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Tax incentives for high skilled migrants: evidence from a preferential tax scheme in the Netherlands

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Tax incentives for high skilled migrants: evidence from a preferential tax scheme in the Netherlands

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Abstract

This paper examines to what extent an income tax exemption affects international mobility and wages of skilled immigrants. We study a preferential tax scheme for foreigners in the Netherlands, which introduced an income threshold for eligibility in 2012 and covers a large share of the migrant income distribution. By using detailed administrative data in a difference-in-differences setup, we find that the number of migrants in the income range closely above the threshold more than doubles, whereas there is little empirical support for a decrease of migration below the threshold. Our results indicate that these effects are driven mainly by additional migration, while wage bargaining responses are fairly limited. We conclude that the preferential tax scheme is highly effective in attracting more skilled migrants.

Keywords: international migration, income tax benefits, wage bargaining, bunching.

JEL: F22, J61, H24, H31

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1 Introduction

International labor migration has grown substantially over the past decades and standard economic theory has pointed to earnings differentials as the key driver of such relocation (Clemens (2022)). Workers are posited to consider financial gains from migration and compare them against the costs – in whatever form – associated with international relocation. Estimating the responsiveness of workers to differences in earnings across countries is challenging due to data limitations on the one hand, and the need for credibly exogenous variation on the other hand.

One approach is to exploit preferential tax schemes for foreigners, which are increasingly common (OECD (2011)) and aim to increase after-tax income in order to incentivize immigration of skilled workers.¹ Despite the popularity of such policies, the effect of such schemes on the mobility decision of high-skilled foreigners is not extensively researched. Most studies have resorted to either within-country variation in tax rates or the mobility of very specific groups of workers.² An exception is the study by Kleven et al. (2014), which considers a preferential tax scheme in Denmark for migrants with an income in the top one percent of the income distribution.

In this paper we provide estimates of the migration response to a large tax exemption offered to skilled workers that migrate to the Netherlands. The policy offers eligibility to all migrants with an income above a threshold, a requirement that almost one-third of all working migrants within our observation period fulfill. Identification is based on a reform that implemented the income threshold, which replaced the existing policy in which migrants had to prove that their skills were sufficiently scarce in the Dutch labor market. Combined with nationwide administrative data covering all migration movements and labour market outcomes in the Netherlands, we show that a large increase occurs in the number of arriving migrants with income above the threshold. While a minor part of this shift in the income distribution appears to result from wage bargaining responses, the majority can be attributed to additional migration.

¹Such schemes exist in 28 EU countries, with varying target populations such as employees performing specific professions or activities, or pensioners (Flamant, Godar, and Richard (2021)).

²Within-country analyses have been performed by Liebig, Puhani, and Sousa-Poza (2007) and Schmidheiny and Slotwinski (2018) for Switzerland and Moretti and Wilson (2017) and Young et al. (2016) for the US. Between-country studies have focused on specific types of workers such as football players (Kleven, Landais, and Saez (2013)) or scientists (Akcigit, Baslandze, and Stantcheva (2016)). See Section 2 for an overview.

In the Netherlands, high-skilled migrants can apply for a reduction of the income tax burden. This preferential tax scheme for foreigners is supposed to cover costs of moving as well as the difference in living costs and attract migrants with skills that are scarce in the Dutch labor market. The scheme grants an eligible employee up to 30% of their income as a tax-free payment for a specified duration. For the employee, this rule implies a substantial increase in after-tax income, while the firm incurs virtually no costs. While the so-called 30% rule has existed for decades, a major reform took place in 2012, introducing an income threshold for eligibility. This creates a discontinuous increase in after-tax income for prospective beneficiaries' if their income exceeds the threshold. In this paper we argue that the income threshold is expected to (i) increase the arrival of migrants with income above the threshold, (ii) affect wage bargaining between the employer and the prospective migrant, and (iii) potentially lead to an adjustment in working hours.

Exploiting that the reform affects migrant eligibility very differently at different parts of the income distribution, we apply a difference-in-differences analysis to estimate the change in the number of arriving migrants around the threshold. We find a strong positive and statistically significant increase in the number of arriving migrants with an income above the threshold. The increase grows over time since the introduction of the reform in 2012. The effects on the number of arriving migrants with income at most 1%, 5% and 10% above the threshold imply a 376%, 148% and 108% increase (respectively), relative to the pre-reform averages. Also the number of arriving migrants with salaries more than 10% above the threshold grows substantially after the reform, although this change is not statistically significant.

At the same time, we find no evidence of a decrease in arriving migrants with income below the threshold. The lack of such a decrease suggests that there is no (substantial) 'bargaining up' of salaries across the threshold, which hints at limited worker bargaining power. The sharp spike of observed migrants with income very close to the threshold does suggest that 'bunching from above' is more common: firms may reap some of the benefits that accrue from the tax exemption by lowering the before-tax salary. Nevertheless, the substantial increase in the number of arriving migrants across a wide range of income bins above the threshold allows us to conclude that the primary effect of the reform has been to increase the number of arriving migrants. Finally, we find no response in working hours, most likely because most expats already worked full-time before the reform.

These results show that migrants are very responsive to tax incentives. Our estimates aggregate to a total of over 8,000 additional migrants that arrive over the course of the

years 2012-2019 due to the reform. This translates into an elasticity of migration with respect to the net-of-tax rate of around two. We conclude that the reform was effective in attaining the policy goal to attract skilled workers.

We also present some secondary findings to gain more insights in the behavior of firms and migrants. First, we find that the duration between the immigration date and the date of applying for the tax exemption decreased substantially after the reform. Fast applications suggest that migrants have been more likely to be aware of the rule upon migrating, supporting that the rule has been an important driver of increased migration. It seems plausible that the income threshold improved transparency of the application process, making it easier for firms and workers to predict their future eligibility. Second, we measure the concentration of firms that make use of the rule over the pre- and post-reform years. We find that after the reform the share of ‘new firms’ (firms that hire a beneficiary for the first time) increases. Where the pre-reform application may have been more complicated and less predictable, it may have been a relatively small group of firms that had experience with using it. The reform has changed this, by making the rule more easily accessible for a wider range of firms.

The paper proceeds as follows. We briefly review the related literature in Section 2. Section 3 presents the Dutch preferential tax scheme and the 2012 reform. In this section we also discuss the expected responses in terms of migration, wage bargaining and labour supply. Section 4 describes the data used and provides descriptive statistics. Section 5 outlines the empirical strategy, motivates the choice of treatment and control groups, and presents the main results, including a number of robustness checks. Section 6 compares the main findings against potential mechanisms. Section 7 concludes.

2 Related literature

Estimating the migration response to taxes matters for optimal taxation, as shown by Mirrlees (1982) who examines optimal taxation in the presence of emigration. More recent studies explore tax schedules for competing governments (Lehmann, Simula, and Trannoy (2014)) and the role of emigration of high-skilled foreigners for optimal taxation (Simula and Trannoy (2010)).

Empirical research on migration responses to income tax incentives is scarce mainly due to data limitations and identification challenges.³ Research in this literature falls

³Our study also relates to research on the relationship between migration and other types of taxation,

in three main categories in terms of the empirical strategy: (i) variation of regional or inter-state taxes, (ii) variation of tax rates across countries over time and (iii) variation due to tax reforms (see Kleven et al. (2020) for a survey). Contributions in the first category mainly exploit variation in regional (income) taxes in Switzerland (Liebig, Puhani, and Sousa-Poza (2007) and Schmidheiny and Slotwinski (2018)) and in the US (Young et al. (2016) and Moretti and Wilson (2017)). While these papers find some evidence for (heterogeneous) cross-state migration in response to taxes, the findings are limited to within-country movements. International migration is likely to differ due to larger cultural, geographical and bureaucratic migration costs.

The second category studies international migration and exploits variation in (income) tax rates across countries over time. These between-country studies mainly focus on certain professions, for example star scientists (Akcigit, Baslandze, and Stantcheva (2016)) or football players (Kleven, Landais, and Saez (2013)). While these papers do capture the international dimension, they are limited to very narrow professions. One exception is Muñoz (2021), who estimates mobility responses of top decile income earners across countries in the European Union.

The third approach is to use natural experiments (tax reforms) to identify migration responses to income taxes. The study most closely related to ours is Kleven et al. (2014), who examine the Danish preferential tax scheme for migrants whose income falls in the top 1% of the Danish income distribution. Workers with such levels of earnings are likely to be more mobile, raising the question whether mobility estimates generalize to lower parts of the income distribution. Our paper examines a reform of a tax scheme that affects migrants in a broad range of the income distribution. The wide applicability of the tax rule results in a large number of affected migrants and a broad range of professions that are potentially impacted by the reform.⁴ This extends the external validity of our findings.

Many of these studies summarize their findings with an estimate of the migration elasticity (the change in migration relative to the change in the net-of-tax rate). The estimated elasticities are generally low or close to zero for domestic individuals (as in Akcigit, Baslandze, and Stantcheva (2016), Young et al. (2016), Kleven, Landais, and Saez (2013) and Muñoz (2021)). For international mobility and especially for high skilled (high income)

for example wealth and property taxes. Examples are Bakija and Slemrod (2004) who consider wealth taxes in the US, Agrawal and Foremny (2019) who study the net-of-tax rate of high-income earners in Spain and Young and Varner (2011) who study a millionaire tax in New Jersey (US)).

⁴Our sample contain more than 220,000 migrants in total and more than 40,000 30%-rule beneficiaries over a period of 13 years.

migrants, implied elasticities are often equal or higher than one (Akcigit, Baslandze, and Stantcheva (2016), Kleven, Landais, and Saez (2013), Kleven et al. (2014) and Schmidheiny and Slotwinski (2018)). Our results show that when considering a more diverse set of employees (in terms of the wage distribution), the elasticity remains broadly consistent with these findings (although somewhat larger).

3 Preferential tax scheme for foreigners in the Netherlands

Variations of a preferential taxation scheme for skilled foreigners with the goal to compensate for migration costs have been in place in the Netherlands since the 1950s. In 2001, the scheme was formalized and the proportion of tax-free income was set to 30% (previously 35%, Kamerstuk (1998)). The benefits from lower income taxation are intended as a compensation for the migrant’s extra-territorial costs, such as moving costs and the difference in the level of living standards (Algemene Rekenkamer (2016)).⁵

The advantages for beneficiaries of the scheme are twofold. Firstly, there is a reduction of income tax payments which translates to a higher net income for the employee. The scheme grants an employee up to 30% of the income as a tax-free payment for a specified duration. Secondly, beneficiaries have the option for an exemption from the wealth tax on foreign assets.⁶ This option may provide an additional incentive for migrants with foreign wealth to apply for the 30% rule and it is assessed that (almost) all 30% rule beneficiaries make use of the option (Kamerstuk (2011a)).

To be eligible, an employee must possess some specific expertise that is scarce in the Dutch labour market and be recruited from abroad with the purpose of entering an employment relationship with a firm that is withholding payroll taxes in the Netherlands. Prior to 2012, the skill criterion was assessed via a list of seven occupations that were deemed as ‘high-skilled’ (Algemene Rekenkamer (2016)). The occupations were defined very broadly, leaving substantial room for discretion when assessing eligibility of applicants.⁷

⁵However, beneficiaries of this scheme do not need to deliver a proof of such expenses.

⁶The so-called partial non-resident taxpayer status effectively decreases tax liability on foreign wealth (capital gains and investments).

⁷These occupations include top managers of international firms, scientists with specific knowledge, product specialists, teachers in international schools, foreign employees of international institutions, and foreign middle and senior employees who are posted abroad as a result of mandatory company rotation

Additionally, applicants must have lived outside the Netherlands for a majority of the two years prior to immigration, and any time spent working or studying in the Netherlands prior to application is deducted from the duration of benefits. Employee and employer need to apply jointly within 4 months of commencing the employment relationship. The application procedure is simple and consists only of a 3-page form to be filled in jointly by the employer and the employee. The employee needs to re-apply for the tax scheme upon switching employer. The criteria for eligibility are evaluated on an annual basis. If an employee does not meet the criteria anymore, they lose eligibility for the scheme.

In principle, the proportion of tax-free reimbursement is not fixed, and it can be 'up to' 30%. From an employer's perspective, there are no objective reasons to grant a lower (than possible) percentage tax free or not to apply at all. Firstly, the employer bears no additional costs from granting a part of the tax-free income. On the contrary, social security payments are calculated based on taxable income and hence the employer might actually marginally save on social security contributions.⁸ Secondly, the tax scheme may facilitate recruitment as it increases the employee's earnings without any additional cost for the employer.

