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Tax Incentives for Migrants with Mid-level Earnings: Evidence from the Netherlands†

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We examine how income taxes affect international mobility and wages. We study a Dutch preferential tax scheme for migrants, which introduced an income threshold for eligibility in 2012. The threshold is low relative to similar schemes in other countries, thereby offering eligibility to migrants with mid-level earnings. We find migration more than doubles closely above the income threshold, while migration below the threshold remains unchanged. These effects appear to be driven by additional migration, while wage bargaining responses are limited. We estimate a migration elasticity ranging from 1.6 to 2.7, somewhat higher than most studies on high-income migrants have found. (*JEL* H24, H31, J15, J31, J82)

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. A growing literature acknowledges the need for quantifying migrants' responsiveness, and has examined how tax differentials induce mobility either within or across countries (Kleven et al. 2020). Such estimates are essential for designing optimal taxation, especially in the light of concerns about tax competition between countries (Flamant, Godar, and Richard 2021).

The existing literature has been constrained by data limitations and lack of credible exogenous variations in tax rates. As a result the majority of the current empirical evidence considers within-country mobility responses, while only a handful of studies is able to track international migration responses. More importantly, the

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vast majority of the elasticity estimates considers solely the very top of the income distribution. For example, various studies consider tax-induced mobility across US states, but only for star scientists (Moretti and Wilson 2017), millionaires (Young et al. 2016), "top earners" (Rauh and Shyu 2024), or professional golf players (Agrawal and Tester 2024). Other within-country evidence comes from Switzerland (Schmidheiny and Slotwinski 2018) and Spain (Agrawal and Foremny 2019) and also focuses mainly on high-income earners. Studies on international migration have a similarly narrow focus: migrants with earnings in the top 1 percent of Denmark (Kleven et al. 2014), European football players (Kleven, Landais, and Saez 2013), or top 1 percent inventors (Akcigit, Baslandze, and Stantcheva 2016).

There are legitimate reasons to focus on high earners when considering mobility. High income implies larger potential tax revenue in the host country and typically coincides with higher skills and productivity, which are common policy goals of preferential tax schemes.[1] In addition, the willingness to relocate is usually expected to be larger for high-income individuals, as they have more to gain from moving to lower tax regions or countries.[2] But as global migration is continuously increasing (OECD 2022) and labor scarcity is growing in many developed countries (Causa et al. 2022), the question arises how responsive potential migrants with lower incomes are to differentials in income taxes. One reason why the responsiveness of mid-level income migration might be substantial is that very few countries offer tax incentives for this particular group of migrants. As a result, policies face "less competition" in attracting mid-income migrants.

In this paper we provide one of the first estimates of the migration response to a large tax exemption that targets not only high-income workers, but also those with mid-level incomes. We consider a tax exemption in the Netherlands that applies to all migrants whose earnings exceed €35,000 in 2012 (which is approximately the sixty-fifth percentile of the Dutch income distribution). Compared to similar policies in other countries (e.g., Kleven et al. 2014), this requirement is not very stringent, and we show that almost one-fourth of all working migrants within our observation period qualifies. We identify the tax exemption's impact on migration from the reform that implemented the income threshold. The threshold 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 labor market outcomes in the Netherlands, we show that a large increase occurs in the number of arriving migrants with income just above the threshold (relative to migrants with income substantially below the threshold). While a minor part of this shift in the income distribution appears to result from firms adjusting their wage offers in response to the threshold, the majority can be attributed to additional migration.

[1] Preferential tax schemes are one of various policies in place to attract high-income or high-skilled migrants, including, but not limited to, preferential visas for graduates (e.g., United States and the Netherlands) or point systems (e.g., Canada).

[2] Kleven et al. (2020) describe another reason for the literature's focus on top earners. Mobility decisions depend on the average tax rate rather than the marginal tax rate. Empirically, it can be complex to measure average tax rates, but for migrants with very high income, the top marginal tax rate provides a reasonable approximation of the average tax rate. We return to this point in Section I.

In the Netherlands, migrants can apply for a reduction of the income tax burden. This preferential tax scheme is supposed to attract individuals with skills that are scarce in the Dutch labor market. The scheme grants an eligible employee up to 30 percent of their income as a tax-free payment for a specified duration (the scheme is often referred to as *the 30 percent rule*). For the employee, this rule implies a substantial increase in after-tax income, while the employer also pays reduced employer social security contributions.[3]

