for up to 10 years after program entry.

We build on the recent study of the Norwegian TDI program by Fevang et al. (2017), which reports average impacts of TDI benefits on the duration and outcome of TDI spells. Our study allows for differential responses by immigrant status and extends the analysis to effects of TDI benefits on earnings of program participants as well as their spouses over a ten-year period following program entry. We also examine how individual characteristics that convey information about potential earnings capacity (such as the highest earnings obtained in prior years), the value of leisure (as captured by family structure), and presumed job finding prospects (as captured by actual work experience and geographical location), interact with benefit generosity.

We confirm that the TDI benefit level on average has a negative influence on the transition rate to regular employment for both natives and immigrants. In addition, we identify considerable negative effects on subsequent labor earnings. Equally important in our context, our results show that the behavioral responses are much larger for immigrants from low-income countries than they are for natives. For example, while a 10% increase in the benefit level is predicted to reduce the hazard rate from TDI to regular employment by 3.1% for native men and by 1.0% for native women, it reduces the hazard rate for immigrant men and women by 6.8% and 5.0%, respectively. Further, while a one Euro increase in the (annualized) benefit level reduces the average annual labor earnings over the next five years by 11 cents for native men and 6 cents for native women, it reduces earnings of immigrant men and women by 33 cents and 23 cents, respectively.

The average responsiveness of a group is greater the larger is the fraction of its members that is close to indifference between employment and non-employment. We show that heterogeneous effects according to earnings potential and family structure explain a considerable part of the observed response differential between immigrants and natives. Allowing for heterogeneous effects also along the dimensions of past work experience (as a proxy for employability) and geographical location, further reduces the immigrant-native response differential to statistical as well as substantive insignificance. The larger benefit sensitivity observed for immigrants than for natives appears to be driven by differences in social and economic circumstances, and should not be taken as conclusive evidence for behavioral differences, given such circumstances.

Since social insurance benefits change the budget constraint as well as preferences for market and non-market activities, more generous TDI benefits may also affect the labor supply of the claimant´s partner. Except for native female claimants, our findings show that the earnings of the spouse drop in response to higher TDI benefits. As for the direct impacts on TDI claimants, the negative earnings effect appears to be larger for immigrants than natives. The spousal responses imply that the negative effects on earnings are amplified when we consider outcomes like family earnings.

The negative earnings effect of higher TDI benefits appears to endure for many years, again with native women as an exception. For immigrants, the evidence points toward a permanent effect with statistically significant negative impacts estimated as long as 10 years after program entry. Even if social insurance benefit receipt mitigates the long run effects on after-tax income, the reduction in labor supply dominates and the net effect of increased TDI benefits on after-tax income is negative for families.

The paper proceeds as follows. In the next section, we start out with an overview of relevant literatures. Section 3 presents a descriptive overview of the income distributions of immigrants and natives in self-supporting employment and social-insurance-supported non-employment, respectively. Section 4 then presents our theoretical considerations, while Section 5 describes the temporary disability insurance program that forms the basis for our empirical analysis. Section 6 presents our data and our identification strategy. Section 7 examines the impacts of the benefit level on transition rates out of TDI and annual labor earnings over a 10-year period after program entry. Section 8 examines the sources behind differential behavioral responses in terms of earnings prospects, family situation, and employment opportunities, and evaluates the role that these factors have in accounting for immigrant-native response differences. Section 9 examines the impacts on the earnings of the claimant's spouse and on after-tax household income. Conclusions are given in section 10.

We emphasize that when we use the term "immigrant" in the remainder of this paper, we refer to immigrants from low-income source countries.[3] Immigrants from other (high-income) countries are dropped from our empirical analysis. The reasons for this are, first, that immi-

[2] A parallel approach has been used in studies of impacts of unemployment benefits on unemployment duration in Norway and Sweden (Røed et al., 2008); the impact of student aid on college enrolment in Denmark (Nielsen et al., 2010); and the impact of disability insurance benefits on labor supply in Norway (Fevang et al., 2017) and Austria (Mullen and Staubli, 2016).

[3] In our samples, immigrants from Iran, Iraq, Pakistan, Somalia, and Turkey form the largest source country groups. The vast majority were admitted as refugees or through family immigration.

grants from high-income countries have a much higher probability of leaving the country, implying that it is difficult to follow them over time in our data, and second, that their tendency to enter and leave the country is highly dependent on their employment status. Moreover, our data indicate that the labor market behavior of immigrants from high-income countries is very similar to that of natives (Bratsberg et al, 2017).

2. Related literature

For those on social insurance programs, more generous benefits reduce the economic gains from exiting the program for employment. As such, our study relates closely to the literature examining the effects of the unemployment insurance (UI) on moral hazard, which shows that the duration of UI receipt tends to rise in response to an increase in the replacement ratio (see, e.g., Krueger and Meyer, 2002; Johansson and Palme, 2002; Røed and Zhang, 2003; Henrekson and Persson, 2004). Our paper is also linked to the broader literature on labor supply effects of disability insurance (DI) (e.g., Bound and Burkhauser, 1999) and, in particular, the studies indicating that there is substantial work capacity among marginal DI claimants (Autor and Duggan, 2003; Maestas et al., 2013; Kostøl and Mogstad, 2014; Borghans et al., 2014; French and Song, 2014). There is a considerable grey area between the roles of the UI and DI programs (for Norwegian evidence, see Rege et al., 2009 and Bratsberg et al., 2013). Studies of labor supply effects of DI fall into two main categories. One branch estimates the effects of receiving (or being denied) DI (e.g., French and Song, 2014), while others identify effects of benefit generosity (e.g., Marie and Castello, 2012) or effective taxes on wage income for those on the program (Kostøl and Mogstad, 2014).

A related strand of the literature shows that labor supply tends to be particularly elastic at the bottom of the wage distribution (e.g., Aaberge et al., 2000; Bargain et al., 2014). High labor supply elasticities in this group have indeed been shown to stem from responses at the extensive margin, where the alternative state of claiming social insurance benefits may appear attractive as the economic gains from labor force participation can be low.

While studies of intra-household labor supply typically examine the effects of partner´s wage or non-wage income (e.g., Blundell and MaCurdy, 1999; Devereux, 2004; Blau and Kahn, 2007), recent analyses of social insurance effects also address the impacts of the claimant's social insurance benefits on the labor supply of the partner. Evidence suggests that unemployment insurance crowds out spousal labor supply (Cullen and Gruber, 2000). Spouses of (randomly) rejected Norwegian DI applicants increased their labor supply (Autor et al., 2019), and a reform-induced increase in DI benefits for Vietnam-era veterans caused their spouses to reduce their labor supply (Duggan et al., 2010). We estimate the labor supply responses of spouses to changes in benefits, adding to the literature on the effects of social insurance generosity beyond the behavioral responses of the claimant.

Our study contributes to the scarce evidence on heterogeneous effects of SI. A study of particular relevance for our paper is Mullen and Staubli (2016), who, based on Austrian benefit reforms, evaluate the elasticity of disability program participation with respect to the prospective benefit level. For the population at large, the authors estimate an elasticity of about 1.2. However, in contrast to the present study, the Austrian study finds that the responsiveness with respect to the benefit level tends to be greater for persons generally considered to be more resourceful, i.e., white-collar workers are more responsive than blue-collar workers, and individuals with high lifetime earnings are more responsive than poorer individuals. The authors interpret these findings as reflecting the better labor market opportunities of the former groups. In line with this, Kostøl and Mogstad (2014) report larger effects of a return-to-work program among DI claimants for those with more education, more labor market experience, and higher pre-program earnings. Other studies uncover no evidence of greater responses among the highly skilled. French and Song (2014) find that DI receipt reduces labor force participation less among college graduates than non-graduates. Maestas et al (2013) report employment effects of DI program admission that are stable across the earnings history distribution, but find heterogeneous effects according to the health condition of the applicant — from zero effect among those with severe impairments to a 50 percentage points reduction in employment for entrants with the least severe health problems.

It is noteworthy that none of the studies described above explicitly examine differences in labor supply responses to social insurance generosity according to immigrant status. The paper that comes closest to ours in this respect is that of Kaestner and Kaushal (2005), who compared the influences of the US Temporary Assistance to Needy Families (TANF) program on the labor supply of foreign-born and native-born low-skilled women, but without finding evidence of significant response differences. There are also a few papers addressing responses within the immigrant population. These include studies of US welfare reforms during the mid-1990s that placed limitations on the welfare eligibility of immigrant households. Drawing on variation in compensating state-funded programs, Borjas (2003) finds that immigrants responded to welfare cutbacks by increasing their labor supply. Kaushal (2010) shows that banning Supplemental Security Income increased employment among elderly foreign-born men, but not among women. In a recent study of immigrant responses to Food Stamp eligibility, East (2018) shows that access to the program reduced the employment rate of single women by 6% and of married men by 5%.

3. Income sources of immigrants and natives

To motivate our analysis, we begin by showing that social insurance is a considerably more important source of income for immigrants than for natives. Based on complete population data for 2013, Table 1 reports the fraction of working-age male and female immigrants and natives with employment (E) or social insurance (SI) as their main source of income (defined as the higher of the two), as well as the average total pre and post-tax incomes of each group. The table reveals that SI is particularly important for immigrants along two dimensions. First, more immigrants have SI as their main source of income. Fully 22.0% of immigrant men receive most of their income from social insurance compared to 11.0% of native men. For immigrant women, 22.7% have social insurance as their main income source, compared to 14.4% for native women.[4] Second, the income differential between the employed and social insurance claimants is much smaller for immigrants. For example, while employed native men on average have a disposable (after tax) income that is almost NOK 219 000 higher than those on social insurance (92% higher), the corresponding differential for immigrants is only 131 000 (60%). [5] The smaller income differential for immigrants does not arise because the SI benefits are particularly large, but reflects substantially lower earnings among those employed.

These figures suggest that pecuniary gains from employment can be small for immigrants, although the numbers in Table 1 cannot be taken as individual replacement ratios. Those observed in employment are likely positively selected from the underlying population in terms of earnings potential, which means that actual replacement ratios are even higher than those implied by Table 1. Even if selection into employment differs for immigrants and natives, the observed patterns of employment and social insurance incomes clearly indicate that the economic rewards for being employed are much larger for natives than for immigrants.

4. Theoretical considerations

Even if SI programs typically specify a set of eligibility criteria related to involuntary unemployment or disability, there is significant scope for

[4] Note that the shares in the two income groups do not sum to 100, as a residual group have zero income from both sources; the latter is the case for fully 12.5% of immigrant women.

[5] The average Euro/NOK exchange rate in 2013 was € 1=NOK 7.81.

Table 1
Incomes within and outside employment, 2013.

	Men		Women	
Main income source	Immigrants	Natives	Immigrants	Natives
Employment (E)				
Share (%)	71.1	87.6	64.9	84.2
Income before tax	477 657	649 859	374 925	462 719
Income after tax	349 932	455 527	288 126	344 725
Social insurance (SI)				
Share (%)	22.0	11.0	22.7	14.4
Income before tax	252 999	280 571	242 408	272 234
Income after tax	218 582	236 652	218 734	238 148
Income ratio before tax (SI/E)	0.530	0.432	0.647	0.588
Income ratio after tax	0.625	0.520	0.759	0.691
Income difference after tax	131 350	218 875	69 392	106 577

Note: Populations are restricted to ages 25-62, with the immigrant population further restricted to those at least three years in the country. Native population is reweighted to have the same age distribution as immigrants. The main income source is defined as the higher of the two (labor earnings and social insurance transfers) during the calendar year.

individuals to influence their program participation. Applying for social insurance is a matter of choice, and once admitted to a program, the decision to exit is in practice often left to the claimant. In order to focus on the role of individual preferences, in this section we abstract from restrictions on eligibility, endogenous search efforts, reservation wages, and human capital investments. As in Saez's (2002) model of labor market participation, agents select the better of the two alternatives of self-supporting employment and social insurance program participation. Employed individuals receive a wage W and a non-pecuniary benefit α (positive or negative). A social insurance program participant receives benefits B. With linear utility, employment is preferred to program participation iff $W + \alpha > B$ The non-pecuniary component α is the net utility of employment, including the pleasure from work, the self-respect, the value of the social network that comes with it (Gill, 1999; Kieselbach, 2004), any lack of stigma associated with social insurance (Moffitt, 1983), and the (negative) values of the lost leisure or home production hours.

For persons with well-paid, decent, and even interesting jobs, utility derived from employment will typically by far exceed the value of social insurance program participation. The work incentives will not be affected by realistic changes in the social insurance benefit. A person with low wage and poor working conditions is more likely to be at the margin where a reform with marginal adjustment of the benefit level B will alter her/his preferred alternative.