The 2012 reform

A major reform of the 30% scheme was implemented in 2012, introducing changes on two dimensions: eligibility and duration. Firstly, the 2012 reform installed two new criteria for eligibility. The first criterion relates to the employee's income: the reform established an income threshold for eligibility, replacing the previous principle related to occupation. The threshold was 35,000 € in 2012 and applied to all employees over 30 years (the threshold is lower for young employees with a Master's degree), with the exception of academics who are always eligible. Both income thresholds are annually indexed. The income thresholds are supposed to serve as a proxy for specific expertise and make the eligibility process simpler and more transparent (Kamerstuk (2011b), p. 21). The eligibility decision based on the income makes the probability of receiving the tax benefits more predictable from both the firm's and migrant's point of view.⁹

and finally there is a category 'other'.

⁸This also implies a slightly lower pension contribution for employees for the duration of the rule. However, this is unlikely to be fully internalized especially by young and very mobile employees.

⁹Indeed, in Section 6 we show that migrants start working faster after immigration and apply more promptly after starting a job. Both can be interpreted as supporting evidence that migrants knew about

A second criterion for eligibility refers to the location of prior residence. The reform imposes that migrants must have resided more than 150 km from the Dutch border prior to immigration. This essentially excludes most residents from Belgium, Luxembourg and a small part of Germany from receiving the 30% rule benefits. Moreover, the reform also reduced the maximum duration for benefitting from the 30% rule from 10 to 8 years for newly arriving migrants.

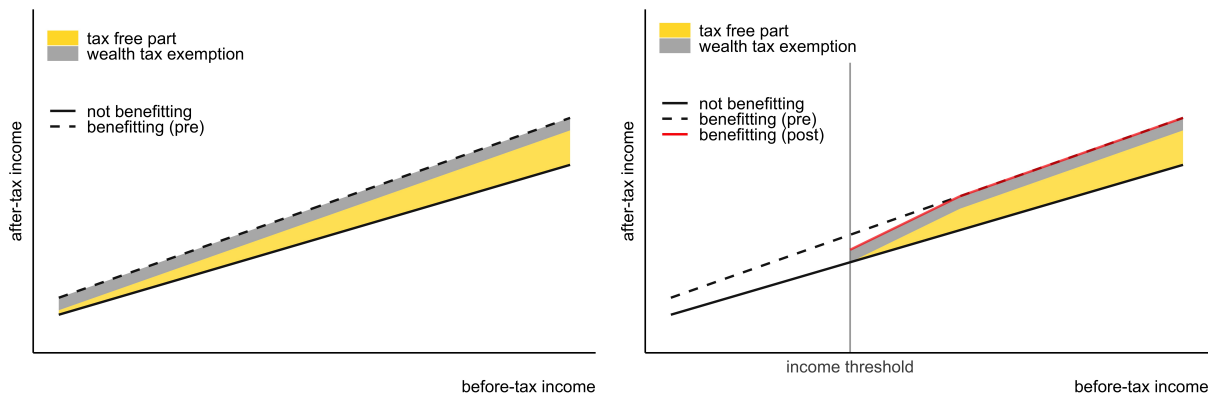
The reform and especially the income threshold were discussed in written exchange between the senate and the house of representatives throughout the year 2011 (starting from the financial agenda published in April, Kamerstuk (2010)). While this may have caused some anticipation where migrants might strategically delay their starting date, there is no discontinuously higher level of arriving migrants in the early months of 2012. In addition, our analysis considers arriving migrants in all years up to 2019, meaning that our results are unlikely to be affected by anticipation effects.

Figure 1 visualizes a simplified relation between before-tax and after-tax income for the pre-reform (left) and the post-reform (right) periods. In the pre-reform period, eligibility was determined by occupation rather than income level. Figure 1 hence plots the hypothetical after-tax income (including the benefit from the wealth tax exemption) for any level of gross income. The tax-free part (yellow) is proportional to the gross income, while the (idiosyncratic) benefit from the wealth tax is assumed to be independent of the level of gross income and is hence pictured as constant (grey). For non-beneficiaries, the entire income is taxed and this is unaffected by the reform. For beneficiaries, the reform introduces a kink in the after-tax income at the income threshold and leaves the benefits unattainable for migrants with income below the threshold.

After the 2012 reform, the magnitude of the tax-free part depends on the distance to the income threshold (τ). Above the threshold, any gross income higher than the threshold and up to 30% of gross income can be paid out as tax-free allowance. For gross income close to the threshold, the income tax benefits are marginally increasing and for higher income, the tax-free part of the income constitutes a constant proportion of gross income. The wealth tax exemption is applicable as soon as the migrant's gross income exceeds the threshold. Therefore, after 2012, the reform introduces a kink in the after-tax income at the threshold and the wealth tax exemption creates a discontinuity where the size depends

the rule before migrating to the Netherlands. Additionally, we note that more firms start using the rule for the first time after the reform, supporting the notion that the application became simpler and more predictable.

Figure 1: Simplified budget constraint before and after the 2012 reform



Note: The figure shows a stylized relation between before-tax income (x-axis) and after-tax income including wealth tax exemption (y-axis) for beneficiaries and non-beneficiaries before (LHS) and after (RHS) the reform. For simplification, the marginal tax rate is assumed to be constant, as well as wealth and the corresponding wealth tax exemption. Beneficiaries are assumed to receive the full 30% tax exemption.

on the migrant's wealth.

Expected effects of the 2012 reform

As described above, the main goal of the reform was to simplify the eligibility process by introducing an income threshold. The new rules have implications for both migrants and employers and may result in (i) an adjustment of the number of arriving migrants, (ii) differential outcomes of the wage bargaining process between arriving migrants and their employers and (iii) an adjustment of working hours (labour supply) of arriving migrants.

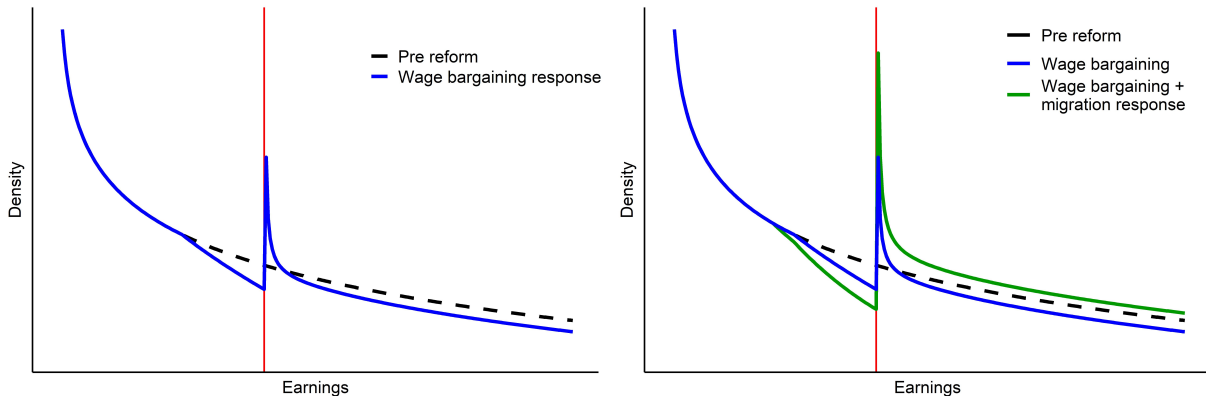
First of all, the reform is likely to affect the migration decision. Eligibility is now determined by the income threshold rather than by an assessment of the scarcity of a migrant's skills. This makes the criteria more objective and the probability of receiving the tax exemption more predictable. Indeed we show in Section 5 that the probability of benefiting from the rule increases substantially after the reform for those with an income above the threshold. The reverse is equally true: arriving migrants with income below the threshold are less likely to benefit after the reform. In a standard neoclassical framework of migration, migrants evaluate costs and benefits and opt for migration if the expected value net of costs is positive (see for example Borjas (1987)). A higher probability of benefiting from the tax reduction increases the benefits from migration. As a result, one would expect to observe an increase in mass in the range above the threshold in the income distribution of arriving migrants. On the other hand, migrants with an income below the threshold

become less likely to benefit and hence have less incentives to migrate. This would be reflected in the income distribution as reduced mass in the range below the threshold. Note that each of these changes are expected to be more apparent ‘closely’ around the threshold, because at very low incomes far from the threshold the share of eligible migrants was very low even before the reform and therefore hardly changed. Similarly, migrants with very high incomes were quite likely to benefit already before the reform and were thus also less affected. We return to this in more detail in Section 5.

Secondly, the reform may affect the wage bargaining process between arriving migrants and their employers. The imposition of a threshold creates a strong incentive for migrants to attempt to attain a wage above the threshold. In the Online Appendix we provide a standard Nash wage bargaining framework and follow Kleven et al. (2014) in showing how a discontinuous jump in the after-tax income at the threshold affects bargaining outcomes. Here we confine ourselves to the predictions, which are fairly intuitive. Wage bargaining responses would be expected to produce bunching at the threshold from both below and above. Bunching from below occurs as workers with income below the threshold manage to bargain up their wage to the threshold, because their gains from reaching the threshold are discontinuous while the loss to the employer is smooth. As a result, the wage distribution of arriving migrants would exhibit reduced mass just below the threshold, and a spike in mass at the threshold. The magnitude of this shift increases with the bargaining power of the worker. Bunching from above occurs as firms bargain down beneficiaries’ wages to gain some of the benefits from the tax reduction that the beneficiary enjoys. Such a response would lower the wage distribution in the range above the threshold and generate even more mass at the threshold. This response increases in magnitude with the bargaining power of the employer. These predictions are visualized in the left panel of Figure 2, while the right panel adds the predicted migration response.

At this point it is important to emphasize that there are two reasons why marginally passing the threshold is beneficial for the employee even if the immediate tax benefits appear fairly small (see Figure 1). First, there are the gains from the wealth tax exemption which may be substantial for some migrants. Second, the rule requires migrants to qualify with their first job in the Netherlands. Applying upon changing jobs in the Netherlands is not possible. That implies that an arriving migrant would need to qualify directly upon arrival to be able to reap the benefits in later years. Since migrants are likely to expect sizeable wage growth in subsequent years, they can typically expect much larger (accumulative) benefits from the 30% rule if they manage to qualify in the year of arrival. Indeed we find

Figure 2: Wage bargaining and migration responses to the introduction of the threshold



Note: The charts visualize the concept of bunching from above and below in the distribution of income in the context of a wage bargaining model. The left-hand side panel illustrates bunching from below (producing a hole below the threshold and excess mass above the threshold) and bunching from above (producing a left shift in mass above the threshold and excess mass at the threshold). The right hand side panel adds the migration response (producing an increase in mass in the range above the threshold).

in our sample of arriving migrants in the Netherlands that the median yearly wage growth is 4.75%. Hence there is a strong incentive to attain a wage above the threshold in order to enjoy the benefits in the medium and long run, even if immediate benefits appear limited.

Thirdly, the income threshold and the resulting kink (notch) may induce a labour supply response in terms of the number of working hours. The literature on bunching (Saez (2010), Chetty et al. (2011), Slemrod (2013) and Kleven and Waseem (2013)) describes how kinks and notches in an income schedule may generate a labour supply response as individuals re-optimize their labour supply in the reaction to the kink (notch). Assuming a positive uncompensated elasticity of labour supply with respect to income, this may induce individuals with an income closely below the threshold to attempt to meet the income threshold by increasing their working hours to benefit from the tax exemption.

In Section 6 we return to the above described predictions in more detail, compare them against the empirical results and provide further evidence to disentangle their relative importance.

4 Data

The empirical analysis is conducted with population-wide administrative data from Statistics Netherlands on migration movements, employment, income, as well as household characteristics for the years 2006 to 2019. These data are combined with information on an

Table 1: Sample construction

| | Pre-refom (2006-2011) | Post-reform (2012-2019) |
|----------------------------------|--------------------------|----------------------------|
| Total inflow (working migrants) | 176,096 | 337,717 |
| - No. of migrants from Belgium | 6,707 | 9,908 |
| - No. of migrants fom Luxembourg | 165 | 401 |
| - No. of migrants in academia | 10,172 | 17,302 |
| - No. of migrants < 30 years | 90,564 | 170,018 |
| Resulting sample size | 78,535 | 155,763 |
| No. 30% rule beneficiaries | 9,354 | 34,817 |

Note: Resulting sample size after removing the above mentioned groups. Note that these are not mutually exclusive.

individual’s beneficiary status for the 30% rule, provided by the Dutch Ministry of Finance (available from 2009 onwards). The information related to migration includes the date of immigration and emigration as well as the country of origin or destination respectively. With respect to income and employment variables, the data contains start and end date per employment spell, taxable income and working hours. Demographic characteristics include age, gender and country of birth. Additionally, the dataset covers information on the users of the 30% rule, namely the start and end date of the eligibility period.