While the scheme has existed for decades, a major reform took place in 2012, introducing an income threshold for eligibility. The threshold creates a kink in the marginal tax rate: Below the threshold the Dutch marginal tax rate applies, while every additional euro earned above the threshold is tax-free (up to 30 percent of total earnings). But the benefits of passing the threshold upon arrival are even larger because future eligibility depends on the salary in the first year (migrants benefit for multiple years, but only if they qualify in the year of arrival). In addition, qualifying for the rule entails a wealth tax exemption. As a result, one can argue that a discontinuous jump in earnings occurs at the threshold: the kink is in fact a notch.[4] In this paper, we discuss how the income threshold may (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. Our control group consists of income bins substantially below the threshold, which we show are unlikely to be affected by the reform. 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 percent, 5 percent, and 10 percent above the threshold imply a 376 percent, 148 percent, and 108 percent increase (respectively), relative to the pre-reform averages. Also the number of arriving migrants with salaries more than 10 percent above the threshold grows after the reform, although this change is not statistically significant. These increases seem to be stronger for migrants from outside the EU than for those from inside the EU (with the caveat that the estimates for within-EU migrants may appear smaller because of increased low-income EU migration, which constitutes the control group). One potential explanation could be that non-EU migrants face generally higher migration costs in terms of obtaining a visa. The increased transparency of the tax benefits may have spilled over to improve awareness and knowledge about

[3] The Dutch 30 percent rule has been linked to stories of extremely wealthy migrants moving to the Netherlands "on paper" to benefit from reduced taxation. These cases are not the focus of this paper, as we study mobility of workers with annual income around the threshold of €35,000. For this population, extreme amounts of wealth and "on-paper" migration are highly unlikely.

[4] As reviewed in Kleven (2016), a kink is defined as a change in the slope of choice sets (e.g., a change in marginal tax rates resulting in a different slope of gross income to net income), while a notch is defined as a discontinuity in the level of the choice set, where one side of the notch is strictly preferred to the other (e.g., eligibility for a lump sum transfer with an income threshold).

the visa applications procedure. Finally, we also show that the largest increase in migrants appeared among those working in the business services sector.

At the same time, we find no evidence of a reduction in arriving migrants with income below the threshold. The absence 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 that offer wages substantially above the threshold may lower their before-tax salary offers as they realize that their prospective employees benefit from the reduced taxes. As a result, such firms reap some of the benefits that accrue from the tax exemption. 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 with middle incomes are very responsive to tax incentives. Our estimates aggregate to a total of almost 11,000 additional migrants that arrive over the course of the years 2012–2019 due to the reform (see Table 3). When considering migrants with medium-range income, this yields an elasticity of migration with respect to the net-of-tax rate ranging between 1.6 and 2.7 (stemming from an additional inflow of 22.5 percent in the medium income range, and a change in the net-of-tax rate between 8 percent and 15 percent).[5] This is somewhat larger than most estimates from studies focusing on higher-income migrants (see Section I for details). In line with this large elasticity, a simple back-of-the-envelope computation shows that the reduced tax rate increased tax revenues.

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 show 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 made the rule more easily accessible for a wider range of firms.

The paper proceeds as follows. We briefly review the related literature in Section I. Section II presents the Dutch preferential tax scheme and the 2012 reform. In this section we also discuss the expected effects in terms of migration, wage bargaining, and labor supply. Section III describes the data used and provides descriptive statistics. Section IV outlines the empirical strategy, motivates the choice of treatment and control groups, and presents the main results, including a

[5] Medium-range income is defined as 100–150 percent relative to the income threshold (see Section IVA for a discussion of affected groups. Table 7 summarizes the calculation for the migration elasticity.)

number of robustness checks. Section V compares the main findings against potential mechanisms. Section VI concludes.

I. Related Literature

Estimating the migration response to taxes is essential for optimal taxation (e.g., 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). The literature on optimal taxation has also pointed towards the risk of tax competition between countries, a concern that appears increasingly relevant within the EU (Flamant, Godar, and Richard 2021).

The primary input for optimal tax design is the degree to which mobility is governed by tax differentials. Empirical evaluation of the labor mobility response to income differentials is limited due to data availability and lack of credible variation. Another challenge for the evaluation of the latter is that mobility decisions depend on the average rather than the marginal tax rate, while the latter is much easier to measure and also exhibits more variation over time that allows identification of mobility responses. For (very) high-income earners the marginal tax rate is a reasonable proxy for the average tax rate, which may be one explanation for the literature mainly focusing on mobility of top earners. For the United States, state differences in top marginal tax rates have been used to identify mobility responses of millionaires (Young et al. 2016), star scientists (Moretti and Wilson 2017), professional golf players (Agrawal and Tester 2024), or top earners in California (Rauh and Shyu 2024; and Varner, Young, and Prohofsky 2018). A similar approach has been applied to measure regional mobility of top 1 percent earners within Spain (Agrawal and Foremny 2019) and location choices of top-earning foreigners within Switzerland (Schmidheiny and Slotwinski 2018; Martínez 2022). Liebig, Puhani, and Sousa-Poza (2007) is an exception that considers a more diverse population to study mobility across Swiss municipalities.