To discuss mechanisms more formally, let $(W + \alpha)$ vary across individuals within group g (where g denotes immigrants or natives), and let $F_g(.)$ be cumulative distribution functions for the value of employment. Let B_g represent benefits, for simplicity assumed to be common to all members of group g. Then, the fraction preferring program participation over employment is simply given by $F_g(B_g)$ Three predictions follow directly from this setup:

(i) For given $F_g(.)$, program participation is more frequent the larger is the benefit level B_g.
(ii) For given B_g, program participation is more frequent the higher is $F_g(B_g)$.
(iii) The effect of a marginal change in benefits, B_g on the program participation rate equals the derivative of F_g with respect to B_g, or $f_g(B_g) > 0$, where f_g is the density function for group g. Within a given group, the sensitivity of labor supply with respect to change in B will be greater the larger is the fraction of group members that initially are "on the fence" between employment and non-employment.

The empirical counterparts to these theory elements are intrinsically unobserved, as neither benefit entitlements for those in employment, nor the potential earnings for those on benefits are realized.

We do observe the conditional after-tax income distributions of immigrants and natives (Figure 1), by main source of income (as in Table 1). While the typical annual income for those with social insurance as their main source of income lies between NOK 240 000 and 280 000 for both natives and immigrants, it is clear that, compared to natives, a much higher fraction of employed immigrants earn less than the median benefit among SI claimants.

We also see from Figure 1 that $f_g(B_g)$ tends to be higher for immigrants; i.e., that a larger fraction of immigrants finds themselves in the region where the monetary payoff from employment is close to the payoff from social insurance. To illustrate, the share of employed men with income below the 75th percentile income of the SI group is 26% for immigrants compared to 12% among natives. For women, the corresponding numbers are 42% for immigrants and 24% for natives. Moreover, among those employed the share with income between the median and the 90th percentile incomes of the SI distribution is 30% and 47% for immigrant men and women, respectively, but only 17% and 24% for native men and women. Based on these numbers, our simple theoretical framework predicts that immigrants have higher rates of SI dependency than natives *and* that they are more responsive than natives with respect to marginal adjustments of SI benefits.

The incentives to work also depend on the non-wage component, α, which reflects the (dis)utility from employment as well as the value of household production and leisure. Although α is intrinsically unobservable, there are several reasons to expect the net value of employment to be smaller (i.e., more negative) for immigrants than for natives. First, evidence indicates that immigrants to a larger extent than natives are mismatched in the labor market with jobs where tasks do not correspond with qualifications (e.g., OECD, 2015, section 6.4), possibly due to inferior networks, employer discrimination due to prejudices or asymmetric information, and non-transferability of education across countries. It naturally follows that immigrants are less likely to enjoy pleasure from work (i.e., lower α). Second, immigrants disproportionally live in households with a homemaker spouse and dependent children, possibly adding to the value of leisure (reducing α). Existing empirical evidence, largely based on retirement decisions, point toward considerable complementarities in spouses' leisure and that this force for leisure coordination typically dominates the offsetting income effect on labor supply; see, e.g., Coile (2004), Gustman and Steinmeier (2004), and Schirle (2008). As many immigrants from low-income countries maintain an attachment to their origin country, the value of joint leisure is reinforced by the prospect of spending time together in locations where the consumption value of Norwegian benefits is high. Finally, as benefit entitlements – unlike wages – depend positively on the number of dependents, persons with a low α tend to have a high B. In a group where

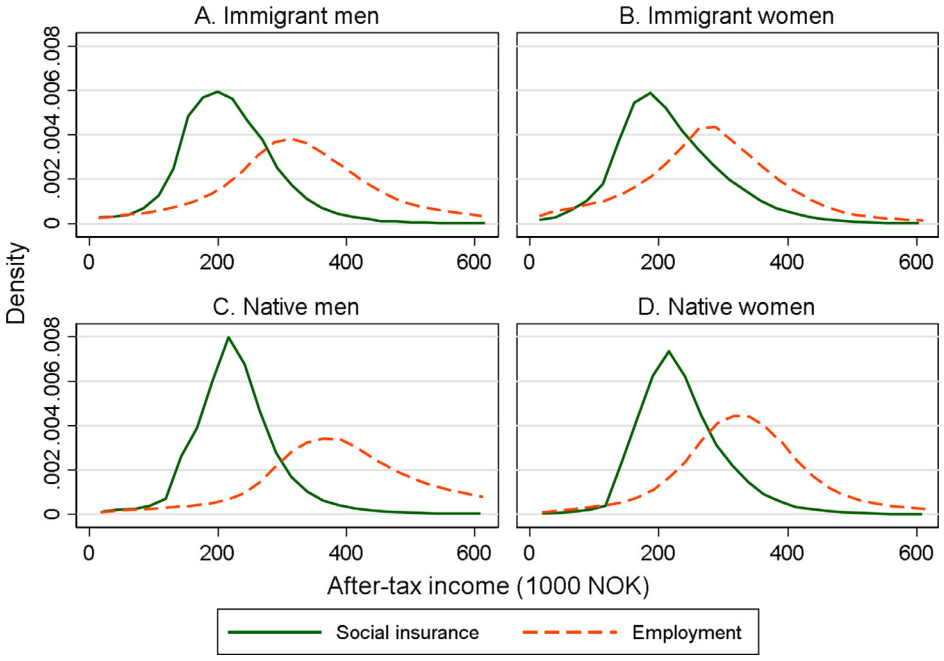

Fig. 1. Distribution of after-tax income for immigrants and natives, by gender and main income source, 2013
Note: The graphs show the distributions of total income, including earnings from employment and social insurance transfers. Employment indicates that labor earnings exceed sum of transfers, while the opposite holds for the social insurance group (see Table 1). Population is restricted to ages 25-62, with immigrants further restricted to those in the country at least three years. Native population is reweighted to have the same age distribution as immigrants.

the (potential) participant tends to be married and have children, the distribution of $B - \alpha$ will be wider and a larger fraction will prefer program participation to employment.

Our theoretical framework ignores that employment opportunities can be limited. Indeed, prior studies interpret empirical evidence on heterogeneous responses to DI benefit generosity as related to differences in employment opportunities rather than in preferences (Kostøl and Mogstad, 2014; Mullen and Staubli, 2016). Individuals with lesser resources might be less responsive to benefit changes simply because their choice set of available job opportunities is limited or even empty. If immigrants on average are more restricted in their choices between employment and non-employment than natives, we would expect immigrants to be less responsive to changes in benefits. Such reasoning contrasts with the predictions from our theory, where the incentives embedded in the social insurance system predict that immigrants from low-income countries will be more responsive to benefit changes than natives. However, job opportunities are not only determined by individual resources, but also by local labor market conditions. Moderate health problems may represent a much larger employment barrier in thin than in thick labor markets. As immigrants from low-income countries tend to be underrepresented in thin rural labor markets, this may be a force for stronger responses among immigrants than natives.

5. The temporary disability insurance program

In order to compare the benefit responsiveness of immigrants and natives and to identify the underlying sources of any differences between the two groups, we focus on the Norwegian temporary disability insurance (TDI) program.[6] We identify causal impacts of marginal changes in TDI benefits on the claimants' transition rate to regular employment and future labor earnings. The TDI program has grown in importance and has more than doubled in size over the last 20 years, and its caseload is now more than twice as large as that of the unemployment insurance program. While the ratio of immigrants to natives in 1996 was similar among TDI claimants as in the population (about 0.035), the growth of immigrant TDI participation has been much larger than what can be accounted for by their rising share of the working-age population. In 2012, the immigrant-native ratio was 0.15 among TDI claimants compared to 0.10 in the population.

The TDI program is part of what the Norwegian social insurance system offers workers with health-related problems. Before entry to TDI, employees are entitled to sickness benefits that typically provide 100% wage compensation and protection against displacement on grounds related to their sickness for up to 12 months. If the health impairment prevails, the TDI program provides benefits to employees with exhausted sick pay entitlements. The TDI program also provides benefits to individuals who are not eligible for sick pay at the time of disablement. To be eligible for TDI, a physician must certify that health impairment is the main cause of loss of at least 50% of the work capacity. However, as the law explicitly states that actual employment opportunities may be considered relevant in the assessment of the health impairment, social insurance caseworkers will consider labor market opportunities and other worker characteristics in their evaluation of program eligibility. As we show below, there is considerable variation in individual benefit entitlements. For persons with stable past earnings and no responsibility for children, the benefit level typically amounts to 60-70% of prior earnings, but the minimum and maximum thresholds as well as child allowances often generate large deviations from this level.

The TDI program offers activities to enhance employability through periods of medical and/or vocational rehabilitation. During our observation period, there was no definite limit on the overall length of TDI.[7] Although we do not evaluate the effectiveness of TDI activities, it is clear that, if the benefit level affects program participation, the effectiveness of rehabilitation activities will influence the overall impacts of TDI benefits on future outcomes. Existing evaluations of the vocational rehabilitation program indicate considerable individual heterogeneity in treatment effects, yet relatively modest impacts on average; see Aakvik et al. (2005) and Markussen and Røed (2014).

[6] Temporary disability insurance programs include "attføring" (1992-2010), "rehabilitering" (2002-2010), "foreløpig uførestønad" (1992-2010), "tidsbegrenset uførestønad" (2004-2010), and "arbeidsavklaringspenger" (2010-2016).

[7] A time limit of four years (with room for exceptions) was introduced in March 2010. However, this was implemented such that ongoing spells at the time of the introduction were granted four *additional* years. As the last entrants in our data started the program in 2004, this has a negligible influence on the spells analyzed in our paper.

Individuals who do not successfully return to work from the TDI program often end up with a permanent disability insurance (DI), with DI benefits that are similar to those of the TDI program, typically at about two thirds of prior earnings.

6. Identification strategy and data

In January 2002, the formula for computation of individual TDI benefits was completely renewed. As a consequence, two persons with identical labor market histories and the same demographic characteristics—and with the only difference that they entered the program before or after the reform—could be entitled to quite different TDI benefits. For some claimants, the reform implied higher benefits if they entered the program after the reform. For others, it implied lower benefits if they entered after the reform. The main changes in the benefit formula were that i) benefits became based on earnings during the past three years only, instead of the complete earnings history; ii) the minimum benefit level was raised; iii) child allowances were considerably reduced, but no longer means-tested; and iv) immigrants obtained full entitlement after only three years of residence instead of partial entitlement determined by their age at immigration (see Fevang et al. (2017) for further details on the reform). With our access to detailed administrative register data, we can accurately compute the hypothetical post-reform benefits for individuals in the pre-reform regime and vice versa.

The essence of our identification strategy is most easily explained within a simple two-period model where we abstract from all sources of time-variation in outcomes apart from those corresponding to the implementation of the TDI reform (we will add vectors of time dummies in the empirical specifications below). Let Y_i be some outcome of interest observed for both pre-reform and post-reform TDI entrants, and let Z_i be a vector of control variables. Furthermore, let b_i^{PRE} and b_i^{POST} be the benefit levels (or their logs) calculated for person i based on the pre-reform and post-reform formulas, respectively. Assuming that the outcomes can be related to the benefit levels both through non-causal (spurious) and causal associations, we can specify outcome equations separately for pre-reform entrants ($i \in N^{PRE}$) and post-reform entrants ($i \in N^{POST}$) as

$$Y_i = Y_i^{PRE} = \alpha^{PRE} + Z_i\gamma + (\beta^{PRE} + \delta)b_i^{PRE} + \beta^{POST}b_i^{POST} + u_i^{PRE},\ i \in N^{PRE}, \tag{1}$$

$$Y_i = Y_i^{POST} = \alpha^{POST} + Z_i\gamma + \beta^{PRE}b_i^{PRE} + (\beta^{POST} + \delta)b_i^{POST} + u_i^{POST},\ i \in N^{POST}. \tag{2}$$

The coefficients β^{PRE} and β^{POST} capture the non-causal associations between benefits and the outcome variable, whereas δ captures the causal effect. The parameters α^{PRE} and α^{POST} represents the period-specific constant terms. We immediately see that the causal effect of TDI benefits cannot be identified from each equation alone. A sample-specific coefficient on actual benefits will capture the causal effect as well as non-causal association ($\beta^{PRE} + \delta$ for pre-reform entrants and $\beta^{POST} + \delta$ for post-reform entrants). However, by combining pre and post-reform entrants, we can exploit that the actual benefit shifts from b_i^{PRE} to b_i^{POST} between the two samples. Let R_i be a dummy variable equal to 1 for a person belonging to the post-reform regime, and otherwise 0. We can then combine Equations (1) and (2) and write an empirical specification for the pooled data of pre and post-reform entrants as

$$\begin{aligned} Y_i &= (1 - R_i)Y_i^{PRE} + R_iY_i^{POST} \\ &= (1 - R_i)\alpha^{PRE} + R_i\alpha^{POST} + Z_i\gamma + \beta^{PRE}b_i^{PRE} + \beta^{POST}b_i^{POST} \\ &\quad + \delta b_i^{ACTUAL} + u_i, \end{aligned} \tag{3}$$

with

$$b_i^{ACTUAL} = (1 - R_i)b_i^{PRE} + R_ib_i^{POST} \text{ and}$$
$$u_i = (1 - R)u_i^{PRE} + Ru_i^{POST},$$

which highlights the difference-in-differences nature of the foundation for identification.[8] Equation (3) also illuminates our identifying assumption: The residual u_i must be uncorrelated with the actual benefit level b_i^{ACTUAL}, conditioned on b_i^{PRE} and b_i^{POST} (and other observed characteristics). In words, any difference in residuals between pre- and post-reform entrants must be uncorrelated with the reform-initiated difference in their entitled benefits. Hence, we do *not* require that the reform's impacts on individual benefit levels are randomly assigned with respect to unobserved characteristics, but we need to assume that the changes is benefits across individuals were orthogonal to any difference in the influence of unobserved characteristics from the pre-reform to the post-reform period.