Descriptive statistics

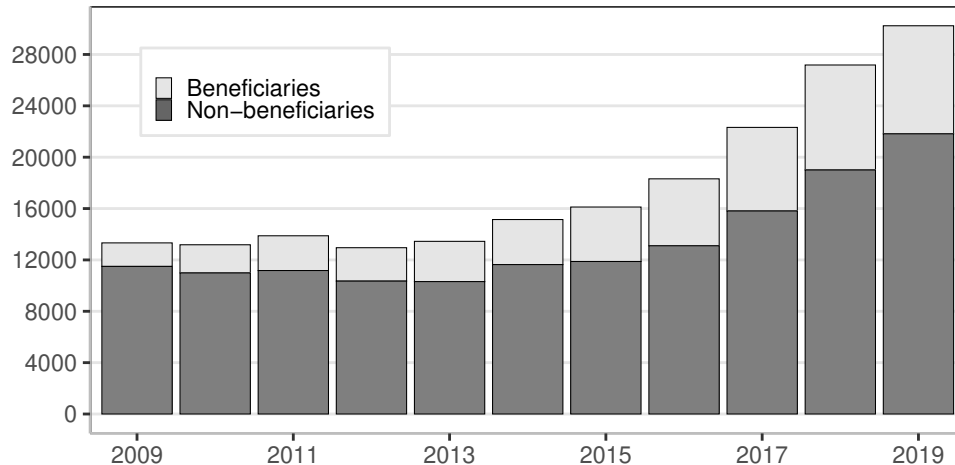
The key outcome of interest is the number of arriving migrants in different parts of the income distribution. For further analysis, we construct a repeated cross-section of annual migrant arrivals. We include migrants that start employment within six months after arriving in the Netherlands. This step is to ensure restricting the analysis to migrants that intend to work in the Netherlands, as opposed to family reunions or migrating partners. Next, to reduce noise from temporary short-term migration, we only include migrants that are employed for at least one full year in the Netherlands. Additionally, three more groups are excluded as the effect of the reform may differ for these groups due to the additional criteria introduced in the 2012 reform. Firstly, migrants from Belgium and Luxembourg, as these are almost always ineligible due to the geographical criteria. Secondly, migrant employees under the age of 30, as a different (lower) threshold applies to them. Additionally, migrants under the age of 30 may be subject to the higher income threshold during their

employment spell upon turning 30 years old. Thirdly, migrants working in academia, as they are always eligible to receive the 30%-rule benefits. Table 1 gives an overview of the number of observations of the excluded groups and the resulting sample size. In total, the sample contains 234,298 observations, consisting of 78,535 migrants arriving in the years prior to the reform and 155,763 arriving after the reform.

The number of arriving migrants has steadily increased over recent years, both for 30%-rule beneficiaries and non-beneficiaries. Figure 3 shows the annual inflow of working migrants, by 30%-rule eligibility. From visual inspection, there is no pronounced change around 2012 in terms of 30%-rule beneficiaries in the overall migration inflow. However, the figure shows the inflow across all income levels, while, as outlined in Section 3, the income threshold’s impact varies across different parts of the income distribution.

Table 2 provides descriptive statistics for our sample. The average age for beneficiaries and non-beneficiaries is around 38 in the year of arrival. Beneficiaries tend to be slightly younger than non-beneficiaries. Slightly more than half of the migrants have resided in Europe prior to immigrating to the Netherlands and this fraction is roughly similar for beneficiaries and non-beneficiaries. The overall second largest group has prior residence in Asia and has a higher representation in the population of beneficiaries than in the population of non-beneficiaries. In terms of the sectors in which migrants work, we find little changes across time. For beneficiaries and non-beneficiaries the “Administrative and

Figure 3: Annual inflow of migrants, by usage of the 30% rule



Note: Inflow of migrants who start working within six months after arrival and stay for at least one full year in the Netherlands. Excluding migrants from Belgium and Luxembourg, employees in academia and migrants under the age of 30 at the time of migration.

Table 2: Balancing table of observable characteristics

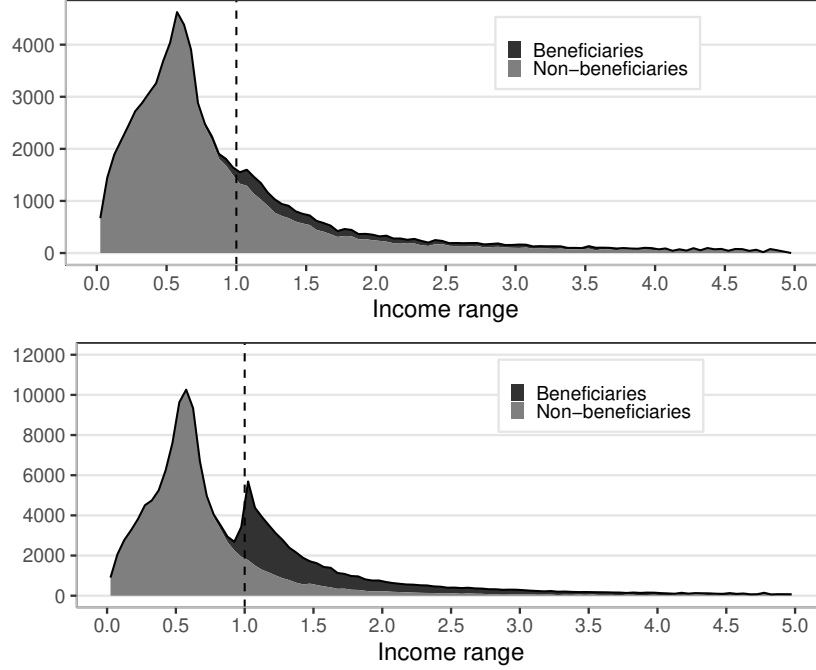
| | Beneficiaries | | Non-beneficiaries | |
|----------------------------------------|---------------|-----------|-------------------|-----------|
| | 2006-2011 | 2012-2019 | 2006-2011 | 2012-2019 |
| Average age | 38.8 | 38.1 | 39.7 | 39.8 |
| Gender (% female) | 23% | 24% | 34% | 38% |
| European | 54.6% | 54.3% | 49.7% | 64.8 % |
| America | 15.3% | 13.0% | 10.4% | 8.2% |
| Asia | 23.2% | 25.5% | 8.9% | 8.2% |
| Other regions | 6.9% | 7.3% | 31% | 18.8% |
| Most frequent sector(s) ^a : | | | | |
| Administrative and support service | 39% | 43% | 46% | 28% |
| Wholesale and retail trade | 17% | 11% | 11% | 9% |
| Manufacturing | 16% | 12% | 6.5% | 7.5% |
| Accommodation and food service | 0.6% | 0.3% | 12% | 10% |
| Other sectors | 22% | 33.7% | 24.5 | 45.5% |

Note: Descriptive statistics of working migrants, based on an annual sample of incoming migrants who start working within 6 months after arrival. Excluding migrants from Belgium or Luxembourg, employees in academia and migrants under the age of 30 at the time of migration. “Other regions” include “unknown”. ^a Sectors following NACE classification.

support service activities” is the largest sector.

We now turn to the income distribution, where we consider labour earnings in the first full calendar year of the arriving migrants. We show the distributions before and after the 2012 reform in Figure 4, where the annual taxable income is normalized relative to the 30%-rule threshold. We make two main observations. First, the income distribution in the pre-reform years is fairly smooth, with a mode around 50 to 75% of the income threshold. There is no discontinuity in the density around the income threshold that was imposed in 2012. Second, in the post-reform period there is a substantial spike in mass above the threshold. The increased density above the threshold is largest closely above the threshold, but also at higher levels of income there is more mass than in the pre-reform period. This comes at the expense of lower mass at lower incomes, with some indication that the largest drop occurs closely below the threshold. Overall, migration increased substantially over time, which is reflected in a higher total number of migrants in the post-reform period.

Figure 4: Counts of migrants by income and beneficiary status in the pre- and post-reform periods



Note: The figure shows the distribution of annual taxable income relative to the 30% rule's income threshold for the inflow of migrants who start working within 6 months after arrival and stay for at least one full year. Excluding migrants from Belgium and Luxembourg, employees in academia and migrants under the age of 30 at the time of migration. Pre-reform period: 2006-2011. Post-reform period: 2012-2019.

5 Empirical approach

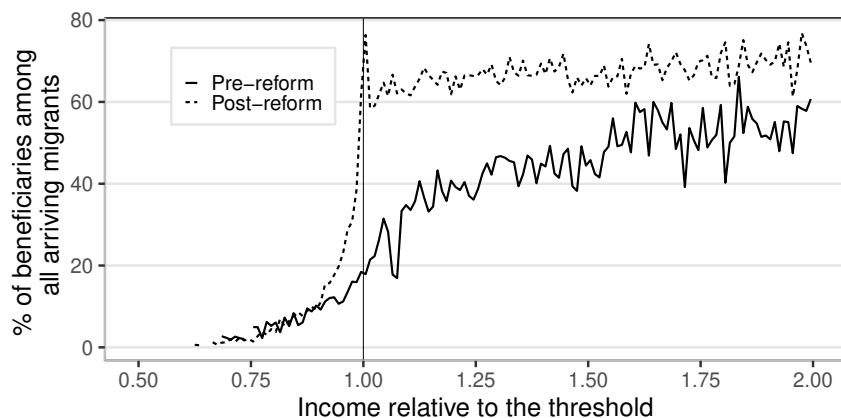
The income threshold's effect on the income distribution of arriving migrants is expected to be strongest locally around the threshold. It may reflect responses in terms of adjusted migration, wage bargaining, or working hours. In the following, we discuss which migrants are affected by the reform and then present two approaches to estimate the impact of the reform on the inflow of migrants in different parts of the income distribution. Firstly, we will compare the densities of the income distribution in the years before and after the reform. Secondly, we will compare the inflow of migrants in different parts of the income distribution in a difference-in-differences (DiD) framework. To do so, we exploit the fact that some parts of the income distribution are unaffected by the reform.

5.1 Affected parts of the income distribution

Figure 5 shows the fraction of beneficiaries relative to all migrants across the income distribution in the pre-reform (2006-2011) and post-reform (2012-2019) periods. The figure shows that the pre-reform fraction of beneficiaries is smoothly increasing with income, indicating that also before 2012 migrants with a higher income were more likely to receive the tax benefits. After the reform and with the imposition of the income threshold, the fraction of beneficiaries sharply increases at the income threshold.¹⁰ The change in the probability of eligibility after the reform is largest closely above the threshold, with an increase of the fraction of beneficiaries of almost 40% points. After 2012, around 65% of those above the threshold benefit, and this share increases only marginally with income.¹¹

Comparing the fractions of beneficiaries before and after the reform, we observe that there are three groups (defined by their income) that are most strongly affected by the reform, on which we will focus in our empirical analysis: (1) those with annual taxable income closely above the threshold, (2) employees with annual taxable income further

Figure 5: Fraction of migrants that benefited from the 30% rule



Note: The chart above shows the fraction of 30%-rule beneficiaries relative to all migrants from the sample per period and in discrete steps of the income relative to the threshold. Pre-reform period: 2006-2011. Post-reform period: 2012-2019.

¹⁰Some exceptions from the income threshold may be possible for employees on parental leave, foster care leave or adoption leave during part of the year. In addition there may be some measurement error in the classification of the sector, meaning that our sample contains some individuals that are exempted from the income threshold requirement because they are in fact academics. This explains why there are some beneficiaries after the reform with an income below the threshold.

¹¹Reasons for an imperfect take-up include ineligibility due to other criteria such as having lived in the Netherlands previously, lack of information about the tax rule, lacking paperwork (e.g. proving previous residence or an employment contract) and unwillingness to apply on the side of the employer or employee.

above the threshold and (3) those with annual taxable income closely below the threshold. Expressed in relation to the income threshold we define these groups as:

- **Closely above the threshold**

We consider three groups: individuals with taxable income at most 1%, 5% or 10% above threshold.

- **Further above the threshold**

This group contains all migrants with an annual taxable income more than 10% above the threshold.