These within-country studies consistently documented substantial responses in location choice to changes in the top marginal tax rates (with the exception of Varner, Young, and Prohofsky 2018).[6] Our study contributes by considering mid-income migrants. We are able to overcome the above mentioned challenges by exploiting a reform that generates a substantial reduction in the *average tax rate* for mid-income migrants, combined with detailed administrative data that allows precise measurement of labor earnings and tax liabilities.

A handful of studies consider international migration and exploit variation in tax rates across countries and time. Between-country studies have mainly focused on certain high profile professions; for example, top 1 percent inventors (Akcigit, Baslandze, and Stantcheva 2016) or football players (Kleven, Landais, and Saez 2013) for

[6] A related literature considers how taxes affect locational choices at a more local level such as within cities or metropolitan areas. Several studies provide evidence of substantial sorting of workers within cities with respect to income taxes (Agrawal and Hoyt 2018; Schaltegger, Somogyi, and Sturm 2011) or property taxes (Song and Zenou 2009) and how these mobility responses affect urban development and commuting times, as well as local rents (Basten, von Ehrlich, and Lassmann 2017).

whom cross-country movements are relatively easy to track. Muñoz (2021) estimates mobility responses across EU countries for a more diverse population, yet restricted to top-decile-income earners. The study most closely related to ours in terms of methodology is Kleven et al. (2014), who examine the Danish preferential tax scheme for migrants whose income falls in the top 1 percent of the Danish income distribution. Exploiting the introduction of the scheme, the authors find that top foreign earners are highly mobile with a corresponding migration elasticity of 1.6.

These papers have shown that location choices of foreign high earners are generally responsive to top tax rates, although domestic workers are substantially less responsive. Our paper examines a tax scheme that affects migrants in a broader range of the income distribution. The income threshold for eligibility is approximately at the sixty-fifth percentile of the wage distribution of all employees in the Netherlands, and around one fourth of arriving migrants fulfill this requirement. As such, our results contribute to the question whether the estimated elasticities generalize to a more diverse population in terms of occupations and income levels. One of the few studies that also considers lower-income migrants is Bassetto and Ippedico (2023), who consider a tax reduction for Italian expatriates who relocate to Italy. Their findings are consistent with ours: They show that the policy strongly increased return migration, with stronger responses for those with below-median wages.

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) 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). We complement these studies by showing that the elasticity for migrants with lower earnings is at least as large and potentially even larger.

Differences in tax regimes are only one source of income variation, and our paper also relates to the broader literature on determinants of migration choices. Borjas (2001) emphasizes that migration flows may be shaped by differentials in economic conditions, thereby "greasing the wheels of the labor market." Moretti (2011) shows how an equilibrium framework can be used to study the effect of various types of regional labor market shocks on spatial sorting. Kennan and Walker (2011) estimate a dynamic choice model of migration within the United States and find that the migration elasticity with respect to a simulated wage increase is 0.5.[7] Our paper

[7] Other empirical contributions also find significant relationships between labor demand shocks and migration (e.g., Monte, Redding, and Rossi-Hansberg 2018; Greenaway-McGrevy and Hood 2016; Niebuhr et al. 2012; and Carrington 1996). Other studies have documented substantial heterogeneity in responsiveness to local labor market conditions, with Cadena and Kovak (2016) and Cadena (2013) finding much larger elasticities for low-skilled Mexican-born immigrants in the United States (as compared to natives), and Jaeger (2007) showing that male employment-based immigrants' location choices are particularly sensitive to economic conditions.

considers a policy variation in income taxes that isolates a pure income effect rather than evaluating the impact of general economic conditions on migration flows.

Finally, our study also relates to research on the relationship between migration and other types of taxation. Particularly prominent is a growing literature on wealth taxation and mobility, mainly motivated by recent debates about wealth inequality and the need for wealth taxes (Saez and Zucman 2022). The focus has been mainly on tax evasion through capital mobility (see for example Leenders et al. 2023), although some studies have estimated physical relocation responses to wealth taxes for the United States (Bakija and Slemrod 2004), Spain (Agrawal, Foremny, and Martínez-Toledano 2020), the United Kingdom (Advani, Burgherr, and Summers 2023), and Switzerland (Brülhart et al. 2022). The preferential tax scheme that we consider also includes a wealth tax exemption (see Section II for details), although given our population of interest we provide evidence that it is unlikely to be the driving force behind our results.

II. Preferential Tax Scheme for Foreigners in the Netherlands

Variations of a preferential taxation scheme for skilled foreigners in the Netherlands have been in place since the 1950s. In 2001, the scheme was formalized and the proportion of tax-free income was set to 30 percent (previously 35 percent; 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).[8]

Before 2012.—Before 2012, eligible migrants could benefit from the tax exemption for a period of 10 years. The advantages of the scheme for the employees are twofold. Firstly, the scheme allows the employee to receive up to 30 percent of their gross income as a tax-free payment. Given that the top marginal tax rate in the Netherlands has been around 50 percent, this generates an increase in after-tax income of around 15 percent of gross income to the employee (we provide more details below). Note that this includes reduced social security contributions. Secondly, beneficiaries have the option for an exemption from the wealth tax. In particular, they can opt for so-called "partial foreign tax liability" which exempts their wealth (assets, savings, and investments) from Dutch taxation.[9] It is assessed that almost all 30 percent rule beneficiaries make use of the option (Kamerstuk 2011a). The relative importance of the wealth tax exemption depends crucially on the amount of wealth the employee owns. Also the employer benefits from hiring an employee under the 30 percent rule, as they pay no employer social security contributions for

[8] Beneficiaries of this scheme do not need to deliver a proof of such expenses.