Identification is thus obtained by exploiting the variation across individuals in the way the reform affected entitled benefits. Without such idiosyncratic variation, Equation (3) would be subject to perfect multicollinearity. This can most easily be seen by noting from Equations (1) and (2) that the expected difference between a post-reform and a pre-reform outcome, conditional on the covariates, is equal to

$$E\left[Y_i^{POST} - Y_i^{PRE} | b_i^{PRE}, b_i^{POST}, Z_i\right] = \alpha + \delta\left(b_i^{POST} - b_i^{PRE}\right), \tag{4}$$

with $\alpha = \alpha^{POST} - \alpha^{PRE}$. Hence, any parallel changes in outcomes over time will be captured by α and therefore *not* interpreted as a reform effect. We will show that the difference between benefits computed using post- and pre-reform rules ($b_i^{POST} - b_i^{PRE}$) vary considerably according to individual characteristics. Because the consequences of the reform for entitled benefits depend on a number of individual characteristics, it is natural to use a difference-in-differences design to identify the causal effect of interest rather than, say, regression discontinuity or event study designs.

Our empirical analyses are based on complete administrative registers covering all entrants to the TDI program over a six-year period, from three years before to three years after the 2002 reform (1999-2004). Ongoing spells will be followed through 2009, but we also extract labor market outcomes such as earnings through 2014. We limit the analysis population to individuals 27 to 59 years of age at the time of TDI entry. The lower age limit is set because there are separate benefit rules for persons below 27 with particularly serious sickness diagnoses, and our data do not allow us to identify this group. The higher age limit is set because rehabilitation attempts typically cease as claimants approach their 60s, and because very few TDI claimants at this age return to work. Finally, we drop TDI claimants who were employed in the public sector just prior to entry, as public-sector workers were eligible for supplemental benefits and largely sheltered from the 2002 reform. Table 2 presents some descriptive statistics. A point to note is that the replacement rate – defined here as the annualized level of benefits divided by the average earnings obtained in the three-year period prior to TDI entry – is on average considerably higher for immigrants than for natives, particularly when based on the post-reform benefit schedule. For example, while male immigrant TDI claimants on average received benefits corresponding to 80% of their pre-entry labor earnings, native men received "only" 65% on average. The reason for this is clearly not that average benefits are higher for immigrants (quite the opposite), but rather that their earnings over the last three years were much lower.

To illustrate how the reform affected the benefits of native and immigrant TDI program participants, Figure 2 shows density plots, separately for each group, of the difference between benefits calculated with the new and old formula, $b_i^{POST} - b_i^{PRE}$ or the hypothetical gains in benefits from the reform. It is clear that there were winners and losers in

[8] Our identification strategy is similar in spirit to, but also different from, the approach used by Gruber and Saez (2002) and Kleven and Schultz (2014) to estimate the elasticity of taxable income drawing on variation caused by tax reforms. While these studies use the counterfactual tax rate based on pre-reform behavior to instrument for the actual tax rate, we use counterfactual benefit levels as additional control variables.

Table 2
Descriptive statistics, TDI program participants.

	Men		Women	
	Immigrants (1)	Natives (2)	Immigrants (3)	Natives (4)
Age (year)	40.0	40.4	39.9	40.4
Educational attainment (%)				
Compulsory	40.5	42.0	46.7	40.5
Upper secondary	33.0	50.0	28.4	45.6
College	15.4	6.2	14.0	12.4
Post graduate	2.8	1.0	2.7	1.1
Unknown	8.2	0.8	8.3	0.4
Employed year before (%)	74.2	86.2	72.0	83.4
Avg. earnings 3 prior years (NOK)	264 695	380 300	204 891	267 696
TDI benefits (NOK):				
Pre-reform rules	158 881	227 417	125 113	172 352
Post-reform rules	210 578	250 443	179 469	199 768
Implied replacement (pre-tax):				
Pre-reform rules	0.620	0.593	0.606	0.642
Post-reform rules	0.798	0.654	0.878	0.744
Spell duration (months)	24.8	25.0	27.7	29.3
Spell outcome:				
Employment	32.9	49.9	26.8	42.7
Permanent disability	15.2	17.0	16.0	19.0
Unemployment	4.4	1.9	2.9	1.5
Non-participation	32.6	15.9	34.9	16.0
Spell in progress 48 months	14.9	15.4	19.4	20.9
Number of spells‘	7 128	64 346	5 267	67 909
Fraction post reform	59.0	54.4	63.2	54.8

Note: Samples consist of new temporary disability insurance spells of individuals age 27-59 that started between Jan 1, 1999 and Dec 31, 2004. Earnings and benefits are inflated to 2013 currency using the social insurance base amount (“G”) index.

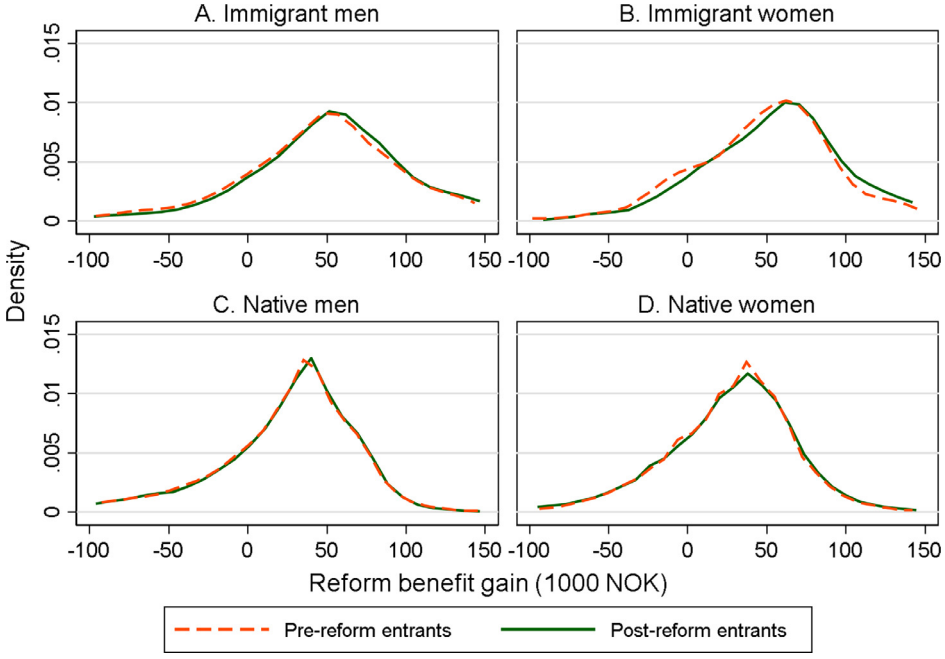

Fig. 2. The distribution of hypothetical change in TDI benefits from the 2002 reform, by gender and immigrant status
Note: The TDI benefit gain is defined as the benefit level calculated according to new rules minus the benefit level calculated according to old rules ($b_i^{POST} - b_i^{PRE}$) TDI benefits are inflated to 2013 currency using the social insurance base amount (“G”) index.

all groups considered, but that the majority of claimants came out with higher benefits if they entered the program after the reform. Immigrants gained more than natives, but the gains among immigrants were also more dispersed.

The key assumption behind the identification strategy is that the benefit gain (the difference between benefits computed according to post-reform and pre-reform rules) is uncorrelated with the *change* in the outcome variable other than through its causal effect. This is tantamount to the common trend assumption in standard difference-in-differences analyses. While Equation (3) effectively controls for all sources of stable non-causal correlation between pre and post-reform benefits and the outcome of interest as well as for common trends, it does not control for *differential trends* in outcomes that may be correlated with changes in benefit generosity. As shown in Appendix A, there is no evidence of diverging pre-trends in earnings outcomes between those with above and below median reform-initiated change in the benefit level. Moreover, a placebo analysis described in Appendix B, where we reproduce our main results based on the imposition of falsely timed reforms within the pre and post-reform periods, fails to indicate the presence of such correlated trends.

The benefit gain distributions in Figure 2 are similar for pre- and post-reform entrants, suggesting that there was little room for strategic timing of program entry and also that the incentive structure had little effect on the composition of program entrants. This is important for the internal validity of our analysis, as large incentive effects on the inflow to TDI could potentially have altered the associations between unobserved characteristics and the two benefit variables, thus invalidating our identification strategy. In Appendix C, we show that the reform-initiated benefit changes had only small effects on the probability of entering the TDI program during the period covered by our analysis and that the effects are not significantly different by immigrant status. Small effects can be explained by imperfect information as potential claimants had little knowledge about their precise benefits prior to actual entry, especially during the initial period following the reform.

7. Effects on exit rates from TDI and future earnings

We first present estimates of the effects of the TDI benefit level on exit rates from the program to regular employment and other destinations. More than four in five participants exit the program within four years, but far from all have a transition to employment (Table 2). While 50% and 43% of native men and women exit to a job, immigrants are even less likely to leave the program for employment (33% and 27% of immigrant men and women, respectively). The TDI program has no fixed duration. To account for alternative outcomes within a competing risk framework, we specify a non-parametric mixed multivariate proportional hazard rate model (MMPH), with exit to employment and non-employment as destination states.[9]

The hazard rate model is based on monthly data, using the competing risks estimation strategy and optimization algorithm described in Gaure et al. (2007). In this model, we follow each TDI spell on a monthly basis, from the month of entry and until it ends with an exit to either employment or non-employment. As we observe labor market status by the end of each month only, we set up the statistical model in terms of grouped hazard rates. We write the integrated month-specific hazard rates φ_{kit} (k=employment or non-employment) as functions of the benefit level and other explanatory variables in line with the identification strategy illustrated in Equation (3); i.e.:

$$\varphi_{kit} = \int_{t-1}^{t} \theta_{kis}\, ds = \exp\left(Z_{it}\gamma_k + \beta_k^{PRE} b_i^{PRE} + \beta_k^{POST} b_i^{POST} + \delta_k\left[(1-R_i)b_i^{PRE} + R_i b_i^{POST}\right] + v_{ki}\right),$$
$$k = \text{employment, non-employment} \qquad (5)$$

where the benefit variables are included in log-form and Z_{it} also incorporates a number of time-varying and time-invariant variables, including quarterly calendar month and spell-duration dummy variables (absorbing the period-specific intercepts in Equation (3)), and variables describing individual characteristics and local labor market conditions; see the note to Table 3 for details. The model also accounts for time-invariant unobserved heterogeneity in the form of a discrete distribution of destination-specific intercepts (v_{ki}) with no restrictions on the correlation structure and with an a priori unknown number of support points (this number is determined through the estimation procedure based on the Akaike information criterion). In order to save space, we do not display the likelihood function, but refer to Røed and Westlie (2012) who present in detail a model with the same technical structure. The model is estimated separately for men and women, with both immigrants and natives in the two estimation samples. Apart from the unobserved mass points, all parameters including the responses to the benefit level are allowed to differ between immigrants and natives.

The estimated effects of the benefit level on the hazard rates are displayed in Table 3, panel A. Since the benefit variables in Panel A enter in log form, the coefficients can be interpreted as elasticities, i.e., the percent change in the respective hazard rate caused by a one percent change in benefits. A first point to note here is that higher benefits have a negative effect on exits from the TDI program to both employment and non-employment. The marginal benefit effect is particularly large for transitions to employment. (Actually, with the exception of native women, the estimated effect of benefits on exit to non-employment is not statistically significant.) Consequently, benefits that are more generous imply that participants in all four groups stay longer in the program.

Benefit generosity has a much larger effect on exits to employment for immigrants than for natives. For immigrant men, the elasticity of exit to employment with respect to benefits is -0.680, compared to -0.312 for natives. For women, the employment exit elasticities are -0.500 for immigrants and -0.096 for natives. The estimated difference in employment elasticities between immigrants and natives is highly significant for both genders.

While the hazard rate model only captures the first transition out of the program, benefit generosity may have persistent effects on labor market outcomes such as earnings, that extend beyond a temporary postponement of the employment transition. The mechanisms behind the effect on hazard rates, such as reduced search intensity and increased reservation wage, are also likely to influence subsequent job match quality. Moreover, the extended TDI duration may itself impact employment opportunities, either because the rehabilitation activities offered by the program affect human capital acquisition positively, or because extended participation implies depreciation of human capital and/or increased stigma (scarring). The net earnings effect also depends on the effectiveness of the vocational rehabilitation activities that is an integrated part of the TDI program.