- **Closely below the threshold**

We consider the group with income 95%-100% of the threshold, but also consider a second group below the threshold (90%-95%) due to potential problems with the 95%-100% group. Firstly, measurement error in taxable income could result in an inflated number of observed migrants very closely below the threshold. Secondly, Figure 5 shows that there is some non-zero fraction of beneficiaries closely below the threshold, mainly related to (parental) leave schemes and/or other exceptions from the 30% rule.

Migrants with an income further below the threshold are unlikely to be affected by the reform. Firstly, Figure 5 shows that very few migrants with an income below 90% of the threshold were benefiting from the scheme even before the reform. Secondly, as argued in Section 3, it is unlikely that migrants within this income range are able to bargain up their wage sufficiently to pass the threshold after the reform.

In the analysis below we use the total number of arriving migrants within these groups, thus irrespective of their beneficiary status. We don't take beneficiary status into account because it is highly selective, and in addition it is not unlikely that the reform also affected decisions of non-beneficiaries. As a result, one might interpret all estimates as intention-to-treat, meaning the effect of the reform on total inflow per income range.

5.2 Comparing income densities before and after the reform

Our first approach to estimating the impact of the reform on the number of arriving migrants is a simple comparison of the income distribution of migrants before and after the reform (Figure 4). By doing so, we calculate the change in the number of arriving migrants

in specific income ranges. An advantage of the policy under study is that it is not necessary to extrapolate a distribution to calculate a counterfactual scenario, as with the established bunching estimator (e.g. Saez (2010) and Kleven (2016)). The pre-reform income distribution captures the scenario without an income threshold. The density comparison approach hinges on the identifying assumption that the income distribution of arriving migrants is constant across years absent the reform. Hence, the difference in densities between periods (years) indicates how the distribution of arriving migrants may have changed across income ranges.

To formalize this idea, define the number of arriving migrants with a wage in bin b (defined relative to the threshold) and year t as N_t^b and the total number of arriving migrants in year t as N_t . Under the assumption of a constant wage distribution, we can estimate the total change in inflow for a particular wage bin b by comparing the densities in that bin in a pre-reform year and a post-reform year along with the change in the total inflow of migrants (across the entire distribution). For example, consider the affected wage bin $b = [1, 1.05)$, which we denote by +5% for brevity:

$$\begin{aligned}\Delta_1(+5\%) &= \left(\frac{N_{post}^{+5\%}}{N_{post}} - \frac{N_{pre}^{+5\%}}{N_{pre}} \right) \cdot N_{post} \\ &= N_{post}^{+5\%} - N_{pre}^{+5\%} \left(\frac{N_{post}}{N_{pre}} \right).\end{aligned}\tag{1}$$

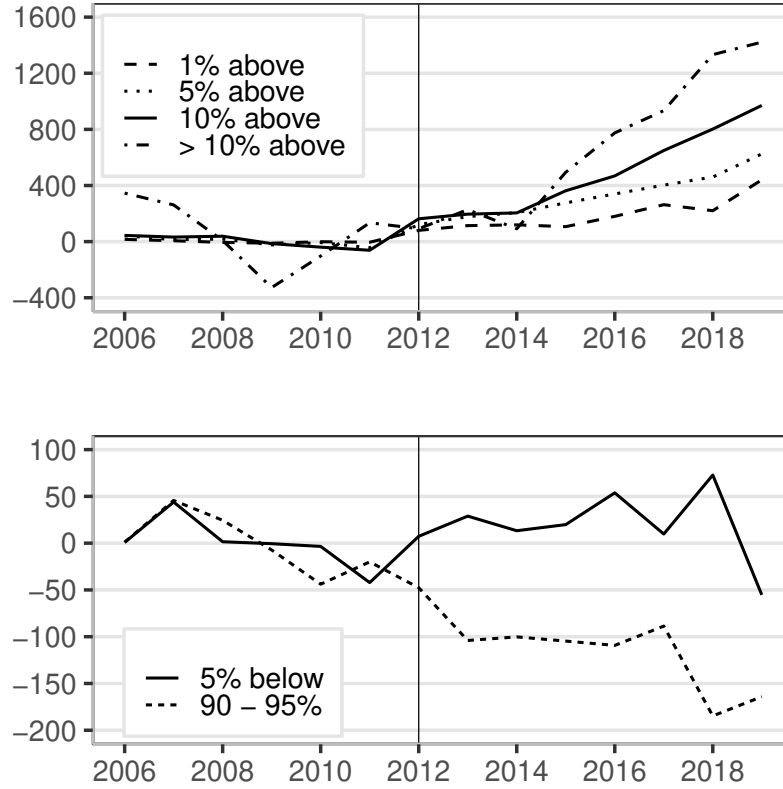
Put differently, the impact of the reform is the difference in the number of arriving migrants in a certain wage bin between the pre and post year, where the post year is scaled using the relative change in the total number of arriving migrants.¹²

Using the above decomposition of income densities, we compare total migrant inflow for a given year with the income distribution in the pre-reform period (2006-2011). The goal is to calculate the total change in inflow of migrants for the affected groups (income bins as defined in Section 5.1).

Figure 6 shows the (yearly) estimates for the (affected) groups above and below the threshold compared to the pre-reform period. The comparison reflects a larger and increasing inflow of migrants above the threshold (implying an additional inflow of up to 1,200 migrants in the range 10% above the threshold) and slightly less inflow in the income range below the threshold. For the change in the inflow below the threshold, we note that the calculated effect for the 5% below the threshold group is close to 0, while the estimated

¹²The identifying assumption here is that in the absence of the reform $\frac{N_{pre}^{+5\%}}{N_{pre}} = \frac{N_{post}^{+5\%}}{N_{post}}$.

Figure 6: Estimated additional inflow of migrants by income range based on density comparison



Note: The upper panel shows the estimated additional number of migrants per year in income bins above the threshold, based on the density comparison. For every year and income range we compare the density with the pre-reform density (averaged over 2006-2011), see equation (1).

effect for the group 90 to 95% below the threshold is slightly negative (around 100 fewer arriving migrants per year compared to the pre-reform period).

This comparison of densities gives the change in the number of migrants in a given year in an income range compared to the pre-reform income distribution. The comparison of density bins however encompasses the entire income distribution. As the density integrates to one, additional (not uniform) inflow may result in a change in proportions across income bins. If the density in bins other than the one under consideration also changes across years, that would impact the estimate of the changed inflow in the particular bin. To address such concerns we now move to a DiD model, where we can ensure to select a control group that is unaffected by the reform.

5.3 Difference-in-Differences

With this framework we compare the change in the number of migrants in an affected income bin against the change in an unaffected bin. We define the control group as the inflow of migrants with income well below the threshold (50%-90%) as these are unlikely to be affected by the rule. This group is sufficiently far away from the threshold so as to contain almost exclusively non-beneficiaries in both the pre- and post-reform periods. The key difference from the previous analysis (density comparison) is that the DiD approach compares the inflow of migrants to specifically selected sections in the income distribution, as opposed to the entire income distribution. Given a common trend in the number of arriving migrants within the affected and control bins, the DiD estimate can be interpreted as the causal effect of the introduction of the income threshold.

Additionally, we refer to Donald and Lang (2007) who posit that standard (asymptotic) inference is limited in the case of a small number of groups and a policy that applies to all members in the group.¹³ They conclude that homoscedasticity of the group error term may hold if the number of observations per group is large or if there are no within-group varying characteristics and the number of observations is the same for all groups (Donald and Lang (2007)).

In our setting the control and affected group(s) are quite different in size. The control group contains at least 4,350 individuals per year in the pre-reform period, while the smallest affected group (1% above the threshold) only contains 51 individuals (average inflow per year in the pre-reform period). Hence, we partition the control group into a set of smaller control groups such that its size matches the treatment group in 2011. Depending on the size of the treatment group, this procedure yields a large(r) amount of control groups. We provide visual evidence that the common trend in the pre-reform years holds for all of these smaller control groups (see Figure 7). In addition we show that the post-reform trends are very similar across all the control groups, underlining that the results are not sensitive to the choice of the exact boundaries of the control group range.

With the outcome y for group g and period p (pre-reform/post-reform period), the basic

¹³According to Donald and Lang (2007), the main problem is that with a small number of groups (two in the most standard DiD design), t -statistics from standard methods (OLS, feasible GLS and two-stage estimation) are not normally distributed. This is due to the distribution of common group errors. Asymptotic validity of the standard errors is based on the number of groups going to infinity.

DiD model is:

$$y_{gp} = \lambda_g + \theta_p + \beta D_{gp} + \varepsilon_{gp} \quad (2)$$

where λ_g and θ_p denote group and period dummies, respectively. The term $D_{gp} = \mathbb{I}(g = \text{treatment group}) \times \mathbb{I}(t > 2012)$ indicates assignment to the treatment group in the post-reform period. The coefficient β captures the reform effect, which compares the changes in groups across the two periods.

Given the dynamic evolution of the total number of migrants (see Figure 3), the simple pre/post specification may fall short to capture the evolution of impact over time. Therefore, we also estimate a dynamic specification, which allows the treatment effect to differ across years. We expect a time-varying impact, as it may take time for firms and individuals to learn about the new rule and adapt hiring policies and migration decisions accordingly. We estimate dynamic treatment effects per year t using the event-study specification:

$$y_{gt} = \lambda_g + \theta_t + \sum_{j=-m}^q \beta_j D_{gj} + \varepsilon_{gt} \quad (3)$$

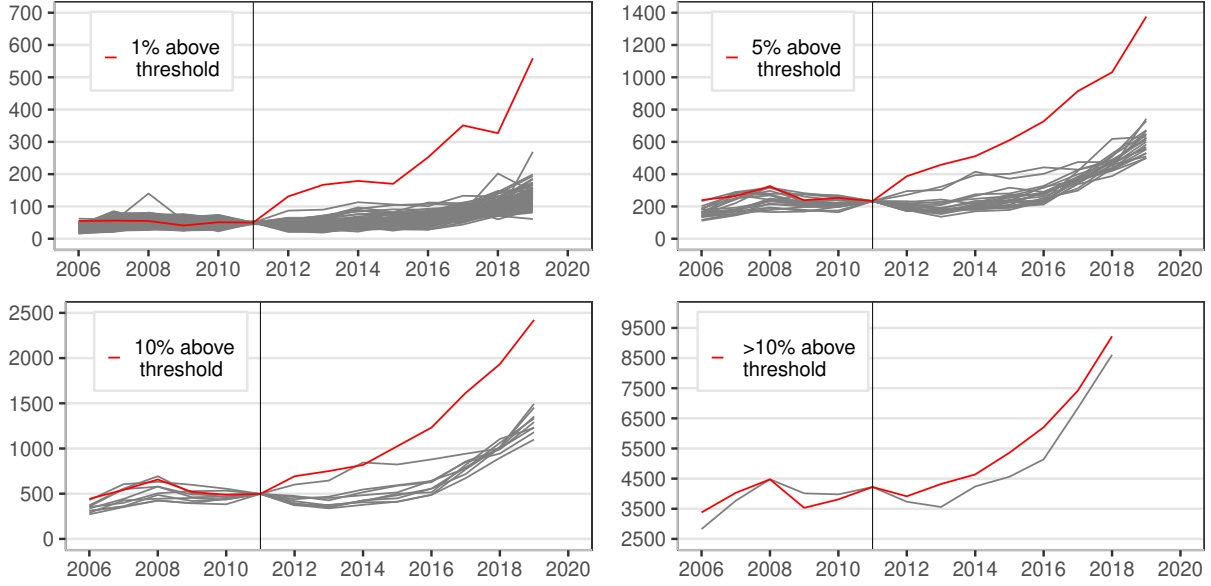
where θ_t is a set of dummy variables for each year t and D_{gt} captures the affected group in year t . The specification includes m leads and q lags, allowing the treatment effect to vary by year.

We now turn to the main results of the DiD analysis. We first present graphical evidence on how the number of migrants within the specified income ranges develops over time. Then we report static DiD estimates, followed by event-study estimates. Finally, we show that our findings are robust against a range of alternative specifications.

Arriving migrants above the income threshold

Figure 7 shows the number of arriving migrants in the affected income ranges above the threshold. In each figure the control groups are subsets of the $[0.5, 0.9)$ income range, defined such that they match the size of the respective affected group in 2011, the year prior to the reform coming into effect. From visual inspection, the number of arriving migrants in the affected and control group(s) is parallel until 2012, but diverges immediately as of 2012. The relative increase is most pronounced for the affected groups closest to the income threshold (1% and 5% above the threshold). All control groups follow very similar paths (both before and after the reform) implying that a differently chosen subset of these groups would lead to very similar results.