[9] The Dutch tax system levies taxes separately for labor income ("Box 1"), income from substantial interests in companies, meaning owning more than 5 percent of a company's shares ("Box 2") and income from other personal assets such as savings and investments ("Box 3"). Partial foreign tax liability exempts the employee from Dutch taxation of Box 2 and Box 3.

the tax-free 30 percent of the salary.[10] We provide a quantitative assessment of these elements below.

Until 2012, eligibility for the tax exemption required an employee to (i) possess some specific expertise that is scarce in the Dutch labor market, and (ii) be recruited from abroad with the purpose of entering an employment relationship with a firm that is withholding payroll taxes in the Netherlands.[11] Employee and employer needed to apply jointly within four months of commencing the employment relationship. The first 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.[12] As a result, the employer was required to make a case for the scarcity of the skills of their prospective employee, and the outcome of the application entailed substantial uncertainty. The employee needs to reapply upon switching employer, and 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 permanently.

The criteria resulted in a strong correlation between eligibility and the employee's income. For example for migrants arriving in 2010, eligibility for those with annual earnings below €35,000 was virtually absent. For arriving migrants with income between €35,000 and €45,000, approximately 40 percent benefited, while the share increased smoothly up to approximately 57 percent for migrants with an income exceeding €50,000.

After 2012.—The eligibility criteria of the 30 percent scheme were drastically changed in 2012. The reform also entailed minor changes to other aspects of the rule, to which we return below. Importantly, the new rules apply to all migrants that arrive and apply from 2012 onwards, and do not affect migrants that received eligibility prior to 2012.

The 2012 reform replaced the "scarce skills criterion" with an income threshold: Employees with an annual *taxable income* exceeding the threshold are granted eligibility. The threshold was €35,000 in 2012 (indexed annually) and applied to all employees over 30 years, with the exception of academics who are eligible irrespective of their income. A lower threshold applies to employees below 30 with an MSc degree. The income threshold was meant to serve as a proxy for specific expertise and make the eligibility process simpler and more transparent (Kamerstuk 2011b, p. 21). The probability of receiving the benefits became more predictable for both the employer and employee.[13]

[10] This also implies a slightly lower pension contribution for employees. However, this is unlikely to be fully internalized especially by young and very mobile employees.

[11] 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.

[12] These 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, and finally there is a category "other."

[13] Indeed, in Section V we show that migrants started working quicker after immigration, and applied more promptly after starting a job. Both findings can be interpreted as supporting evidence that migrants knew about the rule before migrating to the Netherlands. Additionally, we find that more firms start using the rule for the first time after the reform, supporting the notion that the application process was simplified.

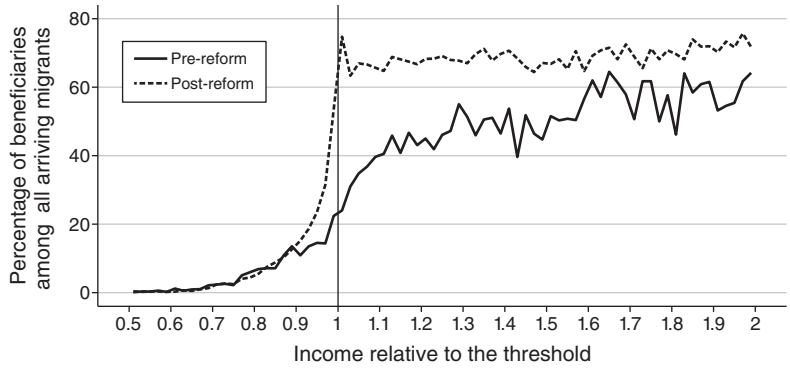

Figure 1. Fraction of Arriving Migrants That Benefited from the 30 Percent Rule

Notes: The chart shows the fraction of 30 percent rule beneficiaries among all arriving migrants (from our sample, see Table 1) before and after 2012. Income is measured in the first full calendar year after arrival, and normalized relative to the threshold (which was €35,000 in 2012). Pre-reform period: 2006–2011. Post-reform period: 2012–2019.