We examine labor earnings effects for up to 10 years after TDI entry by means of linear regressions based on Equation (3), with annual earnings (measured in NOK and inflated to 2013 currency) as the outcome. Empirical earnings equations typically appear in logs, but in our application studying log earnings would leave out a large fraction with zero earnings that obviously represents a valid outcome. We have thus chosen to "de-log" benefits and regress annual labor earnings on the benefit levels. The estimated equations include the same individual characteristics and interaction terms as the hazard rate models, as well as time dummy variables (for the month of TDI entry); see the note to Table 3 for details.

Given the large number of yearly effects estimates, we first give a graphical overview. Figure 3 displays the estimated marginal causal effects (with their 95 % confidence intervals) from separate year-by-year regressions, where year zero indicates the year of TDI program entry. We uncover negative effects of benefits on labor earnings for all four groups, and again the negative effects are more severe for immigrants than for natives. The dynamic pattern is similar across groups and the effects are largest the first three years after TDI entry, which corresponds to a typical program period, consistent with the finding that higher benefits lead to a postponement of the transition to employment.

To facilitate a comparison between immigrants and natives, we sum earnings in years 1 to 5 (short-term outcome) and years 6-10 (long-term outcome), and estimate the effect of the TDI benefit level on average earnings over each of these periods. The short-term earnings effect combines the impact during the program period, as well as the effects on employment and wages over the first subsequent years, whereas the long-term effect primarily captures post-program outcomes. The estimates displayed in Table 3, Panels B and C, confirm that higher benefits do reduce average labor earnings during subsequent years for all four groups, both over the short and long-term horizons. The earnings effects are, however, quite different across groups. First, the effects for men are

[9] In appendix D, we report estimates from an extended model specification where the non-employment state is split into the three components of permanent disability insurance (PDI), unemployment (UI), and non-participation. The chief advantage of the parsimonious structure used in Table 3 is that it facilitates modelling program exit with heterogeneous benefit effects. We return to this issue in the next section.

Table 3
Estimated effects of TDI benefit level on exit rate from program and average earnings 1-5 and 6-10 years after program entry, by gender and immigrant status.

	Men			Women		
	Immigrants (1)	Natives (2)	Difference (3)	Immigrants (4)	Natives (5)	Difference (6)
A. Exit rate from program						
To employment						
Elasticity estimate	-0.680***	-0.312***	-0.368***	-0.500***	-0.096***	-0.404**
	(0.146)	(0.068)	(0.161)	(0.140)	(0.051)	(0.152)
To non-employment						
Elasticity estimate	-0.161	-0.087	-0.074	0.053	-0.163***	0.216*
	(0.103)	(0.063)	(0.121)	(0.111)	(0.054)	(0.123)
Support points		5			4	
Number of spells	7 128	64 346		5 267	67 909	
B. Earnings 1-5 years after program entry						
Effect estimate	-0.328***	-0.106***	-0.221***	-0.229***	-0.058**	-0.170**
	(0.066)	(0.032)	(0.082)	(0.072)	(0.025)	(0.083)
Observations	6 791	61 345		5 128	65 999	
C. Earnings 6-10 years after program entry						
Effect estimate	-0.286***	-0.081**	-0.204*	-0.218**	-0.001	-0.218**
	(0.087)	(0.040)	(0.105)	(0.095)	(0.033)	(0.107)
Observations	6 507	58 256		5 016	64 166	

*/**/*** statistically significant at the 10/5/1% levels.
Note: Standard errors are reported in parentheses. Estimates in Panel A are based on separate models for men and women, each including both immigrants and natives; reported are the coefficients of Equation (5) with benefit variables in log form. Estimates in Panels B and C, columns (1), (2), (4), and (5) are from separate estimations of Equation (3), with outcome 5-year averages of real earnings and with benefit variables in levels. Estimates in columns (3) and (6) are from pooled samples of immigrants and natives. In all models, control variables include educational attainment (5 dummy variables), age (33), family characteristics (5), county of residence (19), and local labor market conditions at program entry. Panel A adds controls for time-varying calendar quarter (40), spell duration by quarter (13), and local labor market conditions in the current quarter, while Panels B and C add dummy variables for month of program entry (72). The local labor market condition is computed from the observed transition rate from unemployment to employment in the claimant's own commuting zone. In pooled models, all variables are interacted with an indicator for immigrant status. Samples in Panels B and C are restricted to those present in Norway and below 61 years of age 5 and 10 years after program entry.

larger than for women, consistent with the greater benefit effect on male exit to employment uncovered in Panel A. Second, earnings effects are substantially larger for immigrants than for natives. While an extra benefit of one Euro reduces native earnings by less than 10 cents (and has no impact at all on native women in the long run), the immigrant earnings drop is around 20-30 cents. The differences between immigrants and natives are highly significant regardless of gender and time horizon.[10]

8. Why are immigrants more responsive to benefit generosity?

Why do changes in TDI benefits have a stronger impact on employment and earnings of immigrants than natives? To examine the roles of labor market opportunities as well as valuation of leisure, we define four sets of indicator variables grouping individuals according to their (predicted) earnings prospects, valuation of non-employment, and their chances of finding work. To characterize the claimants' earnings potential, we identify the three best earnings years prior to program entry and compute the average earnings level in these years.[11] We then divide the immigrant population into quartiles based on these averages, and use the resultant earnings thresholds to categorize natives as well. The "high earnings potential" is thus associated with having had earnings above the 75th percentile of the immigrant earnings distribution.

Table 4, Panel A, describes how earnings potentials differ between immigrants and natives. By construction, exactly 25% of the immigrant claimants are defined as having low earnings potential. Among natives, only 3.7% of men and 5.1% of women fall into this group. Hence, if past earnings predict future earnings potentials, native TDI participants have much more to gain from labor market participation than immigrants.

To characterize the claimants' predicted valuation of non-employment, we use marital status and employment status of the spouse, the latter to account for leisure complementarities within couples. Since employment of the spouse is endogenous, we use information on the spouse's labor market status the year prior to TDI entry. We assume that the value of leisure is highest for claimants with a non-employed (homemaker) spouse and lowest for those who are single. This is of course an extremely crude and imprecise representation of the utility of leisure, yet the approximation is likely to capture some systematic differences in the valuation of non-employment. Panel B of Table 4 clearly shows that immigrant and native TDI claimants also differ considerably in their family situation, even beyond the married/single distinction. In particular, the fraction with a homemaker spouse is more than four times as high for immigrants as for natives.

To characterize the claimants' chances of finding work, we exploit data on actual work-experience in Norway and on geographical location. More specifically, we use a dummy variable indicating a minimum of 10 years of work experience and another dummy variable indicating resi-

[10] Given that the reform affected the incentives of immigrants much more than of natives, it appears possible that the immigrant-native response differentials identified in Table 3 are driven by some form of non-linearity. It turns out, however, that a more flexible specification does not change the estimated differentials very much. For example, if we formulate the effects on the 1-5 year earnings outcome in Panel B in terms of a quadratic function, the immigrant-native differential (evaluated at mean actual benefits for immigrants) becomes -0.197 for men (compared to -0.221 in Table 3) and -0.211 for women (compared to -0.170 in Table 3).

[11] In this exercise we include earnings years up to 12 years prior to entry, but exclude the two years immediately preceding program entry; i.e., we use the three best earnings years during the period of [-12,-3] years of the year of entry.

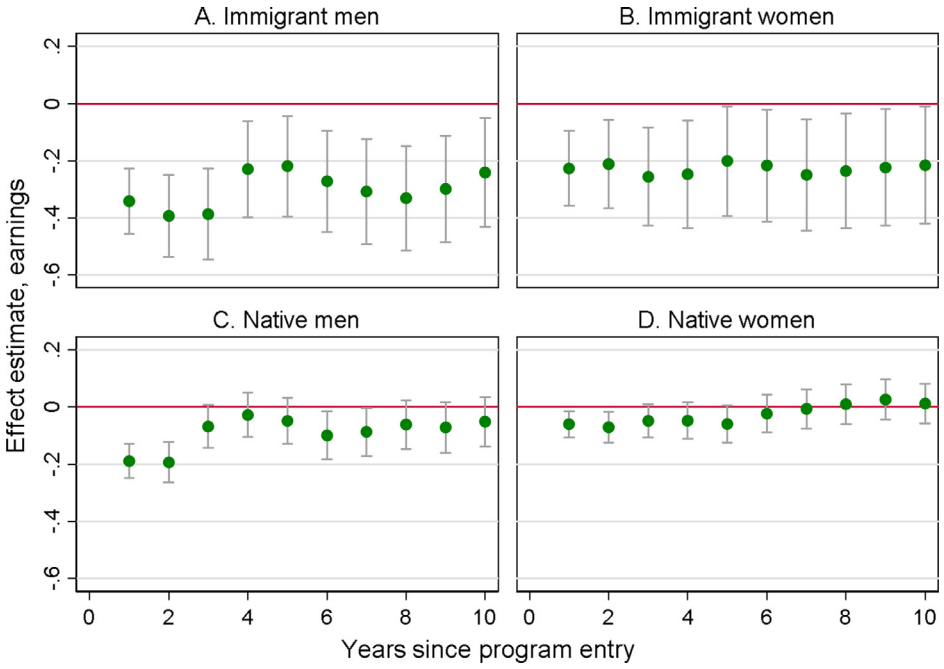

Fig. 3. Estimated effects of one Euro increase in TDI benefit on annual labor earnings
Note: The vertical lines depict 95% confidence intervals around point estimates. In addition to the two hypothetical benefit variables, the vector of control variables includes educational attainment (5 dummy variables), age (33 dummy variables), family characteristics (5 dummy variables), month of program entry (72 dummy variables), county (19 dummy variables), and local labor market conditions. Samples are restricted to those in Norway at the end of the calendar year; sample sizes in years 1 and 10 are 7 055 and 6 506 (Panel A), 5 229 and 5 014 (Panel B), 63 290 and 58 251 (Panel C), and 66 964 and 64 164 (Panel D).

Table 4
Proxies for labor market opportunity and valuation of employment and non-employment.

	Men		Women	
	Immigrants (1)	Natives (2)	Immigrants (3)	Natives (4)
A. Earnings potential				
Low (p1-p25 of immigrant distribution)	25.0	3.7	25.0	5.1
Medium (p26-p75 of immigrant distr)	50.0	39.4	50.0	45.5
High (p76-p100 of immigrant distribution)	25.0	56.9	25.0	49.4
B. Family status				
Single	33.1	65.4	30.1	53.3
Employed spouse	30.2	26.0	47.4	41.4
Spouse homemaker	36.7	8.6	22.4	5.3
C. Actual labor market experience>=10 year	33.8	79.5	22.4	67.7
D. Residence in capital region	47.4	15.1	45.1	17.8

Note: Table entries give the sample share in each cell. Earnings potential is measured by the average of the three best earnings years between 12 and 3 years before TDI entry. The thresholds between low and medium earnings are NOK 79 920 for men and NOK 27 240 for women, and those between medium and high earnings potential NOK 431 078 for men and NOK 316 672 for women. Actual labor market experience reflects years with accrued pension points in the Norwegian public pension scheme at the time of TDI program entry. The capital region consists of Oslo and Akershus counties. Samples consist of new temporary disability insurance spells of individuals age 27-59 that started between 1999 and 2004.

dence in the capital region of Norway.[12] Panels C and D of Table 4 show that immigrants have much less work experience than natives and are heavily overrepresented in the capital region.

To see whether these crude indicators for the valuation of employment and non-employment and for job finding prospects can explain the immigrant-native differential in benefit responsiveness, we specify a regression model where we allow the effect of actual (as well as hypothetical) benefit levels to vary along these characteristics; i.e., we interact the three benefit variables with each of the indicator variables listed in Table 4. Coefficient estimates shown in Table 5, Panel A, stem from a pooled regression model encompassing both immigrants and natives, but estimated separately for men and women. In this exercise, we focus on the short-term earnings outcome (average earnings 1-5 years after program entry), and estimate a version of Equation (3) where we restrict the coefficients of the interaction terms between benefits and the indicator variables in Table 4 to be the same for immigrants and natives.[13] All other explanatory variables are interacted with an immigrant dummy. The purpose of this exercise is then to see how much of

[12] This parsimonious specification is motivated by ease of interpretability. The consequences for the immigrant-native response differential are largely the same if we include a full set of county dummies (instead of the capital region dummy) and a third-order polynomial of years of actual work experience (instead of the dummy for at least 10 years of experience).

[13] A similar exercise for the hazard rate model is provided in the working paper version of this article (Bratsberg et al., 2018b).

Table 5
Estimated heterogeneous effects of TDI benefit level on earnings 1-5 after program entry in pooled samples of immigrants and natives, by gender.