Figure 7: Number of annual arrivals in treatment and control group(s)



Note: The charts show the annual arrival of migrants in the sample, per income group relative to the threshold. The control group (grey) is the number of arrivals in the range corresponding to 50% to 90% of the threshold, partitioned such that the groups have the same size as the affected group in 2011, the year before the reform.

Table 3 presents the estimates for the DiD specification (Equation 2), which compares averages for affected and control group in the pre- and post-reform periods. The estimated treatment effects for the treatment groups just above the threshold are positive, large and statistically significant, while the estimated treatment effect for arriving migrants with income further above the threshold ($> 10\%$) is positive and sizeable, but not statistically significant. The DiD estimates amount to an average additional inflow of 192 and 384 migrants per year in the 1% and 5% groups above the threshold, respectively, which correspond to a 376% and 148% increase relative to the pre-treatment average in the treatment group. The estimate for the group at most 10% above the threshold (108% increase) is also sizeable and significant. Although the estimate for the group more than 10% above the threshold is positive, it is relatively small compared to the pre-reform average inflow and not statistically different from 0.

To allow for dynamically changing treatment effects, we present event-study estimates (Equation 3) in Figure 8. For the treatment groups close to and above the threshold (1%, 5% and 10% above the threshold), the effect is positive after 2012, significant and increasing over time. The treatment effects for the those at least 10% above the threshold are positive, but imprecisely estimated and not significantly different from 0. Notably, for all groups,

Table 3: DiD estimates for treatment groups above the threshold

| | <i>Dependent variable: Number of arriving migrants</i> | | | | |
|-----------------------------------------|----------------------------------------------------------|-------------|--------------|-------------|---------------|
| | Treatment group, defined as income relative to threshold | | | | |
| | 1% | 5% | 10% | > 10% | > threshold |
| Treatment group × Post-reform period | 192*** (16) | 384*** (69) | 548*** (155) | 551 (1,567) | 1,249 (1,823) |
| Observations (bins) | 1,330 | 294 | 126 | 28 | 28 |
| Included individuals | 80,869 | 85,567 | 88,610 | 146,942 | 168,223 |
| Adjusted R ² | 0.31 | 0.36 | 0.34 | 0.19 | 0.22 |
| Pre-reform yearly average (individ.) | 51 | 259 | 524 | 3,908 | 4,376 |
| Effect relative to pre-reform | | | | | |
| yearly average | 376% | 148% | 108% | 14% | 28% |
| Accumulated effect (individ.) | 1,496 | 3,060 | 4,379 | 2,460 | 8,186 |

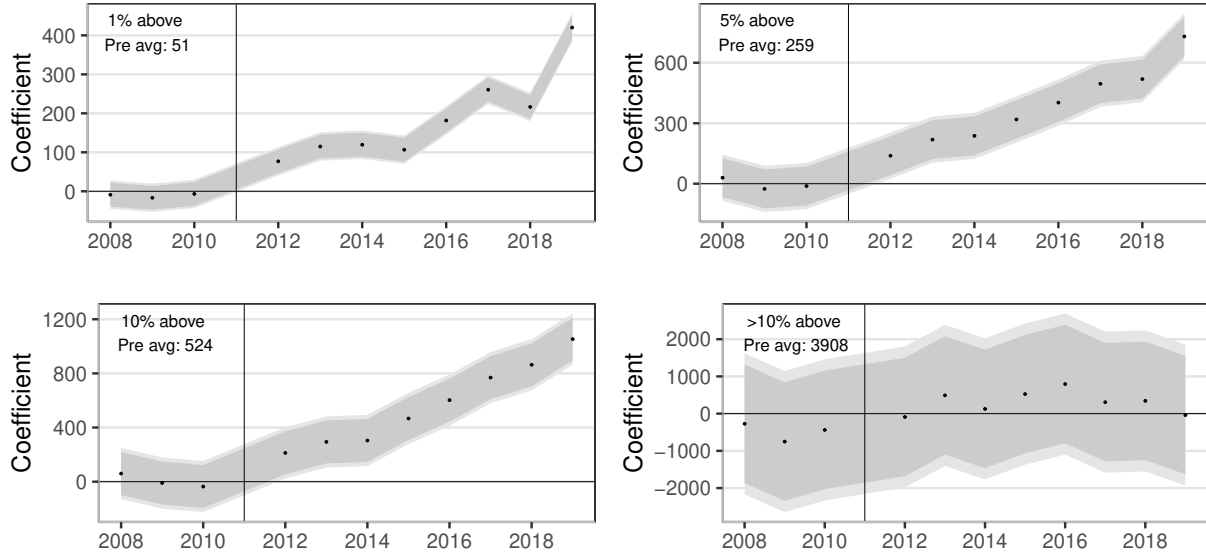
Note: The table shows the coefficients for the interaction effects from the DiD specification (β in Equation 2). Standard errors in parenthesis. "Observations (bins)" captures the amount of bins, containing both the affected income range and control bins. "Included individuals" refers to the total number of individuals in both affected and control group in all years of the sample (2006-2019). "Pre-reform yearly average" refers to the affected group. The control group contains (subgroups of) individuals with income the range 50 – 90% of the threshold. "Accumulated effect" is calculated as the sum of the coefficients for the interaction effects from the event-study specification.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

the estimated treatment effects are very close to 0 in the years prior to the reform, which supports the common-trend assumption. The increase of the treatment effect over time may arise due to adjustment effects, where both employers and employees learn about the rule and firms require some time to update their recruitment strategies.

Arriving migrants below the income threshold

We proceed by examining whether the estimated increase in arriving migrants above the threshold is mirrored by a decrease of migrants below the threshold, as discussed in Section 6. Table 4 shows estimates from the DiD model for a treatment group consisting of arriving migrants with income in the range of 5% below and 90-95% of the threshold. The treatment effect amounts to an average increase (decrease) of 26 (101) migrants per year, which is about 9% (-33%) of the pre-reform average. However, this is imprecisely estimated and not significantly different from 0 for both groups. The event-study estimates (Figure 9) reflect a similar picture. For the 90-95% group we find a negative effect, although statistically not significant until 2017, which is decreasing slightly with time. For the 95-100% group the

Figure 8: Event-study estimates, treatment groups above the threshold



Note: The charts show the coefficients for the interaction terms in the event-study approach, along with the corresponding 90% and 95% confidence intervals. ‘Pre avg’ is the average number of arriving migrants in the respective income bin in the pre-reform period.

estimates are never significantly different from zero.

Overall, the results suggest that the reform leads to more migrant arrivals with an income above and close to the threshold. This effect is increasing over time, as reflected in the increasing estimates from the event-study specification. This dynamic effect is in line with the notion that both employers and employees take some time to learn about the rule and or have some delay in adapting the recruitment process to hire internationally. The estimated additional inflow above and close to the threshold is disproportionately larger than the small (negative) effect for migrants with an income below the threshold.

Notably, the estimated change in the inflow of migrants is very similar in both sign and magnitude for both the DiD approach (Section 5.3) and the estimates based on the entire income distribution (Section 5.2).¹⁴

5.4 Robustness

We consider a range of alternative specification and sample-selection choices. The most prominent may be the choice of the control group in the DiD analysis, which is dictated

¹⁴For a comparison of the estimates from both approaches, see Table A.1 in the Online Appendix).

Table 4: DiD estimates for treatment groups below the threshold

| | <i>Dependent variable: Number of arriving migrants</i> | |
|-------------------------------------------------|--------------------------------------------------------|---------------------|
| | 5% < threshold | 90-95% of threshold |
| Treatment group × Post-reform period | 26 (66) | −101 (81) |
| Observations (bins) | 280 | 224 |
| Included Individuals | 82,878 | 79,524 |
| Adjusted R ² | 0.19 | 0.18 |
| Pre-reform yearly average (individuals) | 274 | 300 |
| Effect relative to pre-reform yearly average | 9% | −33% |
| Accumulated effect (individuals) | 240 | −848 |

Note: The table shows the coefficients for the interaction effects from the DiD specification (β in Equation 2). Standard errors in parenthesis. "Observations (bins)" captures the amount of bins, containing both the affected income range and control bins. "Included individuals" refers to the total number of individuals in both affected and control group in all years of the sample (2006-2019). "Pre-reform yearly average" refers to the affected group. The control group contains (subgroups of) individuals with income the range 50 – 90% of the threshold. "Accumulated effect" is calculated as the sum of the coefficients for the interaction effects from the event-study specification. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

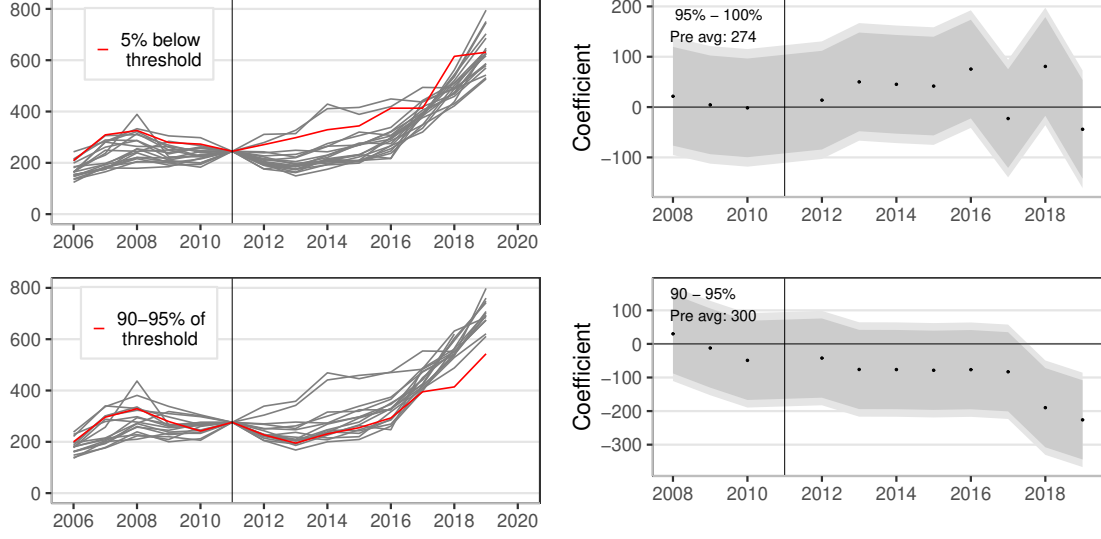
neither by theory nor by policy. Nevertheless, Figure 7 shows that all control groups that we use (which are all subgroups of the 50-90% range of the income distribution) follow very similar trends. As a result, we could select any subset of these control groups and find similar results.

In our main analysis, we include all migrants from Germany. However, migrants from Germany that lived less than 150 km from the Dutch border are ineligible after the reform.¹⁵ In column 2 of Table 5 we show that excluding all Germans from the analysis has only a negligible effect on the DiD estimates, which is unsurprising since Germans make up less than 10% of the total number of migrants.

In another robustness check, we restrict the sample to workers that work at a single firm for their entire first year. This avoids any noise that arises from income extrapolation across months for workers that change employer throughout the year. Again, in column 3

¹⁵Since we do not observe the exact location of residence we cannot identify those that come from these regions close to the border.

Figure 9: Number of arrivals and event-study estimates, migration below the threshold



Note: The left-hand side chart show the number of (annual) arrivals of migrants in the sample, per income group relative to the threshold. The control group is the number of arrivals in the range corresponding from 50% to 90% of the threshold. It is normalized such that it matches the treatment group in 2011, the year prior to the reform being in effect. The right-hand side chart shows the coefficients for the interaction terms in the event-study specification, along with the 90% and 95% confidence intervals. ‘Pre avg’ is the average number of arriving migrants in the respective income bin in the pre-reform period.

of Table 5 we show that results remain very similar.

Next, we provide robustness estimates based on income from the year of arrival, extrapolated into an annual income.¹⁶ The advantage is that this measure reflects earnings from the first employment in the Netherlands, but the extrapolation is also prone to measurement error.¹⁷ The estimates (column 4 of Table 5) are again consistent with our baseline results, although somewhat smaller in magnitude. The effect for the group 5% below the threshold is positive and statistically significant for the partial-year sample. This difference might result from the partial-year income measure underestimating realized income and hence overestimating the number of migrants in an income range just below the threshold.

A final potential concern is related to the impact of the wage growth on the definition of the affected groups. More specifically, it could be argued that above average wage growth may shift the entire income distribution and hence induce some spurious effect in the count of migrants per income range. However, this concern is not grave in our setting. Firstly,

¹⁶The partial-year income is linearly extrapolated based on the realized income in the months (weeks) of working in the Netherlands, in the year of arrival.