In addition, a second criterion for eligibility was introduced based on the location of prior residence: Migrants must have resided more than 150 km from the Dutch border prior to immigration. This means that after 2012, residents from Belgium, Luxembourg, and a small part of Germany were unable to benefit from the 30 percent rule benefits.[14] Moreover, the reform also reduced the maximum duration of the tax exemption from ten to eight years for newly arriving migrants. Given that only a relatively small share of 30 percent rule beneficiaries stays beyond eight years, this is unlikely to be of major importance.[15]

The introduction of the income threshold led to a substantial change in the share of arriving migrants that were granted eligibility. In Figure 1 we show the relationship between arriving migrants' income and the share that was eligible for the tax exemption, before and after 2012. The figure is based on migrants that arrived in the Netherlands between 2006 and 2019, excluding the exempted groups (migrants from Belgium and Luxembourg, migrants below 30 years old, and academics). Income has been normalized relative to the threshold.[16] The figure demonstrates the essence of how the reform changed eligibility. Prior to 2012, very few migrants with income below the later installed threshold benefited. The share increases with income: Just above the threshold 30 percent of migrants benefit, and at twice the threshold around 60 percent benefit. After 2012 the shares look very different. The share of beneficiaries below the threshold remained very low.[17] But the share of

[14] This new criterion also affected those arriving between 2006 and 2012 retrospectively, but only after having been in the Netherlands for five years. Since we consider new arrivals this is irrelevant for our analysis. See Giarola et al. (2023) for an analysis of the impact on out-migration of the retrospective requirement.

[15] In the latest cohort that we can follow for eight years (those arriving in 2011), 60 percent have left the Netherlands within eight years.

[16] For the years prior to 2012, we compute the hypothetical threshold using indexation relative to 2012. We provide more details on the data in Section III.

[17] One would expect exactly zero beneficiaries below the threshold after 2012, but some exceptions from the income threshold exist for employees on parental leave, foster care leave or adoption leave. 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.

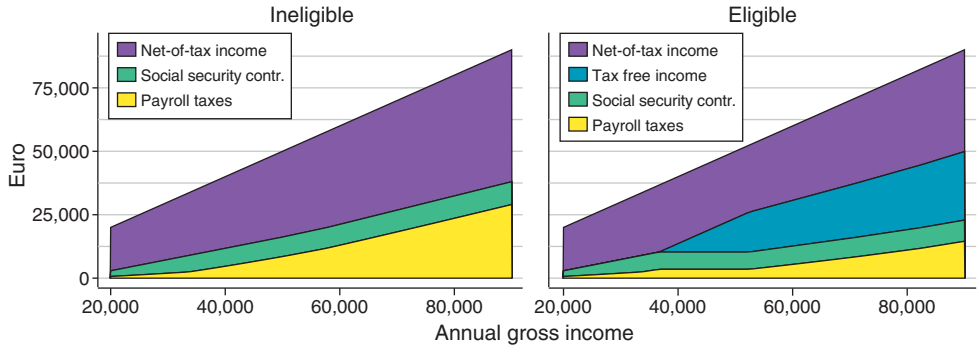

Figure 2. Employees' Income Taxation with and without 30 Percent Rule

Note: Employees' income taxation incorporates applicable tax credits (general tax credit and labor tax credit).

Source: The Tax (n.d.)

beneficiaries at the threshold spikes up to almost 80 percent, and remains around 70 percent for all migrants with income above the threshold. This provides the basis of our interpretation of the reform: It strongly increased the availability of the tax exemption for migrants with "midrange" income (income between 1 and 1.5 times the threshold), while changing little for migrants with lower or higher incomes.

The 30 Percent Rule Benefits: An Illustration.—We now turn to a precise characterization of the benefits of the 30 percent rule for employees and employers. Marginal tax rates vary slightly from year to year, and we use the 2015 tax scheme for this illustration (the 30 percent rule threshold was €36,889 in 2015). Figure 2 shows the various components of an employee's gross income for eligible and ineligible employees: the net-of-tax income (what remains of the taxable income after tax), tax-free income, payroll taxes and social security contributions. On the left we see that ineligible employees pay a progressive tax rate. Eligible employees (on the right) earn an effective income equal to the sum of the tax-free income (blue part) and the net-of-tax income (purple part), at the expense of a substantial reduction in payroll taxes (yellow part).[18] The tax-free income corresponds to 30 percent of the gross income, although it is smaller if gross income is closely above the threshold. In that case the tax-free amount is limited by the fact that the taxable income needs to exceed the threshold for the employee to remain eligible.[19] At a gross income of €50,000, the net benefit is €5,000, while at a gross income of €90,000, the net benefit increases to €15,000. In Supplemental Appendix A.1 we show similar figures demonstrating that also the employer saves on social security contributions, although these savings are considerably smaller than the gains to the employee.

[18] The social security contributions are unaffected by the tax-free allowance as they are only levied on the lowest income bracket. As a result, eligible workers do not face reduced entitlements for unemployment benefits, sickness benefits, or disability benefits. They may receive reduced pension contributions from their employer if these are proportional to the gross income.