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Regression coefficients						
Actual benefits	-0.106***	-0.384***	-0.427***	-0.058**	-0.303***	-0.322***
	(0.032)	(0.139)	(0.143)	(0.025)	(0.114)	(0.114)
Actual benefits*immigrant	-0.221***	-0.151*	-0.054	-0.170**	-0.115	-0.045
	(0.082)	(0.088)	(0.095)	(0.083)	(0.084)	(0.088)
Other benefit interactions						
Medium earn potential		0.168	0.135		0.035	0.050
		(0.117)	(0.119)		(0.090)	(0.091)
High earn potential		0.293**	0.228*		0.177**	0.195*
		(0.120)	(0.127)		(0.090)	(0.097)
Spouse employed		0.037	0.035		0.173**	0.170**
		(0.086)	(0.086)		(0.080)	(0.080)
Single status		0.048	0.069		0.098	0.113
		(0.084)	(0.084)		(0.082)	(0.082)
Work experience>=10			0.115			0.031
			(0.071)			(0.050)
Capital region			-0.115*			-0.144***
			(0.064)			(0.046)
B. Weighted average effect of actual benefits						
Immigrants	-0.328***	-0.348***	-0.336***	-0.229***	-0.242***	-0.233***
	(0.066)	(0.077)	(0.077)	(0.072)	(0.080)	(0.080)
Natives	-0.106***	-0.197***	-0.282***	-0.058**	-0.127***	-0.188***
	(0.032)	(0.048)	(0.059)	(0.025)	(0.035)	(0.040)
Comment		Weighted with immigrant characteristics			Weighted with immigrant characteristics	

*/**/*** statistically significant at the 10/5/1% levels.

Note: Standard errors are reported in parentheses. Samples are restricted to those present in Norway 5 years after program entry. Sample sizes are 6 791 immigrant men, 61 345 native men, 5 128 immigrant women, and 65 999 native women. All models include the following controls: Benefits according to old and new formulas, educational attainment (5 levels), age (33), family characteristics (5), month of program entry (72), county (19), and local labor market conditions, all interacted with an immigrant dummy. The model in columns (2) and (5) interact the benefit variables with 3 levels of predicted earnings potential and 3 household categories; the models in columns (3) and (6) add interactions with indicators for at least 10 years of actual experience and residence in the capital region. Where applicable, average effects in Panel B are evaluated at the weighted average immigrant frequency count across earnings potential, household structure, experience, and region cells.

the immigrant-native response differential that can be "explained away" by the employment valuation and job opportunity indicators in Table 4.

The results of these exercises are shown in Table 5. For comparability, columns (1) and (4) first repeat the baseline results from Table 3, showing that the estimated negative effect of a one Euro increase in the (annualized) benefit level on annual labor earnings 1-5 years after TDI entry is 22 cents larger for immigrant men than for native men and 17 cents larger for immigrant women than for native women. However, when we allow for differential responses along the indicators for earnings potential and family structure, the estimated immigrant-native response differential falls by approximately 30% in absolute value for both men and women, and renders it statistically insignificant for women; see columns (2) and (5). Of note, for men it is earnings potential that seems to be of critical importance for the responsiveness with respect to the TDI benefit level. This fits well with the theory set out in Section 4, as high earnings potential also becomes a proxy for being far from indifferent between employment and non-employment. Similarly, we find evidence that high earnings potential significantly reduces the responsiveness to benefits among women. But for women, family structure also appears to be of some importance, as the estimated benefit responsiveness is less severe for women who are single or who live with an employed spouse. If leisure complementarities imply that a homemaking spouse increases the valuation of non-employment, this can again be interpreted as a reflection of larger distance to indifference between employment and non-employment.

Finally, adding into the model interaction terms between the benefit level and the proxies for employability and local labor market thickness further reduces the estimated immigrant-naïve response differential toward zero, and renders the differentials for both men and women economically as well as statistically insignificant; see columns (3) and (6). Employability – as proxied by actual work experience – seems to reduce responsiveness, which appears to contrast with predictions from theory. The estimates are not statistically significant, however, and a plausible interpretation is that past work experience not only captures chances of finding work, but also the valuation of existing job opportunities; hence there is no clear-cut distinction between the probability of finding work and the valuation of it. The thickness of the labor market – as proxied by living in the capital region – has a considerable impact on the responsiveness to benefit changes, though. As pointed out in Section 4, access to a larger and more diverse labor market is likely to diminish the employment barriers imposed by moderate health problems, and thus increase the scope for individual choice.

To summarize, when we account for heterogeneous benefit effects with respect to a set of rather crude indicators of job chances and valuation of employment versus non-employment, the estimated response differential between immigrants and natives approaches zero, and it is no longer statistically significant. In Panel B, we report the average effect of the benefit level on earnings, weighted with immigrant characteristics. In the extended model specification, the average effects are very similar for immigrants and natives. It appears that the larger responsiveness among immigrants observed in the prior section is primarily explained by two factors. First, immigrants face a much lower future earnings potential in the labor market, implying that immigrants are more likely to be close to indifference between employment and non-employment. And second, the immigrant population is heavily overrepresented in the capital region with relatively good and diverse employment opportunities, which implies that even though available jobs may be of low quality, they do exist.

The interaction effects reported in Table 5 also provide a benchmark for interpreting the magnitude of the immigrant-native benefit effect differentials. For example, for both men and women, we note that the overall estimated differentials of -0.22 and -0.17 reported in columns

Table 6
Descriptive statistics, married TDI program participants and their spouses.

	Men		Women	
	Immigrants (1)	Natives (2)	Immigrants (3)	Natives (4)
Age	40.9	44.0	40.0	42.6
Spouse age	36.5	41.4	44.5	45.3
Spouse immigrant (%)	89.2	6.0	83.9	3.2
Employed year before (%)	78.8	95.9	76.2	88.5
Spouse employed year before (%)	46.5	75.7	71.0	90.6
Avg. earnings 3 prior years (NOK)	289 812	463 345	211 900	277 697
Spouse's average earnings 3 prior years (NOK)	135 701	257 889	323 210	521 533
Number of observations (couples)	4 278	21 249	3 479	30 237

Note: Samples consist of married entrants to the TDI program between 1999 and 2004 and their spouses. Samples are restricted to claimants age 27-59 at the time of entry and to spouses not in the TDI program and with less than NOK 500,000 in capital income. The latter restriction is to avoid extreme outliers in the analyses of total household income, reducing the sample by 2.3%. Average earnings are inflated to 2013 currency using the social insurance base amount ("G") index.

(1) and (4) almost exactly correspond to the difference associated with high versus low earnings potentials, see columns (3) and (6).

The results in Table 5 are based on the assumption that the benefit interaction terms have the same effects on immigrants and natives. In Appendix E, we assess this assumption by estimating a more flexible model, where the interaction terms are allowed to affect immigrants and natives differently. Although point estimates vary somewhat between immigrants and natives, none of the equality restrictions is rejected by the data at conventional levels of significance. Together, the interaction terms included in columns (3) and (6) in Table 5, imply that the estimated benefit responsiveness take 36 different values, depending on the combination of covariates. In Appendix E, we report all these 36 estimates, both based on the restricted model (with the same interaction effects for immigrants and natives) and the flexible model.

9. Effects on spousal earnings and household income

From a household perspective, higher TDI benefits change the budget constraint and will influence outcomes via preferences for market and non-market activities. Consequently, the benefit level may also affect the labor supply of the claimant´s spouse. First, if the claimant's overall income level rises with the increase in the TDI benefit level (i.e., when the additional benefits outweigh the adverse effect on claimant earnings), there is a negative income effect on spousal labor supply. Second, if the claimant's own labor supply declines with higher benefits (as indicated by the results presented in previous sections), there is a substitution effect that is negative if hours spent at home enter the partner's utility function as a complement (e.g., due to valuation of joint leisure), and positive if they enter as a substitute (e.g., due to decreasing returns in home production).

In order to examine spousal responses, we limit the analysis to married claimants (living with the spouse) at the time of TDI program entry. This reduces the native sample by as much as 60%, but reduces the immigrant sample by "only" 35%, reflecting the much larger share of married persons in the immigrant claimant group. Table 6 reports summary statistics for the resultant sample. It is notable that the vast majority of female claimants, whether immigrant or native, have an employed spouse, whereas a slight majority of the male immigrant claimants have an economically inactive spouse.

Figure 4 displays the estimated effects of TDI benefits on the spouses' annual labor earnings. Although many of the coefficients are individually insignificant, the overall pattern of point estimates paints a rather consistent picture. Except for native women, the earnings of the spouse drop in response to higher TDI benefits. As for TDI claimants themselves, the negative earnings effect appears to be larger for spouses of immigrants than natives. This is confirmed by Table 7, where we again show results for earnings averaged over five-year periods. For the period immediately following TDI entry, we find that the cross effect of higher benefits for male claimants is -0.171 for immigrant spouses and -0.085 for native spouses. Similar, and even slightly larger, effects are estimated for the following five years. The differences between immigrants and natives are, however, not statistically significant.

For the spouses of female TDI claimants, the effects differ for immigrants and natives. The earnings of immigrant spouses drop (as for spouses of male claimants), and the estimated cross effects of higher benefits on the labor supply of non-claimant immigrant husbands are even larger than the direct impacts on their own earnings. Over the first five-year period, our estimates suggest that spouses of immigrant women reduce their own earnings by as much as 55 cents for each extra Euro that she receives in TDI benefits. And this negative effect persists almost at the same level in the subsequent five-year period. In contrast, the earnings effect for husbands of native female claimants is slightly positive, both over the short and long-term horizons. For spouses of female TDI claimants, the different responses between immigrants and natives are thus highly significant, both from economic and statistical viewpoints.

The cross effects on spouses suggest that the earnings effects of increased benefits will be amplified, or modified, when we extend the impacts to the family. In Table 7, we also report estimated effects on the average earnings of the family (sum of both spouses). For immigrants, an increase in the TDI benefit level of one Euro leads to an estimated drop in average family earnings over the first five years of 52 cents for men and 78 cents for women, and similar effects persist into the subsequent five-year period. Even for native men, the reduced labor supply of their wives reinforces the negative effect on individual earnings and the effect on family earnings is a reduction of 27 cents during the first five years. For native women, however, the increased labor supply of the non-claimant husband cancels out any individual response, leaving family earnings largely unaffected by higher benefit levels.

The results in Figure 4 and Table 7 indicate some interesting asymmetries in men's and women's valuation of joint leisure and/or in the structure of joint home production, particularly within native families. For immigrant couples, the strong decline in husbands' labor supply suggests that that the value of leisure of immigrant men increases quite substantially when the wife reduces her labor supply in response to higher benefits. The effects go in the same direction, but are consid-

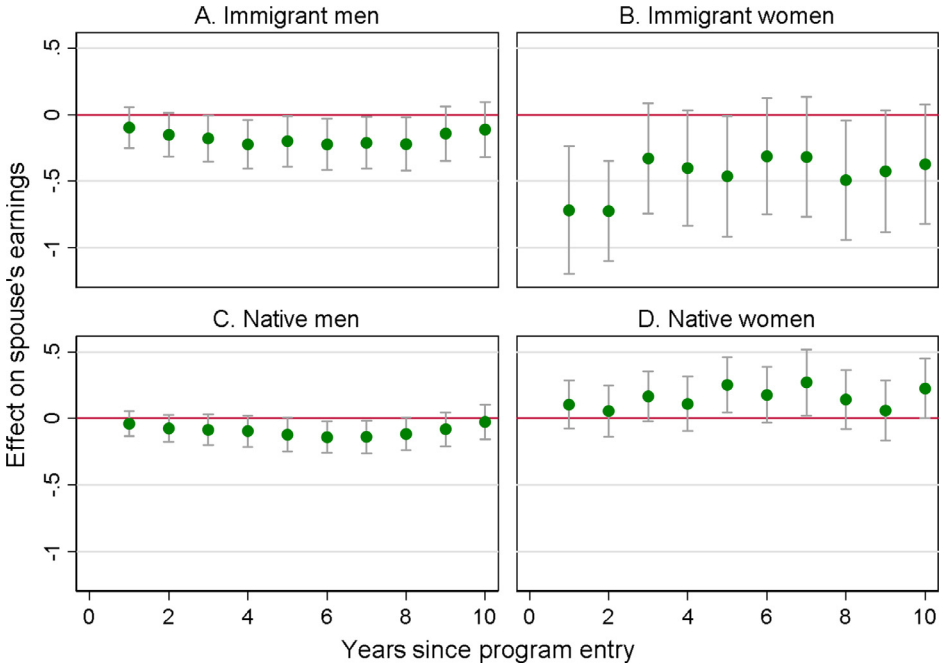

Fig. 4. Estimated effects of one Euro increase in TDI benefit on spouse's annual labor earnings
Note: The vertical lines depict 95% confidence intervals around point estimates. See also note to Figure 3.

Table 7
Estimated effects of TDI benefit level on own and spouse's earnings, family earnings, and after-tax family income, by gender and immigrant status and 1-5 vs 6-10 years after program entry.