¹⁷It may not fully capture a change in compensation, a potential bonus or it may be lower if an employee decides to alter working hours during the year (potentially with the aim to meet the threshold).

Table 5: DiD estimates: robustness against alternative samples and measurements

| <i>Dependent variable: Number of arriving migrants</i> | | | | |
|--------------------------------------------------------|-----------------|-----------------|-----------------|---------------|
| | Baseline | Excl. Germans | Non-changers | Partial year |
| 1% above threshold | 192*** (15.8) | 187*** (14.2) | 176*** (13.8) | 89*** (20.4) |
| 5% above threshold | 384*** (68.5) | 377*** (63.8) | 328*** (57.9) | 264*** (78.8) |
| 10% above threshold | 548*** (155.3) | 554*** (141.9) | 448*** (135.5) | 364 (174.3) |
| > 10% above threshold | 551 (1,567) | 466 (1,486.5) | 1,081 (1,010.0) | 262 (1,489.0) |
| > threshold | 1,249 (1,822.7) | 1,130 (1,733.9) | 1,743 (1,179.3) | 712 (1,753.7) |
| 5% below threshold | 26 (66.1) | 26 (64.1) | 32 (44.1) | 295*** (81.7) |
| 90-95% of threshold | -101 (81) | -90 (75) | -64 (52) | 6 (83) |

Note: The table shows the DiD treatment effects (coefficient for interaction terms in DiD specification Equation 2) for 3 samples (in columns: baseline, baseline excl Germans, baseline with workers that stay at one firm for an entire year, partial year). The rows capture the treatment groups. The control group contains migrant inflow in the 50% to 90% range relative to the threshold. "Accumulated effect" is calculated as the sum of the coefficients for the interaction effects from the event-study specification. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

we express the income distribution relative to the indexed income threshold.¹⁸ With the indexed threshold, the modes of the income distribution in the pre- and post-reform periods are in a similar location (see Figure 4). Secondly, above average wage growth would result in a right shift of the entire income distribution, but would not explain bunching at the threshold.

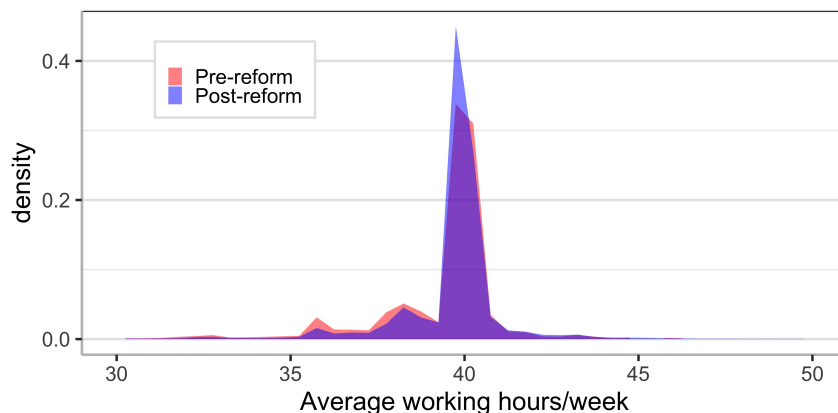
6 Discussion

We have shown that there is a substantial increase in the inflow of migrants in the wage bins 1%, 5% and 10% above the threshold. Also the number of arriving migrants with income more than 10% above the threshold appears to have increased, although this change is not statistically significant. The number of arriving migrants with income just below the threshold appears to remain unaffected.

We now compare these findings against the predictions discussed in Section 3, to investigate what drove these changes. Firstly, we compare working hours of beneficiaries before

¹⁸The income threshold is indexed to the average consumer price growth in the two previous years (as is the case for all taxation brackets, e.g. Uitvoeringsbesluit (1965)). Year-on-year CPI growth is strongly correlated with the year-on-year change in the labour price index and wage costs.

Figure 10: Distribution of average weekly working hours



Note: The chart shows the densities of average weekly working hours for beneficiaries in the pre-reform (2006-2011) and post-reform (2012-2019) periods. Weekly working hours are averaged by person and year, for the weeks in which an individual was employed.

and after the reform to examine whether migrants have increased their labour supply in order to pass the earnings threshold. Secondly, we assess to what extent our findings are in line with the predicted wage bargaining responses and the predicted migration responses. Finally, we provide some secondary findings on firm and worker behaviour that support the notion that the reform improved transparency and predictability of the rule.

One way for a migrant to increase their income to meet the threshold is to increase working hours (labour supply). To examine to what extent this adaption takes place, we compare the distribution of average weekly working hours of beneficiaries before and after the reform in Figure 10. The distribution is concentrated at 38-40 hours in the pre-reform period and remains virtually the same after the reform.¹⁹ Given the similarity of the distributions, a behavioural response related to working hours is unlikely to be a major force of adjustment explaining the increase in arriving migrants with wages above the threshold.

Next, we consider the wage negotiations between prospective migrants and their employers. As explained in Section 3 and in the Section B in the Online Appendix, we expect bunching from below and bunching from above. Bunching from below occurs when workers have sufficient bargaining power, and would result in reduced mass below the threshold and a spike in mass at the threshold. While we observe a spike at the threshold, we find little

¹⁹Also for non-beneficiaries the distribution of average weekly working hours hardly changes, indicating the absence of a time trend (see Figure A.1 in the Online Appendix).

evidence for reduced mass below the threshold. We conclude that it is unlikely that the additional migrants would, absent the reform, have arrived with wages below the threshold. Bunching from above occurs when employer bargaining power is sufficiently large, and would result in a spike in mass at the threshold and lower mass at ranges above the threshold. Our finding of substantially increased arrivals closely above the threshold is in line with this prediction, but the fact that also the number of arrivals at income ranges further above the threshold increased, is not. Such increases can only be explained by a substantial ‘migration response’: additional migrants that arrived specifically due to the large post-reform likelihood of receiving the tax benefit. Combining these results, we arrive at the following conclusions:

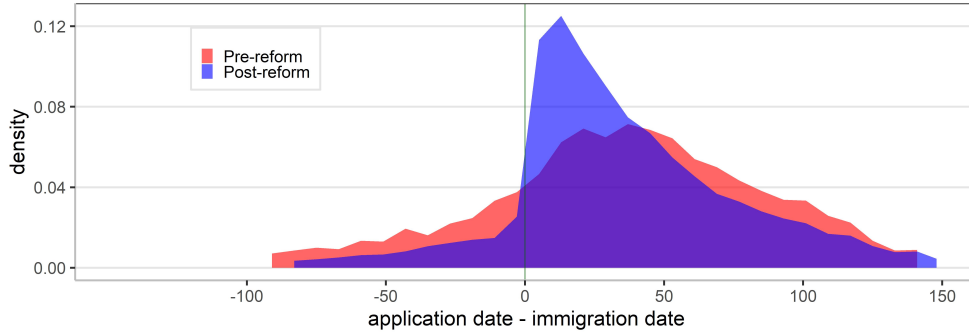
1. Our results are inconsistent with ‘bunching from below’, suggesting that worker bargaining power is limited.
2. ‘Bunching from above’ may have contributed to the increase in observed salaries closely above the threshold, suggesting that employer bargaining power is substantial.
3. An increase in arriving migrants (‘migration response’) is likely to be the main driver of the substantially higher inflow of migrants with wages above the threshold.

With the conclusion that additional migration is the main driver of the increase in arriving migrants above the threshold, we now present some secondary findings that support the idea that the reform has been able to attract more migrants because it improved the transparency and predictability of the application.

Explaining the migration response

We investigate whether the substantial migration response can be explained by increased transparency of the rule: by imposing an income threshold it may have become easier for potential employees and their employers to assess whether they would be eligible for the benefits before applying. Employers would for example be able to actively advertise the tax exemption in their recruitment efforts. As a result, one would expect migrants (to be more likely) to arrive with knowledge about the rule. We assess this prediction by considering the duration between the date of immigration and the date of application, as a proxy for knowledge about the rule. Increased knowledge about the rule would translate into faster applications after arrival. We find that this is indeed the case. In Figure 11 we show the

Figure 11: Distribution of time between application and immigration



Note: The chart shows the distribution of the difference between application date and immigration date. Positive values mean that the application was filed after the immigration date. Negative values mean that the application was filed before the immigration date. The figure refers to the sub-sample of 30%-rule beneficiaries.

distribution of the duration between arrival and application for beneficiaries, for the pre-reform period (in red) and the post-reform period (in blue). After the reform, migrants file their application for the tax exemption much quicker after their arrival.²⁰ These changes support the notion that migrants are more likely to (i) know about the tax exemption at the time of migration and (ii) have taken the tax exemption into account when making the decision to migrate.

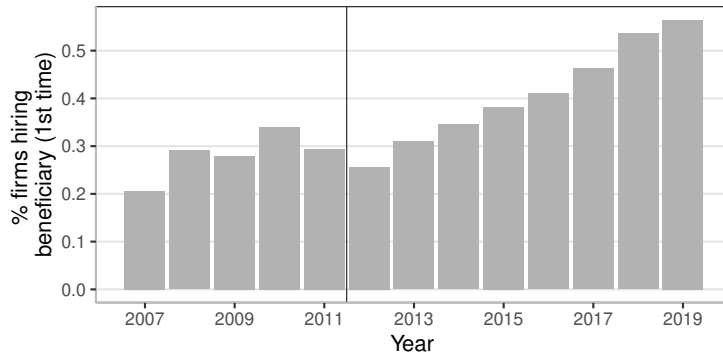
We can also consider the firms that hire migrants eligible for the 30% rule. Our interest is in the distinction between firms expanding their recruitment of beneficiaries versus firms that *start* hiring beneficiaries for the first time. On the one hand, firms that have been hiring beneficiaries prior to the reform are already informed about the rule, the application process and potential firm benefits from hiring beneficiaries. Hence, those firms barely face information frictions and could easily expand recruiting beneficiaries. On the other hand, improved transparency of the rule may have incentivized a broader range of firms to apply. We provide two statistics to assess the impact on the type of firms.

First, we show the share of firms that hire a beneficiary for the first time. We find that the share of all firms that hire a beneficiary for the first time increases steadily after 2012 (Figure 12). This observation is in line with the idea that the reform simplified access to the tax rule for a larger range of firms. In addition, this finding might be driven by a larger range of occupations that now fulfill eligibility criteria.

Second, we compute the hiring concentration of firms (Herfindahl-Hirschmann-Index,

²⁰In the Online Appendix, we show that also relative to the start of employment the application is filed earlier (see Figure A.2).

Figure 12: Percentage of firms that hire a beneficiary for the first time



Note: The chart shows the fraction of firms that hire a migrant benefitting from the 30% rule for the first time (in firm history), relative to the total number of active firms in a given year. Active firms are defined as firms that employ workers in a year. The black horizontal line marks the year 2012, when the reform came into effect.

Table 6: Hiring concentration per period

| Group | Pre | | Post | | Difference |
|---------------|-------|----------|-------|----------|------------|
| | Mean | Std. dev | Mean | Std. dev | |
| Beneficiaries | 0.286 | 0.054 | 0.375 | 0.070 | 0.088* |
| Migrants | 0.221 | 0.020 | 0.278 | 0.042 | 0.057** |
| Employees | 0.171 | 0.055 | 0.178 | 0.074 | 0.006 |

Note: Average and standard deviation of annual Herfindahl-Hirschmann-Index over pre-reform (2006-2011) and post-reform (2012-2019) period. The last column shows a t-test for the difference per period. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

HHI) per year (sum of the squared share of beneficiaries hired per firm). The HHI for hiring beneficiaries increases by approximately 30% in the post period, while it only increases slightly by 3.5% for all hired workers (see Table 6). While these two findings appear somewhat contradictory, they can certainly be reconciled: at the same time we see that an increasing number of firms engages in hiring beneficiaries, but that the bulk of beneficiaries are (increasingly) hired by a relatively small number of firms.

Our results are consistent with the fact that the reform has been able to attract more migrants. To quantify the impact we can compute the implied migration elasticity, capturing the relation between the percentage change in migration after the reform and the percentage change in the net-of-tax rate. Section C in the Online Appendix outlines back-of-the-envelope calculations in which we compute the migration elasticity for all migrants with income above the threshold. One crucial assumption when calculating the change in

the net-of-tax rate is to what extent migrants factor in the higher probability of receiving the tax benefits after the reform.²¹ We consider two scenarios. Firstly, prospective migrants with income above the threshold do not expect to receive benefits before the reform, but do so after the reform as the income threshold makes the tax rule more predictable. This scenario gives an upper bound for the change in the average tax rate and yields an elasticity of 2.1 as a lower bound. Alternatively, we can assume that prospective migrants consider the true probability of getting the tax benefits (as shown in Figure 5) and the corresponding change in the *expected* net-of-tax rate. In this scenario, we find a large elasticity of 10.5.