[19] As an example, if the threshold equals €36,889 and an employee earns €40,000 gross, they can only receive €3,111 tax-free.

From Figure 2 it appears as if marginally passing the threshold has little benefits for the employee. There are, however, two reasons why this may not be the case. First, the rule requires migrants to qualify with their first job in the Netherlands. 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 if they qualify in the year of arrival. Indeed, we find in our sample of arriving migrants in the Netherlands that the median yearly wage growth is 4.75 percent. Second, there are the gains from the wealth tax exemption, which may be substantial for some migrants.

Figure 2 only depicts the benefits from the labor income tax exemption. The additional benefits from the wealth tax exemption are harder to quantify as they depend on an individual's wealth level, which are not captured in the administrative data because they are exempted from taxation. In any case, migrants need substantial amounts of wealth for the wealth tax exemption to provide benefits of the magnitude of the labor income tax exemption. Particularly for the group of migrants that was mainly affected by the reform (those earning between 1 and 1.5 of the threshold), this seems fairly uncommon.[20] By considering wealth of observationally equivalent natives, we indeed find limited gains for the wealth tax exemption.[21,22]

Expected Effects of the 2012 Reform.—The changes in the eligibility criteria in 2012 have implications for both migrants and employers, and may result in (i) a change 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 (intensive margin labor supply) of arriving migrants.

First of all, the reform is likely to affect the migration decision. Eligibility is now determined by the income threshold, instead of an assessment of the scarcity of a migrant's skills. As we showed in Figure 1, the probability of benefiting from the rule increased substantially after the reform for those with an income closely above the threshold (at the threshold it increased from 20 percent before 2012 to 80 percent after). 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

[20] As an illustration, consider a migrant arriving in 2015 owning wealth only in the form of financial savings (taxed in Box 3, see footnote 9). If savings do not exceed €21,139, they are exempt from taxes in any case. Any savings above this amount are taxed at 1.2 percent, such that a migrant with €100K would gain €946 from the wealth tax exemption.

[21] The administrative data contain information on wealth at the household level. We match beneficiaries to natives based on age, gender, household size, and working hours. We refer to financial wealth (excluding mortgages or housing wealth), as Box 3 taxes income from assets, savings, and investments. In Box 3 the return on assets worth more than €21.000 (per person in the household) is taxed.

[22] When comparing all beneficiaries with observationally similar natives, we find median wealth of around €21.000 for a two-person household. The seventieth percentile is around €45.000, just slightly above the tax-free threshold. When considering beneficiaries with an income closely above the threshold (100–110 percent), the median and eightieth percentile of wealth of observationally similar natives are around €14.500 and €28.000, respectively.

to observe an increase in mass in the range above the threshold in the income distribution of arriving migrants. For migrants with an income below the threshold, little changed, as they were very unlikely to benefit both before and after the reform. The same holds for migrants with income far above the threshold: Their probability of benefiting only increased marginally, as they were likely to benefit already before the reform. We return to this in more detail in Section IV, where we present our empirical strategy for identifying the changes in arriving migrants.

Secondly, the reform may affect the wage bargaining process between arriving migrants and their employers. The imposition of a threshold creates discontinuous benefits for migrants who attain a wage above the threshold. The same holds for employers, although to a lesser extend (see panel C in Figure 2). As discussed earlier, the discontinuous benefits do not arise from direct tax reductions (these only generate a "kink"), but rather from the expected gains in subsequent years and from the wealth tax exemption. In the Supplemental Appendix B 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 (and a similar reduction in employers' labor costs) 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. The predictions in terms of changes in migration and wage bargaining are visualized in Figure 3.

Finally, the income threshold and the resulting kink (notch) may induce a labor supply response in terms of the number of working hours for arriving migrants.

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 labor supply response as individuals reoptimize their labor supply in the reaction to the kink (notch). Assuming a positive uncompensated elasticity of labor 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. Such a response would add to the "bunching from below" in the income distribution, as we would observe reduced mass for incomes slightly below the threshold and higher mass at the threshold.

Jointly, these three responses predict (i) a reduced inflow closely below the threshold if workers have bargaining power and/or increase their working hours, (ii) an increased inflow at the threshold and closely above it, and (iii) an ambiguous change in inflow at incomes "further" above the threshold. In Section V we return

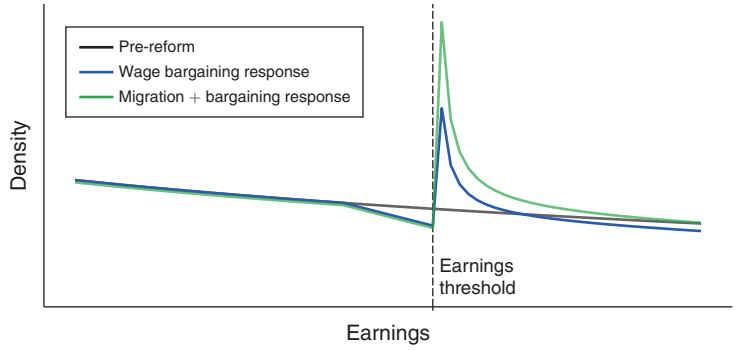

Figure 3. Wage Bargaining and Migration Responses to the Introduction of the Threshold

Notes: The chart visualizes expected changes in the income distribution following the reform of the 30 percent rule in 2012. In blue it illustrates wage bargaining responses that may result in "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 green line adds the migration response, producing an increase in mass in the range above the threshold.

to these predictions and compare them against the empirical results in an attempt to disentangle their relative importance.