	Men			**Women**		
	Immigrants (1)	Natives (2)	Difference (3)	Immigrants (4)	Natives (5)	Difference (6)
A. 1-5 years after program entry						
Own earnings	-0.346***	-0.188***	-0.158	-0.231**	-0.047	-0.184
	(0.087)	(0.065)	(0.124)	(0.097)	(0.043)	(0.118)
Spouse's earnings	-0.171**	-0.085*	-0.085	-0.549***	0.179*	-0.728***
	(0.078)	(0.052)	(0.099)	(0.186)	(0.090)	(0.247)
Family earnings	-0.520***	-0.274***	-0.246	-0.781***	0.132	-0.914***
	(0.121)	(0.084)	(0.161)	(0.213)	(0.101)	(0.276)
After-tax family income	-0.041	0.131	-0.173	-0.275**	0.224***	-0.499**
	(0.068)	(0.077)	(0.141)	(0.124)	(0.074)	(0.200)
Observations	3 375	17 596		2 635	26 001	
B. 6-10 years after program entry						
Own earnings	-0.262**	-0.093	-0.169	-0.350***	-0.053	-0.296*
	(0.112)	(0.078)	(0.148)	(0.135)	(0.056)	(0.154)
Spouse's earnings	-0.194**	-0.110*	-0.084	-0.463**	0.147	-0.610**
	(0.097)	(0.060)	(0.117)	(0.213)	(0.104)	(0.284)
Family earnings	-0.456***	-0.203**	-0.253	-0.816***	0.096	-0.912***
	(0.157)	(0.100)	(0.194)	(0.264)	(0.120)	(0.329)
After-tax family income	-0.201**	-0.017	-0.184*	-0.447***	0.121	-0.568**
	(0.079)	(0.057)	(0.109)	(0.139)	(0.094)	(0.253)
Observations	3 230	16 882		2 511	25 023	

*/**/*** statistically significant at the 10/5/1% levels.
Note: Standard errors are reported in parentheses. Samples are restricted to those married at the time of program entry, with both spouses present in Norway 5 or 10 years after program entry, spouse not in program, and spouse without self-employment income above 0.5 million NOK.

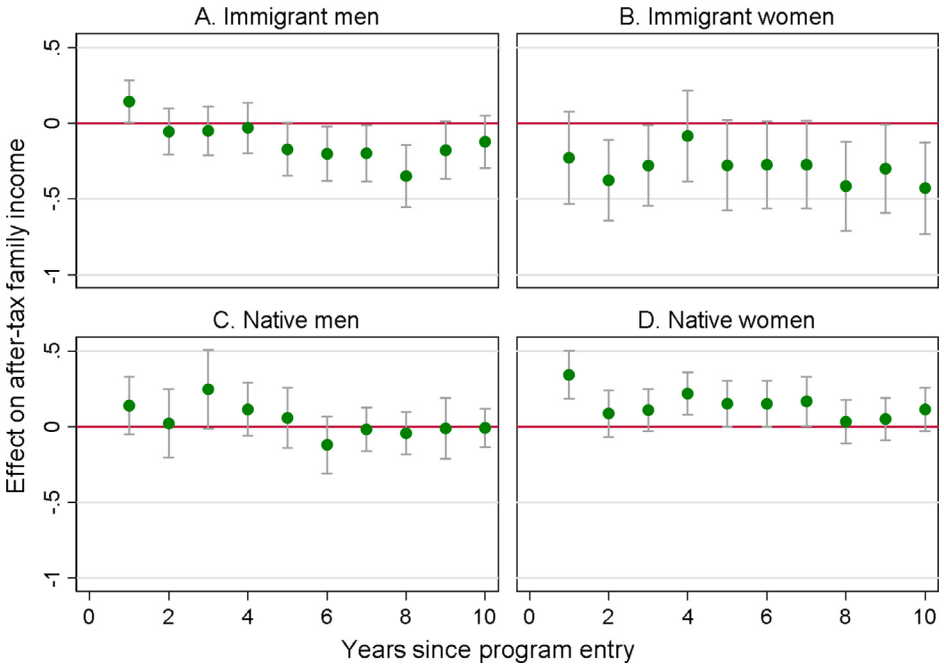

Fig. 5. Estimated effects of one Euro increase in TDI benefit on after-tax family income
Note: The vertical lines depict 95% confidence intervals around point estimates. See also note to Figure 4.

erably smaller, when the wife reduces her labor supply in response to the higher benefits of claimant immigrant husbands.

For native couples, the picture is similar as for immigrants with respect to the behavior of non-claimant wives. When the male claimant lowers his labor supply in response to higher benefits, his female partner works less, consistent with leisure complementarity. However, the responses of husbands are very different. Here, we identify a small but positive cross labor supply effect, consistent with an explanation based on decreasing returns to home production: The marginal value of his time off work decreases when she is more at home in response to higher benefits; hence he raises his labor supply.

Since the main goal of the TDI program is to provide insurance against (negative) health shocks, it is of considerable interest to see how higher benefits affect disposable income of families. Higher benefits obviously raise transfer incomes, but since higher TDI benefits also reduce labor earnings of both spouses, positive income effects will be mitigated. Benefit substitution will also dilute the income gain as more generous TDI benefits may crowd out other (means-tested) transfers such as social assistance. Thus, a natural question to ask is whether higher benefits make the TDI claimants economically better off at all. To answer this question, in Figure 5 we repeat the year-by-year regressions with total household annual after-tax income (i.e., earnings plus total benefits minus direct taxes) as the dependent variable.

For natives, higher benefits make the affected households better off economically in the first few years after TDI entry. The gain is larger for women, partly because their husbands work more. For immigrants, the overall impact on family after-tax income is less clear. Figure 5 shows that the households of male immigrant claimants receive higher incomes in the very short run, but thereafter the persistent negative effects on labor earnings dominate. For immigrant women, the effect on after-tax income is negative even in the short run, with the driving force being the reduced labor supply of the spouse.

The estimates reported in Table 7 confirm the positive effects on family income among natives, albeit statistically significant only for women during the first five years. For immigrants, the combined effects of labor supply responses and benefit substitution lead to a decline in household after-tax income, particularly over the long-term horizon. The negative earnings effects are amplified for family earnings, simply because the pattern of responses among spouses mirror those of the claimants. For family earnings, the negative effects for immigrants are particularly large.

When we turn to after-tax family income, we see that the social insurance arrangements (including permanent disability pensions) actually neutralize the long run effects for households of native male TDI claimants. Among immigrant families, income effects are also mitigated by various social insurance benefits. Nevertheless, the results show sizeable, negative long-run effects of increased benefits on the after-tax incomes of families of immigrant TDI claimants.

10. Conclusions

The economic implications of migration are intimately related to the host-country labor market performance of immigrants. In many welfare state economies, there has been a tendency for immigrants from low-income countries to perform poorly in the labor market, and thus become overrepresented in social insurance programs. When these programs are redistributive, replacement ratios are larger for individuals with bleak labor market opportunities, and the moral hazard affecting work incentives is potentially more important for immigrants. Since a larger fraction of immigrants face small economic gains from employment relative to receiving social insurance, ambitious welfare state institutions with comprehensive coverage against health and income shocks may widen rather than reduce immigrant-native employment gaps.

To motive our study based on administrative data from Norway, we first document that immigrants from low-income countries are more likely to receive transfers from the welfare state and that 22% of immigrant men have social insurance as their main source of income, compared to 11% among native men. The employed native men have a disposable (after-tax) income that is almost twice that of social insurance recipients. The corresponding differential for immigrants is only 60%, suggesting that the economic rewards for being employed are much larger for natives.

A reform of the benefit formula used by the temporary disability insurance (TDI) program enables us to exploit exogenous variation in benefits in order to identify and estimate the causal effect of benefits on employment and earnings. Identical individuals were entitled to different benefits, depending on the exact timing of program entry.

We find that higher benefits reduce the transition rate to employment and prolong the program spell of participants, and also lead to

considerably lower labor earnings several years after entry to the TDI program. We find a distinct pattern of greater responses to benefit generosity among immigrants from low-income countries, when compared to natives, for both exits to employment and subsequent labor earnings.

Our findings are consistent with a simple theoretical framework in which behavioral responses at the extensive margin reflect heterogeneity in circumstances affecting the value of leisure and in labor market opportunities. Greater responses to benefit generosity are expected among groups such as immigrants from low-income countries, who are located close to the threshold where there are small economic gains from being employed and/or high utility of non-employment (e.g., due to their family structure). The fact that immigrants are heavily overrepresented in the capital area where the supply of diverse (and low-skill) employment opportunities are at its largest, pulls in the same direction. Allowing for heterogeneous effects of benefits along the dimensions of predicted earnings prospects, family structure, work experience, and place of residence, we find that the responsiveness of natives approaches that of immigrants, and that the immigrant-native response differential becomes substantively as well as statistically insignificant.

We find that benefit generosity also affects spousal labor supply. Except for spouses of native women, the earnings of the non-claimant spouse drop in response to higher TDI benefits. Thus, the effects of benefit generosity are amplified when we consider family earnings. As for TDI claimants, the negative cross effects on spousal earnings are larger for immigrants than for natives. For immigrant women, the cross effect on spouses is even larger than the direct effect on their own earnings. Thus, employment of immigrant men is affected both directly and indirectly by the generosity of the TDI program.

The negative earnings effects prevail even 6-10 years after program entry, expect for native women who are unaffected in the long run. Again, the largest effects are found for immigrants. Actually, the negative labor supply effect among immigrants means that household after-tax income is reduced in the long run. While all groups gain economically from higher benefits in the short run, the after-tax income of native claimants are unaffected in the long run.

If policy makers aim at substantially reducing the negative immigrant-native employment differential, it may thus be necessary to make employment more attractive relative to non-employment for persons with characteristics implying small utility gains from employment. Our estimates suggest that lower social insurance benefits will reduce program participation and raise employment as well as earnings. However, lower benefits would inevitably make those who are unable to find self-supporting work worse off, and hence make the insurance against adverse income or health shocks less effective.

Declaration of Competing Interest

None.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.labeco.2020.101854.

Appendix A. Pre-trends

As noted in Section 6, the identification strategy used in this paper hinges on the assumption that the reform-initiated changes in individual TDI benefits do not correlate with changes in expected outcomes related to unobserved heterogeneity not accounted for in the analysis. One way to assess the validity of this assumption is to examine pre-trends in economic outcomes for individuals who were differently affected by the reform. To do this, we divide the sample of pre-reform entrants into two equally sized groups—those with above median reform-initiated benefit gains and those with above median gains. We then look at average earnings over the five-year period following TDI entry. The results, shown in Figure A1, do not point to differential time trends with respect to the subsequent reform-initiated changes in benefit entitlements.

In order to test for differential trends more formally, we set up a regression model where we use earnings 1-5 years after program entry as the dependent variable, and examine whether the pre-trend in this outcome is affected by a dummy variable indicating above median benefit gain. The results, shown in Table A1, do not indicate any significant pre-reform trend differentials.

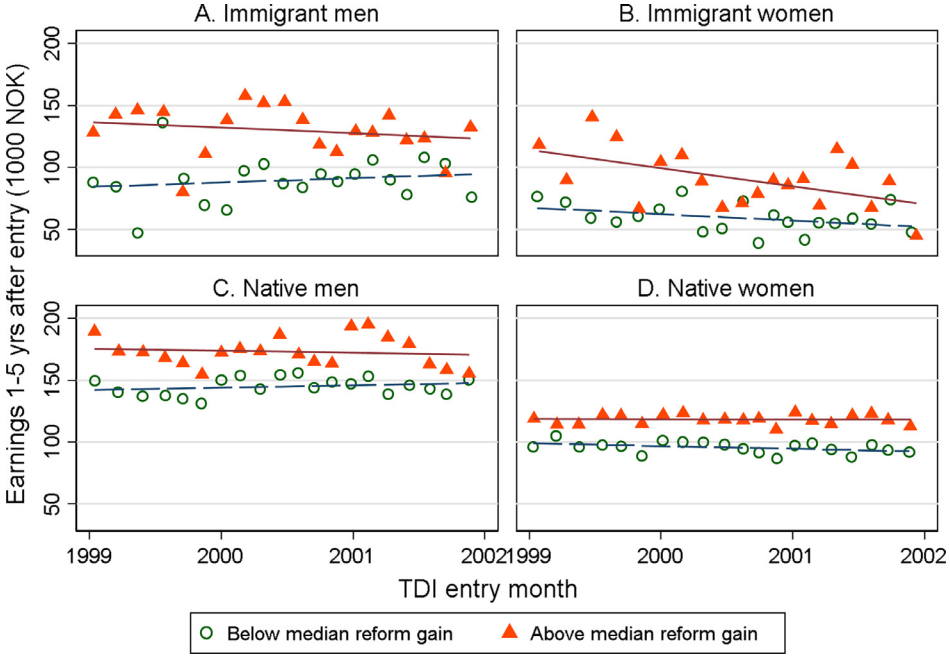

Fig. A1. Binned scatter plots of trends in earnings 1-5 years after program entry for pre-reform entrants with high and low benefit gains from reform
Note: Dependent variable is average real annual earnings in 1000 NOKs. Regressions control for educational attainment (5 levels), age (33), and family characteristics (5).