When interpreting the large magnitude of this elasticity, it is important to keep in mind that our estimate may somewhat overstate the true elasticity for two reasons. First, the calculation is based on income and taxes in the first year of arrival. Therefore, the concept does not incorporate that migrants may anticipate any increases in benefits due to wage growth (and hence a higher untaxed part of the income). This is particularly relevant in our setting where tax benefits last for up to eight years (compared to for example three years in the case of Kleven et al., 2014). Furthermore, the wealth tax exemption that migrants in the Netherlands benefit from, offers additional benefits which may be partly responsible for the substantial migration response.

Given the large additional migration inflow and the effective lower taxation for beneficiaries, one may want to consider the implications for the government budget as well. The Online Appendix shows some back-of-the-envelope calculations which are based on a number of simplifying assumptions.²² We find that the net effect from the additional migration on the government budget is likely to be positive. Intuitively, this finding is driven by the fact that, although beneficiaries pay less taxes than regular migrants, they still pay taxes on 70% of their income. Hence, the tax base increases and this increases tax revenues more than the decrease in tax revenues due to a higher fraction of beneficiaries.

²¹For the net-of-tax rate, we use the average tax rate, as migration is an extensive margin decision. We define migration elasticity as $\frac{\% \text{change in migration}}{\% \text{change in net-of-tax rate}}$, where the net of tax rate is $1 - \text{average tax rate}$. We obtain information for the tax rates and social security payments from the OECD Tax-Benefits Calculator (<https://www.oecd.org/social/benefits-and-wages/tax-benefit-web-calculator/>) and calculate a population weighted mean of the average tax rate for (non-)beneficiaries with income above the threshold.

²²More specifically, we assume that migrants do not crowd out domestic workers, and calculate individual tax rates for a single individual (abstracting from joint taxation with spouses and/or tax exemptions for children). Using the control groups from the DiD as described earlier, we estimate the additional number of migrants after the reform. We calculate the additional (reduced) tax revenue from those migrants along with the loss of tax revenue from granting the benefits to more migrants (as reflected by the share of beneficiaries).

7 Conclusion

In this paper we examine the effects of a preferential income tax scheme for foreigners on migration of skilled workers to the Netherlands. We exploit a reform of an existing scheme that introduced an income threshold for eligibility in 2012. As a result, a broader range of migrants qualified and the rule became simpler and more predictable. Comparing migrant inflow in various income ranges over time, we estimate the reform’s impact on arriving migrants. We find large and statistically significant increases in the arrival of migrants with income closely above the income threshold. The increase amounts to a doubled inflow in the income range of at most 10% above the threshold. The impact on the number of arriving migrants with income closely below the threshold is small and statistically insignificant.

We argue that these effects are mainly driven by an increase in the number of arriving migrants, and to a lesser extent by a wage bargaining response. We find more evidence for employers bargaining down wages for migrants with earnings above the threshold (thereby acquiring some of the tax benefits), than for employees with earnings below the threshold bargaining up their wages. Finally, we show that the scope for a labour supply response is small, as most beneficiaries already work (almost) full time before the reform.

Why was the reform successful in attracting additional migrants? We hypothesize that increased transparency and predictability of the application process and eligibility rules played an essential role. The duration between application for the tax benefits and start of employment decreases substantially after the reform, suggesting that migrants are more likely to be informed about the tax benefits upon arrival. While most existing evidence pertained to the very top of the income distribution (or very specific occupations only), our findings are based on a much wider range of the income distribution. As such we find that tax-incentives are an effective policy tool in attracting international migrants also for mid-range earnings levels.

Our study focuses on the direct migration response to tax incentives, while two further considerations are essential for policy making. First, increased migration may generate negative or positive spillovers on domestic employment and wages (e.g. the overview by Dustmann, Schönberg, and Stuhler, 2016). Studying such effects is beyond the scope of this paper and may constitute a direction for further research. Nevertheless, impacts on natives are likely of second-order magnitude given that even our largest estimate for the accumulated additional number of migrants (8,186, Table 3) is still only a small fraction of

aggregate Dutch employment. Second, it is likely that attracting skilled workers through tax incentives impacts the countries of origin of the migrants. Again, while important, such considerations are beyond the scope of this study.

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Online Appendix

A Additional empirical results

Table A.1: Estimates from density comparison and DiD

| Affected group | Avg effect | | Total effect | |
|----------------|------------|-------|--------------|-------|
| | Density | DiD | Density | DiD |
| 1% above | 196 | 192 | 1,578 | 1,496 |
| 5% above | 326 | 384 | 2,610 | 3,060 |
| 10% above | 477 | 548 | 3,816 | 4,379 |
| > 10% above | 632 | 551 | 5,680 | 2,460 |
| > threshold | 1,109 | 1,249 | 5,055 | 8,186 |
| 5% below | 19 | 26 | 150 | 240 |
| [0.9, 0.95) | -113 | -101 | -903 | -848 |

Note: The table above shows the estimated effect from the density comparison and the DiD estimation. For each method, the table shows the estimated average additional migration in the post-reform period as well as the estimated accumulated effect. The accumulated effect is based on the comparison of the pre- and post-reform period density and the sum of estimated annual additional inflow respectively.

Table A.2: DiD estimates for partial-year sample

| <i>Dependent variable: Number of arriving migrants</i> | | | | | | | |
|----------------------------------------------------------|------------|-------------|-------------|-------------|-------------|-------------|-----------|
| Treatment group, defined as income relative to threshold | | | | | | | |
| | 1% | 5% | 10% | > 10% | > threshold | 5% below | 90% - 95% |
| Effect | 89*** (20) | 264*** (79) | 364** (174) | 262 (1,489) | 712 (1,754) | 295*** (82) | 6 (83) |
| Observations (bins) | 1,008 | 266 | 126 | 28 | 28 | 252 | 238 |
| Included individuals | 80,776 | 85,745 | 85,712 | 133,938 | 155,901 | 83,565 | 83,579 |
| Adjusted R ² | 0.19 | 0.26 | 0.25 | 0.17 | 0.19 | 0.28 | 0.18 |
| Pre-reform yearly average (individ.) | 49 | 253 | 505 | 3,689 | 4 194 | 264 | 296 |
| % effect, relative to pre-reform avg. | 181% | 104% | 72% | 7% | 17% | 112% | 2% |
| Accumulated effect (individ.) | 646 | 2,047 | 2,834 | 125 | 3,583 | 2,265 | 57 |

Note: The estimates refer to the partial-year sample (extrapolated income for migrants that arrive during a year). The first row shows the coefficient for the interaction term from the DiD specification (β in Equation 2). This can be interpreted as the average additional number of migrants in the post-reform period. Standard errors in parenthesis. "Observations (bins)" captures the amount of bins, containing both the affected income range and control bins. "Included individuals" refers to the total number of individuals in both affected and control group in all years of the sample (2006-2019). "Pre-reform yearly average" refers to the affected group. The control group contains (subgroups of) individuals with income the range 50 – 90% of the threshold. "Accumulated effect" is calculated as the sum of the coefficients for the interaction effects from the Event study specification. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.3: DiD estimates for "non-changers" sample

| <i>Dependent variable: Number of arriving migrants</i> | | | | | | | |
|----------------------------------------------------------|-------------|-------------|--------------|---------------|---------------|----------|-----------|
| Treatment group, defined as income relative to threshold | | | | | | | |
| | 1% | 5% | 10% | > 10% | > threshold | 5% below | 90% - 95% |
| Effect | 176*** (14) | 328*** (58) | 448*** (135) | 1,081 (1,010) | 1,743 (1,179) | 32 (44) | -64 (52) |
| Observations (bins) | 826 | 182 | 84 | 28 | 28 | 210 | 182 |
| Included individuals | 37,345 | 38,943 | 40,495 | 95,374 | 105,621 | 37,484 | 37,853 |
| Adjusted R ² | 0.40 | 0.44 | 0.42 | 0.44 | 0.52 | 0.23 | 0.18 |
| Pre-reform yearly average (individ.) | 35 | 171 | 353 | 301 | 3,554 | 167 | 174 |
| % effect, relative to pre-reform avg. | 503% | 192% | 127% | 36% | 52% | 19% | -37% |
| Accumulated effect (individ.) | 646 | 2,047 | 2,834 | 125 | 3,583 | 2,265 | 57 |

Note: The estimates refer to the 'full-time' sample (include only migrants that stay work at one employer during the entire first year after migration). The first row shows the coefficient for the interaction term from the DiD specification (β in Equation 2). This can be interpreted as the average additional number of migrants in the post-reform period. Standard errors in parenthesis. 'Observations (bins)' captures the amount of bins, containing both the affected income range and control bins. 'Included individuals' refers to the total number of individuals in both affected and control group in all years of the sample (2006 - 2019). 'Pre-reform yearly average' refers to the affected group. The control group contains (subgroups of) individuals with income the range 50 – 90% of the threshold. 'Accumulated effect' is calculated as the sum of the coefficients for the interaction effects from the Event study specification. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

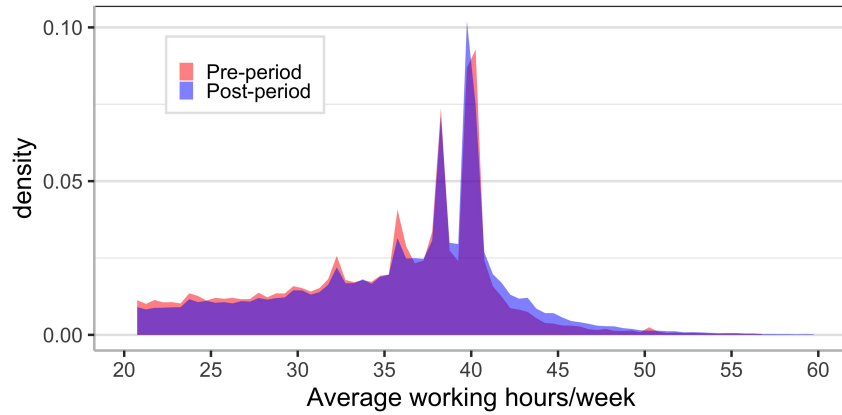
Table A.4: DiD estimates for baseline sample excl. Germans

| | <i>Dependent variable: Number of arriving migrants</i> | | | | | | |
|---------------------------------------|----------------------------------------------------------|-------------|--------------|-------------|---------------|----------|-----------|
| | Treatment group, defined as income relative to threshold | | | | | | |
| | 1% | 5% | 10% | > 10% | > threshold | 5% below | 90% - 95% |
| Effect | 187*** (14) | 377*** (64) | 554*** (142) | 466 (1,487) | 1,131 (1,734) | 25 (64) | -90 (75) |
| Observations (bins) | 1,456 | 308 | 154 | 28 | 28 | 280 | 238 |
| Included individuals | 77,930 | 82,020 | 88,052 | 136,072 | 155,689 | 77,358 | 78,280 |
| Adjusted R ² | 0.33 | 0.37 | 0.36 | 0.21 | 0.24 | 0.20 | 0.19 |
| Pre-reform yearly average (individ.) | 45 | 231 | 470 | 3,549 | 4,019 | 248 | 270 |
| % effect, relative to pre-reform avg. | 416% | 163% | 118% | 13% | 28% | 10% | -33% |
| Accumulated effect (individ.) | 1,482 | 3,045 | 4,464 | 1,908 | 7 388 | 252 | -753 |

Note: The estimates refer to the baseline sample excluding migrants with previous residence in Germany. The first row shows the coefficient for the interaction term from the DiD specification (β in Equation 2). This can be interpreted as the average additional number of migrants in the post-reform period. Standard errors in parenthesis. ‘Observations (bins)’ captures the amount of bins, containing both the affected income range and control bins. ‘Included individuals’ refers to the total number of individuals in both affected and control group in all years of the sample (2006-2019). ‘Pre-reform yearly average’ refers to the affected group. The control group contains (subgroups of) individuals with income the range 50 – 90% of the threshold. ‘Accumulated effect’ is calculated as the sum of the coefficients for the interaction effects from the Event study specification.