III. Data

The empirical analysis is conducted with population-wide administrative data from Statistics Netherlands on migration movements, employment, income, and household characteristics for the years 2006–2019.[23] The information related to migration includes the date of immigration and emigration as well as the country of origin / destination. With respect to income and employment, the data contains start and end date per employment spell, taxable labor income, working hours and the sector of the employer. Demographic characteristics include age, gender, and country of birth. These data are combined with individuals' beneficiary status for the 30 percent rule (provided by the Dutch Ministry of Finance for the years 2009–2019), including the start and end date of the eligibility period.

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 (see first row of Table 1). Repeated spells by the same individual are included only if they are at least two years apart. We restrict the sample to migrants that start employment within six months after arriving in the Netherlands. This step is to ensure a sample of migrants that intend to work in the Netherlands, as opposed to family reunions, migrating partners, or refugees. Next, to ensure we observe a precise annual taxable income, we only include migrants that are employed for at least one full calendar year in the Netherlands.[24]

[23] See Centraal Bureau voor Statistiek (2018, 2021a, b, 2006).

[24] While this step excludes some migrants that may have been affected by the reform, their relevance for the policy is limited as they only stay in the Netherlands very briefly.

Table 1—Sample Construction

	All migrants		30% rule beneficiaries	
	Pre-refom (2006–2011)	Post-reform (2012–2019)	Pre-refom (2009–2011)	Post-reform (2012–2019)
Total inflow (all migrants)	603,237	1,086,680	21,194	104,281
+ Starts employment within 6 months	205,801	369,690	20,059	103,148
+ In NL for at least 1 calendar year	138,693	221,753	12,050	73,987
+ Older than 30 years	65,834	103,725	8,299	45,526
+ Not in academia	64,034	102,136	7,323	42,218
+ Not from Belgium	61,428	98,280	7,118	42,041
+ Not from Luxembourg	61,383	98,158	7,103	42,013
Resulting sample:	61,383	98,158	7,103	42,013

Notes: The years refer to the year of arrival. The numbers refer to migration spells, excluding repeated spells by individuals with less than two years in between. Total inflow refers to migrants above 18 years at the time of arrival. Information for beneficiaries is available as of 2009.

Additionally, three more groups are excluded since 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 they are never eligible due to the geographical criterion.[25] Secondly, migrant employees under the age of 30, as a different (lower) threshold applies to them if they hold a MSc degree.[26] Thirdly, migrants working in academia, as they are always eligible to receive the 30 percent rule benefits.[27] Table 1 shows the remaining sample after the sequential selection steps. Our resulting sample size of arriving migrants is 159,541, consisting of 61,383 migrants arriving in the years prior to the reform and 98,159 arriving after the reform. As a comparison, the size of the labor force was around 7.7 million in 2012 (Centraal Burau voor Statistiek 2022), while the stock of 30 percent rule beneficiaries was around 45,600 in 2012 (Flamant, Godar, and Richard 2021). After the sample selection described above, our sample contains roughly 50 percent of the inflow of beneficiaries in the period 2009–2019.

Figure 4 shows that the number of arriving migrants has steadily increased over recent years, both for 30 percent rule beneficiaries and nonbeneficiaries. From visual

[25] The distance criterion refers to the place where a person lived and worked for the majority of the preceding two years. As Table 1 (column 4) shows, there are some beneficiaries that come from Belgium after the reform. These exceptions can occur because some migrants (i) may have worked in a different country while still being registered in Belgium or Luxembourg, or (ii) were registered in Belgium, but only for a short amount of time and hence still comply with the "majority of the two years outside of a 150 km radius" criterion.

[26] As Table 1 shows, the share of migrants below 30 is fairly large. Given that they face a lower income threshold for eligibility, one could consider an analysis analogous to ours for this group specifically. We do not perform such an analysis for two reasons. First, the lower threshold only applies for migrants that hold a MSc degree, which we do not observe. As a result we cannot identify the affected population. Second, migrants aged below 30 may realize that the higher threshold will apply once they turn 30, which makes it unclear which threshold is the relevant one to consider for analyzing their inflow. We show the distribution of labor earnings for migrants below 28 in Figure C.1 in the Supplemental Appendix (equivalent to Figure 5) and find limited bunching at both thresholds.