Table A1
Tests of parallel trends in earnings 1-5 years after program entry for pre-reform entrants with high and low benefit gains from reform.

	Men		Women	
	Immigrants (1)	Natives (2)	Immigrants (3)	Natives (4)
Coefficient of				
Trend variable	3.88	1.98	-5.34	-2.37
	(4.83)	(1.77)	(4.28)	(1.21)
Trend interacted with above median gain	-9.58	-3.52	-10.17	2.46
	(6.91)	(2.54)	(6.38)	(1.72)
Above median gain	57.55***	35.38***	52.96***	21.12***
	(11.99)	(4.37)	(11.64)	(3.00)
Number of observations	2 775	27 850	1 880	29 712

*/**/*** statistically significant at the 10/5/1% levels.
Note: Standard errors are reported in parentheses. Samples are restricted to pre-reform program entrants and those present in Norway 5 years after program entry. Dependent variable is average real annual earnings in 1000 NOKs. Regressions control for educational attainment (5 levels), age (33), and family characteristics (5). The above-median gain coefficient is evaluated at month of program entry January 1999.

Appendix B. Placebo analysis

Fevang et al. (2017) assessed the validity of the identifying assumption also used in the present paper by means of imposing falsely timed reforms in the middle of the pre and post-reform periods, and then re-estimating their competing risks hazard rate model separately for those entering before or after the true timing of the reform. None of the resultant "placebo" analyses indicated any violation of the identifying assumption. In this appendix, we perform a similar placebo analysis for our 1-5 years annual earnings outcome, yet to increase power we pool the pre and post-reform TDI entrants into a single dataset. The results are provided in Figure B1, which can be compared directly to Figure 3 above. The significant effects identified when we use the correct timing of the reform are now absent. In particular, we see none of the significant and quite substantial negative effects identified for immigrants. This result speaks against the concern that the results in Figure 3 are driven by differential trends not accounted for in the analysis.

Appendix C. TDI program entry

It is plausible that the reform-initiated changes in TDI benefits also triggered changes in the patterns of inflow into the TDI program. If the benefit level affects entry into social programs, this clearly adds a possible explanation for the immigrant-native employment differential that is of interest in itself. In addition, it may raise questions about the internal validity of the analysis performed in this paper, as effects on the inflow into the program potentially could change the correlation pattern between pre and post-reform benefit levels on the one hand, and unobserved heterogeneity on the other.

In this appendix, we examine empirically the extent to which entry into the TDI program was affected by benefit changes generated by the reform. This is not a straightforward exercise, however, as it is not clear how we should define the population at risk for this transition. Here, we exploit the fact that a considerable fraction of TDI claimants enter the program directly or shortly after having exhausted their 12-month period of sick-pay entitlements. Hence, workers who approach sick-pay

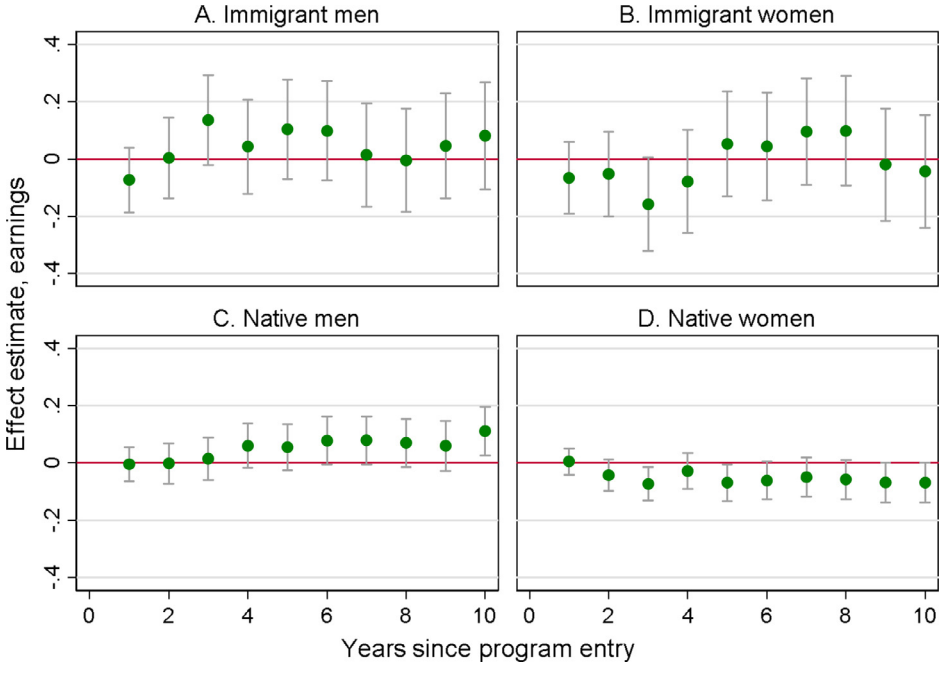

Fig. B1. Placebo analyses of effects of a one Euro increase in TDI benefit level on annual labor earnings 1-5 years after program entry
Note: Vertical lines depict 95% confidence intervals around point estimates. See also note to Figure 3.

Table C1
Descriptive statistics, sick leave samples.

	Men		Women	
	Immigrants (1)	Natives (2)	Immigrants (3)	Natives (4)
Age	40.2	44.3	39.4	43.3
Educational attainment:				
Compulsory	40.0	36.7	47.5	37.4
Upper secondary	34.3	55.2	28.2	50.3
College	15.0	6.2	13.1	10.4
Post graduate	2.3	1.4	2.7	1.5
Unknown	8.5	0.5	8.6	0.3
Employed year before	93.5	96.9	91.8	95.8
Average earnings 3 prior years	345 798	474 920	258 496	338 184
TDI benefits:				
Pre-reform rules	180 038	250 456	135 168	189 006
Post-reform rules	237 255	271 938	197 836	222 194
Implied replacement (pre-tax):				
Pre-reform rules	0.521	0.527	0.523	0.559
Post-reform rules	0.686	0.573	0.765	0.657
Outcomes:				
Temp disability 6 months later	46.2	46.1	45.6	47.7
Employed next year (w/o TDI)	35.9	42.8	35.6	41.1
Number of spells	7 198	99 215	4 208	79 631
Fraction post reform	55.1	51.0	56.9	51.1

Table C2
Estimated effects of TDI benefit level on TDI entry and employment, by gender and immigrant status.

	Men			Women		
	Immigrants (1)	Natives (2)	Difference (3)	Immigrants (4)	Natives (5)	Difference (6)
A. TDI after 6 months						
Logit coefficient	0.198	0.084	0.114	0.284	0.078	0.205
	(0.132)	(0.062)	(0.146)	(0.181)	(0.067)	(0.193)
Elasticity	0.106	0.046		0.154	0.041	
	(0.071)	(0.034)		(0.098)	(0.035)	
Mean outcome	0.462	0.461		0.456	0.477	
B. Job next year						
Logit coefficient	-0.240*	-0.129*	-0.112	-0.189	-0.141**	-0.048
	(0.143)	(0.067)	(0.158)	(0.193)	(0.072)	(0.206)
Elasticity	-0.154*	-0.074*		-0.122	-0.083**	
	(0.092)	(0.038)		(0.124)	(0.042)	
Mean outcome	0.359	0.428		0.356	0.411	
Number of spells	7 198	95 215		4 208	79 631	

*/**/*** statistically significant at the 10/5/1% levels.
Note: Standard errors are reported in parentheses. Samples consist of individuals 9 months into sickness absence spell.

exhaustion constitute a major risk population for TDI entry. To study the impact of the TDI benefit level on program entry, we therefore construct a data set consisting of all persons who had exhausted 9 months of their sick-pay entitlements in the period between January 1999 and December 2003, except those who reached this stage of sick pay in the period from July to September 2001.[14] We then define the dependent variable as the incidence of participating in the temporary disability program six months thereafter.

Table C1 offers some descriptive statistics. In total, there are 186,252 sick-pay spells lasting at least nine months included in our analysis data and 47% were TDI program participants six months later. As the table shows, TDI benefits under both the old and new sets of computation rules are somewhat higher for natives than for immigrants from low-income countries. Relative to average earnings during the three years prior to the sick pay spell, the post-reform benefit levels are, however, considerably higher for immigrants.

In order to assess the role that the TDI benefit level has on the propensity to enter the program, we estimate logit models with TDI participation after six months (i.e., three months after potential sick-pay exhaustion) as the dependent variable, and with the structure of explanatory variables as in Equation (3). In Table C2, panel A, we report the elasticities from models estimated separately by gender and immigrant status. In general, the point estimates suggest that more generous TDI benefits trigger larger inflows from long-term sickness. The estimated impacts are small, however, and none of the estimates is statistically significant; see panel A. This finding may reflect that the benefit calculation schedules are complicated and that many entrants do not have knowledge about the precise benefit level before they actually enter the program. The estimated negative impacts on employment status next year appear to be larger, and also more statistically significant. However, it is important to bear in mind that the estimates in Panel B also capture the effects that operate through the influence on exit rates already identified for actual participants.

Viewed as a whole, our interpretation of the findings in Table C2 is that they suggest that entry effects of TDI benefits are small. Paired with the evidence presented in Section 6, Figure 2, where we showed that the distributions of benefit gains (the difference between the benefit levels calculated according to the post-reform and pre-reform schedules) were almost identical for pre-reform and post-reform entrants, the indi-

[14] We drop these observations because there is some ambiguity as to which benefit regime they would belong.

Table D1
Estimated hazard rate elasticities with respect to TDI benefit.

	Men		Women	
	Immigrants (1)	Natives (2)	Immigrants (3)	Natives (4)
Log actual TDI benefit				
Effect on transition to:				
Employment	-0.647***	-0.311***	-0.424***	-0.084
	(0.143)	(0.068)	(0.127)	(0.052)
PDI	-0.086	-0.136	0.028	-0.111
	(0.180)	(0.102)	(0.160)	(0.086)
Unemployment	-0.538	-0.103	-0.466	0.156
	(0.398)	(0.250)	(0.675)	(0.383)
Non-participation	-0.069	-0.137	0.128	-0.171**
	(0.127)	(0.091)	(0.111)	(0.084)
Number of spells	7 128	63 346	5 267	67 909
Number of support points in heterogeneity distribution	5	6	1	6

*/**/*** statistically significant at the 10/5/1% levels.
Note: Standard errors are reported in parentheses. In addition to the two hypothetical benefit variables, the vector of control variables includes educational attainment (5 dummy variables), age (33 dummy variables), family characteristics (5 dummy variables), time-varying calendar quarter (40 dummy variables), county (19 dummy variables), spell duration by quarter (13 dummy variables), and local labor market conditions at entry and in current quarter.

cation is that the potential for disturbing changes in the spurious correlation patterns between the two benefit variables and relevant unobserved characteristics of TDI program participants is unlikely to invalidate our analysis.

Appendix D. Extended hazard rate model

To shed further light on the various exit rates from the TDI program, in Table D1 we report coefficient estimates from a mixed proportional competing risks hazard rate model in which the non-employment state (see Table 3) is divided into three separate destinations: Permanent disability insurance (PDI), unemployment, and non-participation. Due to the computational challenge, this model is estimated separately for immigrants and natives only. For the non-employment exit routes, most of the estimated elasticities are negative, although only the impact on the hazard rate to non-participation for females is statistically significant. Importantly, estimated elasticities of benefits on the transition hazard to employment are similar to those from the more parsimonious model underlying Table 3.

Appendix E. Heterogeneous effects on earnings

Table E1 reports the estimated interaction effects of the actual benefit level and the covariates listed in Table 4, when these are estimated separately for immigrants and natives; see columns (1) and (2) for men and columns (4) and (5) for women. These are directly comparable to the estimates reported for the pooled sample in Table 5, columns (3) and (6). Columns (3) and (6) of Table E1 report the immigrant-native differential interactions effects, estimated in the pooled samples. As the columns show, none of the interactions differs significantly for immigrants and natives, justifying the parsimonious specification with common interaction terms used in Table 5.

Table E2 reports the implied group-specific estimates for all 36 possible combinations of the relevant covariates when the benefit interaction effects are estimated separately for immigrants and natives, whereas Table E3 reports the same estimates when the interaction effects are restricted to be the same for immigrants and natives. Table E3 thus contains the estimated response parameters based on the causal estimates reported in Table 5, columns (3) and (6). The weighted average effects presented in Panel B of Table 5 simply weight the 36 re-

Table E1
Estimated heterogeneous effects of TDI benefit level on earnings 1-5 after program entry, by gender and immigrant status. Tests of equality of benefit interaction terms for immigrants and natives.