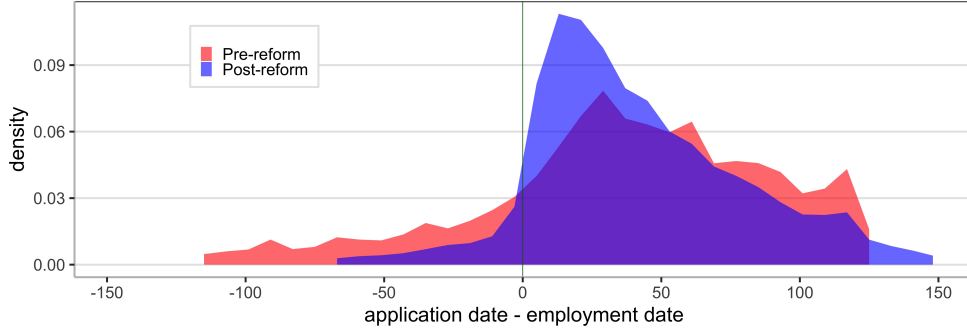
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Figure A.1: Distribution of average weekly working hours, non-beneficiaries



Note: The chart shows the densities of average weekly working hours for non-beneficiaries in the pre-reform (2006-2011) and post-reform (2012-2019) periods. Weekly working hours are averaged by person and year, for the weeks in which an individual was employed.

Figure A.2: Distribution of time between application and start of employment



Note: The charts show the distribution of the difference between application date and start of employment. Positive values mean that the application was filed after starting employment (immigration). Negative values mean that the application was filed before the beginning of employment (immigration). The figure refers to the sub-sample of 30%-rule beneficiaries.

B Wage bargaining model

Using the framework of a static wage bargaining model with matching frictions, we show how the income threshold may induce a differential wage bargaining outcome. We follow Kleven et al. (2014) in incorporating the tax-rule eligibility threshold into a standard Diamond-Mortensen-Pissarides wage bargaining framework (Mortensen and Pissarides, 1994). For simplification we take the meeting process between employer and employee as given. Wage bargaining occurs if the employee's marginal product at the employer, y , is equal or higher than the migrant's reservations wage, y_0 , which captures the minimum compensation for which the migrant is willing to move to and work in the Netherlands. In the event of an employer-employee match where $y \geq y_0$, any wage $z \in [y_0, y]$ will be acceptable to both parties.

The reservation wage is the wage that equalizes net income in the Netherlands and net income in the home country plus migration costs. With τ and τ_H denoting the average tax rate in the Netherlands and in the migrant's home country respectively, z_H the wage in the home country and c the migration costs, the (pre-tax) reservation wage y_0 is such that $(1 - \tau) \cdot y_0 = (1 - \tau_H)z_H + c$, or

$$y_0 = \frac{(1 - \tau_H)z_H + c}{1 - \tau}$$

Given a range of acceptable wages, the wage z is determined through a bargaining process. A well-established solution is Nash bargaining, which splits employer and employee surplus based on an exogenous parameter for employee bargaining power $0 \leq \beta \leq 1$ (conversely, $1 - \beta$ captures employer bargaining power). The Nash bargaining solution

maximises

$$W = \underbrace{(y - z)^{1-\beta}}_{\text{firm surplus}} \underbrace{((1 - \tau)(z - y_0))^\beta}_{\text{worker surplus}}$$

In the absence of a discontinuity (i.e. with a constant tax rate τ), the maximisation problem yields a wage of

$$z = \beta y + (1 - \beta)y_0$$

Now the income threshold introduces a kink in the tax rate: below the threshold, the regular Dutch income tax rate applies (denoted as τ). For income above the threshold, a reduced income tax rate applies ($\tilde{\tau} < \tau$) for beneficiaries. As shown by Kleven et al. (2014), this has two consequences: (1) the reservation wage for beneficiaries (\tilde{y}_0) decreases and (2) the bargained wage for beneficiaries (\tilde{z}) shrinks correspondingly, as long as firms have some bargaining power ($\beta < 1$).

$$\begin{aligned}\tilde{y}_0 &= \frac{(1 - \tau_H)z_H + c}{1 - \tilde{\tau}} < \frac{(1 - \tau_H)z_H + c}{1 - \tau} = y_0 \\ \tilde{z} &= \beta y + (1 - \beta)\tilde{y}_0 < \beta y + (1 - \beta)y_0 = z\end{aligned}$$

Intuitively, as benefiting workers receive more after-tax income due to the lower average tax rate, they are willing to accept a slightly lower before-tax income y_0 to move to the Netherlands and take up work. This reduction of the reservation wage increases the range of acceptable wages (by decreasing the lower bound), and hence results in a lower bargained wage as long as firms have some bargaining power.

In the presence of a kink in after-tax income, the wage bargaining model accommodates both bunching from below (employees with income below the threshold bargain up the wage and meet the threshold), and bunching from above (employees with income above the threshold are bargained down) for beneficiaries. Firstly, consider bunching from below: employees with initial income closely below the threshold may be able to bargain up their wage to meet the threshold. This results in disproportionate increase in employee's surplus for a small price for the employer and hence may be consistent with a solution in the bargaining problem. In the income distribution this would be reflected in a decrease in mass in a range closely below the threshold and an increase in mass at the threshold. This type of bunching would be stronger, the higher the employee bargaining power is.

Secondly, consider bunching from above. As stipulated earlier, the reservation wage decreases for beneficiaries. The lower reservation wage results in a decrease of the bargained wage, with the threshold being a lower bound (a wage below the threshold would come with

a disproportionately large loss in worker surplus). This type of bunching is more prevalent when firms have higher bargaining power. It is reflected in the income distribution with a left-shift of the distribution above the threshold and excess mass at the threshold.

C Calculating migration elasticity

Definition The migration elasticity captures the change in migration (flow) relative to the change in the net-of-tax rate: $\frac{\% \text{migration (inflow)}}{\% \text{net-of-tax-rate}}$, where the net-of-tax rate is defined as $1 - \text{average tax rate}$ or $\frac{\text{net income}}{\text{gross income}}$. The net income for non-beneficiaries (beneficiaries) is defined as gross income minus tax payments minus social security contribution. The difference between beneficiaries and non-beneficiaries is that for non-beneficiaries, gross income is equal to taxable income. For beneficiaries, gross income includes both taxable income and the tax-free benefit (up to 30% of taxable income). Hence for beneficiaries, the net-of-tax rate is higher as the average tax rate is lower due to an untaxed component in the gross income.

Data We obtain data on taxes and benefits in the Netherlands from the OECD Tax-benefit web calculator²³ for a range of income levels and years. We calculate the personal income tax schedule for a single individual of 38 years without kids.²⁴

The information on the income distribution of migrants comes from the CBS data for the sample described in the main part of the paper.

Calculation steps For beneficiaries and non-beneficiaries, we separately calculate the average net-of-tax rate per year, weighing by the fraction of migrants in income ranges j :

$$\overline{\text{net rate}} = \sum_{j=1}^J \frac{N_j}{N} \text{net rate}_j$$

for income bins j and the number of migrants in a bin, N_j relative to the total number of migrants, N .

Next, we weigh the average net-of-tax rates for beneficiaries and non-beneficiaries by the probability of receiving the benefits to calculate an expected value of the net-of-tax rate. The (expected) probability of receiving the benefits matters to define the change in the average tax rate: For example, if migrants expect to be beneficiaries in the post-reform period, but assign a low (or 0) probability to being eligible prior to the reform, the main

²³<https://www.oecd.org/social/benefits-and-wages/tax-benefit-web-calculator/>

²⁴Hence abstracting from joint taxation of spouses and tax benefits for families.

change in the net-of-tax rate is manifested in the difference between net-of-tax rates for beneficiaries and non-beneficiaries. Table C.1 gives an overview of how the (subjective) probability to receive the benefits affects the elasticity estimate.

Table C.1: Probabilities for benefiting and resulting migration elasticity

| $\mathbb{P}(\text{pre})$ | $\mathbb{P}(\text{post})$ | % change net-of-tax rate pre-post | elasticity |
|--------------------------|---------------------------|-----------------------------------|------------|
| 0 | 1 | 13.8 | 2.07 |
| 0.1 | 1 | 12.4 | 2.3 |
| 0.2 | 1 | 11 | 2.59 |
| 0.3 | 1 | 9.7 | 2.94 |
| 0.4 | 1 | 8.4 | 3.39 |
| 0.5 | 1 | 7.2 | 3.98 |
| 0.534 | 0.682 | 2.7 | 10.5 |

Note: The table shows the resulting migration elasticity when varying the weights used as an input for the "expected" net of tax rate. The average tax rates refer to the population weighted measure. The calculation uses the following net-of-tax rates (τ), $\tau_{pre}^{beneficiaries} = 69.1$, $\tau_{post}^{beneficiaries} = 69.9$, $\tau_{pre}^{non-beneficiaries} = 61.5$, $\tau_{post}^{non-beneficiaries} = 61.6$ and an increase in migration above the threshold of 28.5%. The top panel of the table shows how the change of the net-of-tax rate (the denominator of the migration elasticity) varies with the assumption on migrant's expectation of getting the tax benefits in the pre-reform period. The bottom panel of the table shows the percentage change in the net-of-tax rate and the corresponding migration elasticity when using the average fraction of beneficiaries as a proxy for the (expected) probability of receiving the tax benefits.

D Government Budget calculation

Assumptions The goal of this exercise is to calculate the implications of additional migration and the difference in eligibility criteria on government tax revenue. The income threshold affects government tax revenue through two main channels: Firstly, as the eligibility criteria are more clear, this may attract more migrants (beneficiaries). Secondly, more migrants can apply for (and also eventually benefit from) the tax benefits, even those that would have come absent the reform.

We make a few simplifying assumptions. Firstly, we assume that migrants do not crowd out domestic workers (or in other words, migrant's employment is created independently of domestic employment). Secondly, related to the first point, we assume away equilibrium wage effects between migrants or between domestic and migrant workers which could arise through competition and/or (positive) productivity spillovers. Thirdly, we calculate tax

rates for a single individual without children²⁵ and we only take into account the personal income tax. Lastly, we calculate the change in tax revenue for a given year, without taking into account migrants' duration of stay in the Netherlands and hence abstracting also from considerations related to costs of health care and pensions.

We calculate the additional inflow of migrants per income bin (defined in 5% brackets) relative to the threshold. For each of these income bins, we calculate the additional inflow using the DiD approach: Compared with the trend in the control group (50% to 90% of the income threshold), how many (more/less) migrants arrive in a given year in a given income bin? With this approach, we define marginal migrants (the number of migrants that arrives in proportion to the trend in the control group) and infra-marginal migrants (the number of additional migrants) in a given income bin.

We define the foregone tax revenue as the reduced tax revenue from marginal migrants that qualify for the tax benefits after the reform. Next, we define the additional tax revenue as the entire tax revenue from infra-marginal (additional) migrants, even though their taxable income may be reduced in accordance with the preferential tax scheme.

Lastly, we sum both foregone tax revenue and additional tax revenue over the income bins and calculate the difference between additional tax revenue and foregone tax revenue.

E Income thresholds & calculation examples

Calculation examples from the Dutch Tax Office (2020):

Example 1 The wages inclusive of the reimbursement are 60,000 €. The tax free reimbursement of the extra-territorial costs is a maximum of $30\% \times 60,000 \text{ €} = 18,000 \text{ €}$.

Example 2 The wages inclusive of the reimbursement are 40,000 €. As the wages exclusive of the reimbursement must be at least 38,347 €, you can issue the employee a reimbursement of a maximum of $40,000 \text{ €} - 38,347 \text{ €} = 1,653 \text{ €}$.

Example 3 The wages exclusive of the reimbursement are 40,000 €. The tax free reimbursement of the extra-territorial costs is a maximum of $30/70 \times 40,000 \text{ €} = 17,143 \text{ €}$.

²⁵Hence abstracting from joint taxation for spouses and tax benefits for families

Table E.1: Income thresholds for eligibility to
the 30% rule

| | general | < 30 years & Master's degree | academia |
|------|----------|---------------------------------|---------------|
| 2012 | 35,000 € | 26,605 € | <i>always</i> |
| 2013 | 35,770 € | 27,190 € | <i>always</i> |
| 2014 | 36,378 € | 27,653 € | <i>always</i> |
| 2015 | 36,705 € | 27,901 € | <i>always</i> |
| 2016 | 36,889 € | 28,041 € | <i>always</i> |
| 2017 | 37,000 € | 28,125 € | <i>always</i> |
| 2018 | 37,296 € | 28,350 € | <i>always</i> |
| 2019 | 37,743 € | 28,690 € | <i>always</i> |
| 2020 | 38,347 € | 29,149 € | <i>always</i> |

Source: Dutch Tax Office (*Belastingdienst*).