	Men			Women		
	Immigrants (1)	Natives (2)	Difference (3)	Immigrants (4)	Natives (5)	Difference (6)
Interaction between actual benefits and						
Medium earnings potential	0.117	0.110	0.007	0.251*	-0.060	0.310
	(0.136)	(0.190)	(0.245)	(0.136)	(0.120)	(0.189)
High earnings potential	0.141	0.197	-0.056	0.290*	0.087	0.203
	(0.178)	(0.195)	(0.282)	(0.175)	(0.125)	(0.227)
Spouse employed	0.037	0.045	-0.008	0.127	0.147	-0.020
	(0.123)	(0.110)	(0.179)	(0.141)	(0.097)	(0.182)
Single	0.117	0.071	0.046	0.094	0.087	0.007
	(0.135)	(0.105)	(0.188)	(0.163)	(0.098)	(0.202)
Experience>=10	0.173	0.126	0.046	0.115	0.043	0.072
	(0.130)	(0.083)	(0.172)	(0.155)	(0.053)	(0.177)
Capital region	0.002	-0.172**	0.173	-0.072	-0.159***	0.088
	(0.101)	(0.078)	(0.140)	(0.099)	(0.052)	(0.119)

*/**/***Statistically significant at 10/5/1% level.
Note: Standard errors are reported in parentheses. Regressions include the full set of control variables described in note to Table 5. Models in columns (1), (2), (4), and (5) are estimated in separate samples; models in columns (3) and (6) in pooled samples of immigrants and natives with all regressors interacted with the immigrant indicator variable.

Table E2
Estimated effect of TDI benefit level on earnings 1-5 after program entry in 36 cells given by earnings potential, family status, actual experience, and region of residence. Separate regressions by gender and immigrant status.

	Men		Women	
	Immigrants (1)	Natives (2)	Immigrants (3)	Natives (4)
Spouse home, low exper, not capital reg				
Low earn potential	-0.566***	-0.401*	-0.538***	-0.200
	(0.158)	(0.208)	(0.171)	(0.145)
Medium earn potential	-0.449***	-0.29**	-0.288*	-0.26**
	(0.127)	(0.123)	(0.153)	(0.102)
High earn potential	-0.425**	-0.203	-0.249	-0.113
	(0.178)	(0.132)	(0.193)	(0.108)
Spouse empl, low exper, not capital reg				
Low earn potential	-0.529***	-0.355*	-0.412***	-0.053
	(0.151)	(0.194)	(0.147)	(0.118)
Medium earn potential	-0.412***	-0.245***	-0.161	-0.113**
	(0.107)	(0.094)	(0.102)	(0.052)
High earn potential	-0.388**	-0.158	-0.122	0.034
	(0.159)	(0.103)	(0.154)	(0.064)
Single, low exper, not capital reg				
Low earn potential	-0.45***	-0.330*	-0.444***	-0.113
	(0.153)	(0.184)	(0.164)	(0.117)
Medium earn potential	-0.333***	-0.219***	-0.194	-0.173***
	(0.127)	(0.075)	(0.138)	(0.049)
High earn potential	-0.309*	-0.132	-0.155	-0.026
	(0.181)	(0.092)	(0.18)	(0.061)
Spouse home, high exper, not capital reg				
Low earn potential	-0.394**	-0.274	-0.424*	-0.157
	(0.195)	(0.214)	(0.228)	(0.152)
Medium earn potential	-0.277*	-0.164	-0.173	-0.217**
	(0.152)	(0.107)	(0.207)	(0.100)
High earn potential	-0.253*	-0.077	-0.134	-0.07
	(0.149)	(0.101)	(0.176)	(0.095)
Spouse empl, high exper, not capital reg				
Low earn potential	-0.356*	-0.229	-0.297	-0.01
	(0.197)	(0.200)	(0.214)	(0.126)
Medium earn potential	-0.239	-0.118*	-0.046	-0.070
	(0.146)	(0.071)	(0.177)	(0.049)
High earn potential	-0.215	-0.032	-0.007	0.077**
	(0.137)	(0.057)	(0.14)	(0.039)
Single, high exper, not capital reg				
Low earn potential	-0.277	-0.203	-0.330	-0.069
	(0.200)	(0.194)	(0.223)	(0.128)
Medium earn potential	-0.160	-0.093	-0.079	-0.129**
	(0.163)	(0.057)	(0.197)	(0.052)
High earn potential	-0.136	-0.006	-0.040	0.018
	(0.163)	(0.051)	(0.163)	(0.042)
Spouse home, low exper, capital reg				
Low earn potential	-0.565***	-0.572***	-0.61***	-0.359**
	(0.161)	(0.219)	(0.178)	(0.152)
Medium earn potential	-0.448***	-0.462***	-0.359**	-0.419***
	(0.129)	(0.141)	(0.157)	(0.112)
High earn potential	-0.423**	-0.375**	-0.320*	-0.272**
	(0.178)	(0.148)	(0.192)	(0.116)
Spouse empl, low exper, capital reg				
Low earn potential	-0.527***	-0.527**	-0.483***	-0.212*
	(0.16)	(0.205)	(0.154)	(0.126)
Medium earn potential	-0.41***	-0.416***	-0.232**	-0.272***
	(0.117)	(0.115)	(0.107)	(0.069)
High earn potential	-0.386**	-0.330***	-0.194	-0.125*
	(0.165)	(0.122)	(0.153)	(0.075)
Single, low exper, capital reg				
Low earn potential	-0.448***	-0.501***	-0.516***	-0.272**
	(0.156)	(0.194)	(0.169)	(0.124)
Medium earn potential	-0.331**	-0.391***	-0.265*	-0.332***
	(0.129)	(0.097)	(0.140)	(0.063)
High earn potential	-0.307*	-0.304***	-0.226	-0.185***
	(0.180)	(0.110)	(0.177)	(0.07)
Spouse home, high exper, capital reg				
Low earn potential	-0.392**	-0.446**	-0.495**	-0.316**
	(0.19)	(0.225)	(0.231)	(0.159)
Medium earn potential	-0.275*	-0.335***	-0.244	-0.376***
	(0.143)	(0.128)	(0.207)	(0.110)
High earn potential	-0.251*	-0.249**	-0.206	-0.229**
	(0.139)	(0.122)	(0.173)	(0.104)

(*continued on next page*)

Table E2 (*continued*)

	Men		Women	
	Immigrants (1)	Natives (2)	Immigrants (3)	Natives (4)
Spouse empl, high exper, capital reg				
Low earn potential	-0.354*	-0.400*	-0.368*	-0.169
	(0.196)	(0.211)	(0.217)	(0.135)
Medium earn potential	-0.237*	-0.290***	-0.118	-0.229***
	(0.143)	(0.098)	(0.178)	(0.066)
High earn potential	-0.213	-0.203**	-0.079	-0.082
	(0.132)	(0.088)	(0.135)	(0.056)
Single, high exper, capital reg				
Low earn potential	-0.275	-0.375*	-0.401*	-0.229*
	(0.194)	(0.204)	(0.224)	(0.134)
Medium earn potential	-0.158	-0.264***	-0.150	-0.289***
	(0.154)	(0.085)	(0.195)	(0.066)
High earn potential	-0.134	-0.178**	-0.112	-0.142***
	(0.153)	(0.081)	(0.156)	(0.054)

*/**/***Statistically significant at 10/5/1% level.

Note: Standard errors are reported in parentheses. Regressions include the full set of control variables described in note to Table 5.

Table E3

Estimated effect of TDI benefit level on earnings 1-5 after program entry in 36 cells given by earnings potential, family status, actual experience, and region of residence. Pooled samples of immigrants and natives and with benefit interaction terms restricted to be the same for immigrants and natives.

	Men		Women	
	Immigrants (1)	Natives (2)	Immigrants (3)	Natives (4)
Spouse home, low exper, not capital reg				
Low earn potential	-0.481***	-0.427***	-0.367***	-0.322***
	(0.139)	(0.143)	(0.128)	(0.114)
Medium earn potential	-0.346***	-0.292***	-0.316***	-0.272***
	(0.105)	(0.102)	(0.107)	(0.087)
High earn potential	-0.253**	-0.199*	-0.172	-0.127
	(0.117)	(0.111)	(0.113)	(0.094)
Spouse empl, low exper, not capital reg				
Low earn potential	-0.446***	-0.392***	-0.196*	-0.152
	(0.135)	(0.131)	(0.113)	(0.093)
Medium earn potential	-0.311***	-0.257***	-0.146*	-0.101**
	(0.095)	(0.081)	(0.085)	(0.049)
High earn potential	-0.218**	-0.164*	-0.002	0.043
	(0.106)	(0.090)	(0.092)	(0.060)
Single, low exper, not capital reg				
Low earn potential	-0.412***	-0.358***	-0.254**	-0.209**
	(0.134)	(0.12)	(0.117)	(0.091)
Medium earn potential	-0.277***	-0.223***	-0.204**	-0.159***
	(0.097)	(0.065)	(0.09)	(0.047)
High earn potential	-0.184*	-0.130	-0.059	-0.015
	(0.112)	(0.082)	(0.096)	(0.058)
Spouse home, high exper, not capital reg				
Low earn potential	-0.367**	-0.312**	-0.335**	-0.291**
	(0.154)	(0.144)	(0.139)	(0.119)
Medium earn potential	-0.232**	-0.178**	-0.285**	-0.240***
	(0.111)	(0.088)	(0.115)	(0.086)
High earn potential	-0.139	-0.085	-0.141	-0.096
	(0.109)	(0.082)	(0.111)	(0.081)
Spouse empl, high exper, not capital reg				
Low earn potential	-0.331**	-0.277**	-0.165	-0.120
	(0.151)	(0.134)	(0.127)	(0.099)
Medium earn potential	-0.197*	-0.142**	-0.115	-0.07
	(0.103)	(0.065)	(0.095)	(0.047)
High earn potential	-0.104	-0.049	0.03	0.074**
	(0.099)	(0.052)	(0.09)	(0.037)
Single, high exper, not capital reg				
Low earn potential	-0.298*	-0.243*	-0.223*	-0.178*
	(0.153)	(0.126)	(0.131)	(0.100)
Medium earn potential	-0.163	-0.109**	-0.172*	-0.128**
	(0.109)	(0.053)	(0.102)	(0.050)
High earn potential	-0.070	-0.016	-0.028	0.017
	(0.110)	(0.047)	(0.097)	(0.039)

(*continued on next page*)

Table E3 (*continued*)

	Men		Women	
	Immigrants (1)	Natives (2)	Immigrants (3)	Natives (4)
Spouse home, low exper, capital reg				
Low earn potential	-0.596***	-0.542***	-0.511***	-0.466***
	(0.140)	(0.154)	(0.129)	(0.122)
Medium earn potential	-0.461***	-0.407***	-0.461***	-0.416***
	(0.105)	(0.116)	(0.109)	(0.096)
High earn potential	-0.368***	-0.314**	-0.316***	-0.271***
	(0.117)	(0.124)	(0.113)	(0.101)
Spouse empl, low exper, capital reg				
Low earn potential	-0.561***	-0.506***	-0.341***	-0.296***
	(0.136)	(0.144)	(0.115)	(0.102)
Medium earn potential	-0.426***	-0.372***	-0.290***	-0.245***
	(0.096)	(0.099)	(0.087)	(0.063)
High earn potential	-0.333***	-0.279***	-0.146	-0.101
	(0.107)	(0.106)	(0.092)	(0.070)
Single, low exper, capital reg				
Low earn potential	-0.527***	-0.473***	-0.398***	-0.354***
	(0.133)	(0.132)	(0.117)	(0.099)
Medium earn potential	-0.392***	-0.338***	-0.348***	-0.303***
	(0.096)	(0.084)	(0.09)	(0.059)
High earn potential	-0.299***	-0.245**	-0.204**	-0.159**
	(0.110)	(0.097)	(0.094)	(0.065)
Spouse home, high exper, capital reg				
Low earn potential	-0.481***	-0.427***	-0.479***	-0.435***
	(0.155)	(0.155)	(0.141)	(0.126)
Medium earn potential	-0.347***	-0.292***	-0.429***	-0.384***
	(0.110)	(0.104)	(0.116)	(0.095)
High earn potential	-0.254**	-0.199**	-0.285**	-0.240***
	(0.109)	(0.098)	(0.111)	(0.088)
Spouse empl, high exper, capital reg				
Low earn potential	-0.446***	-0.392***	-0.309**	-0.264**
	(0.152)	(0.146)	(0.128)	(0.108)
Medium earn potential	-0.311***	-0.257***	-0.259***	-0.214***
	(0.103)	(0.085)	(0.096)	(0.061)
High earn potential	-0.218**	-0.164**	-0.114	-0.070
	(0.099)	(0.076)	(0.09)	(0.051)
Single, high exper, capital reg				
Low earn potential	-0.412***	-0.358***	-0.367***	-0.322***
	(0.152)	(0.137)	(0.132)	(0.107)
Medium earn potential	-0.277***	-0.223***	-0.317***	-0.272***
	(0.107)	(0.074)	(0.102)	(0.061)
High earn potential	-0.185*	-0.130*	-0.172*	-0.127**
	(0.107)	(0.069)	(0.095)	(0.05)

*/**/***Statistically significant at 10/5/1% level.

Note: Standard errors are reported in parentheses. Regressions include the full set of control variables described in note to Table 5.

sponse estimates with the immigrant frequency distribution across the 36 cells.

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