[27] We do not observe whether an individual migrant is eligible based on the 'academic research / education' criterion. Instead, we exclude all migrants working in the sector "Public sector: Education and scientific research," which is a close proxy for being an academic. It contains employees employed in "educational institutes," "public hospitals," "public research institutes," and "educational support institutes." Throughout the paper we use the term "academia" when referring to this group.

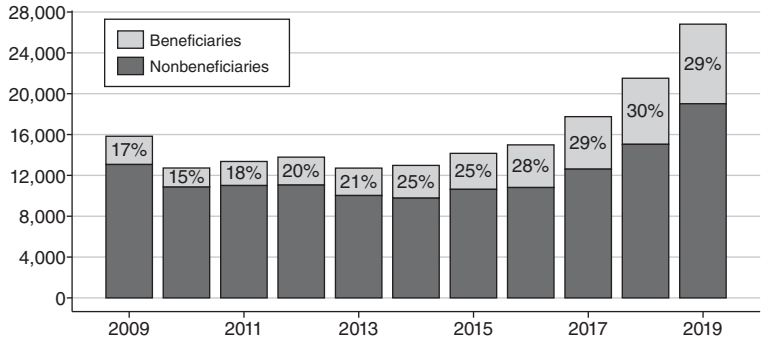

Figure 4. Annual Inflow of Migrants, by Usage of the 30 Percent Rule

Notes: 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. The percentage indicates the share of beneficiaries among all migrants for each year.

inspection, there is no pronounced change around 2012 in terms of 30 percent rule beneficiaries in the overall inflow. However, the figure shows the inflow across all income levels, while, as outlined in Section II, the income threshold's impact varies across different parts of the income distribution.

Descriptive Statistics.—Table 2 provides descriptive statistics. The average age for beneficiaries and nonbeneficiaries is around 38 in the year of arrival. Beneficiaries tend to be slightly younger than nonbeneficiaries (note that our sample excludes migrants below 30 years old). Around half of the migrants have resided in the EU prior to immigrating to the Netherlands, and this fraction has increased for nonbeneficiaries. The overall second largest group has prior residence in Asia and has a higher representation among beneficiaries than among nonbeneficiaries. In terms of the sectors in which beneficiaries work, "business services" is the largest (see Table G.2 in the Supplemental Appendix for the occupations included in this sector). Other major sectors for beneficiaries are "wholesale and retail trade" and "manufacturing," while "food services" attracts many nonbeneficiaries.

We now turn to the income distribution, where we consider labor earnings (taxable income) of arriving migrants in their first full calendar year.[28] The administrative data does not report the tax-free allowance and, unless mentioned otherwise, we use taxable labor earnings as the income measure throughout the rest of the paper. We show the distributions before and after the 2012 reform in Figure 5, where annual taxable labor income is normalized relative to the 30 percent rule threshold. We make two main observations. First, the income distribution in the pre-reform years is fairly smooth, with a mode around 50 percent to 75 percent of the income threshold. There is no discontinuity in the density around the income threshold that

[28] We use the first full calendar year to measure income, because extrapolating partial income in the year of arrival introduces substantial measurement errors. This arises from systematic variation in monthly salary payments across the year due to holiday allowance payments (mainly in May) and end-of-year bonuses (mainly in December).

Table 2—Descriptive Statistics: Individual Characteristics

	Beneficiaries		Nonbeneficiaries	
	2009–2011	2012–2019	2006–2011	2012–2019
Age (mean)	38.4	37.2	39.5	39.4
Gender (% female)	23.8%	25.3%	33.4%	38.9%
Region of origin:				
EU	51.5%	44.3%	46.1%	58.6%
Asia	22.1%	26.1%	9.4%	10.3%
America	13.2%	12.5%	10.2%	9.1%
Other	13.3%	17.1%	34.4%	22.0%
Sector of employment:				
Business services	45.1%	55.5%	12.1%	15.0%
Wholesale and retail trade	17.1%	14.7%	10.8%	10.7%
Manufacturing	16.4%	14.8%	8.8%	12.4%
Food services	0.6%	0.3%	9.9%	11.9%
Other sectors	20.8%	14.6%	58.4%	50.1%
Number of observations	7,103	42,013	61,383	98,158

Notes: Descriptive statistics for working migrants, based on an annual sample of incoming migrants who start working within six 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." Data on beneficiaries is available as of 2009. EU countries refer to the EU composition in a given year. The definition for "Business services" is based on the employer's activity classification by Statistics Netherlands, including, but not limited to, services related to legal affairs, IT, consulting, journalism, administration, and financial intermediary services (for more details, see Table G.2 in the Supplemental Appendix).

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 of a drop closely below the threshold. There are some beneficiaries below the threshold in the post-reform period, which can be explained by a number of factors that we have discussed in footnote 17. Overall, migration increased substantially over time for all income levels as also shown in Figure 4.

IV. 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

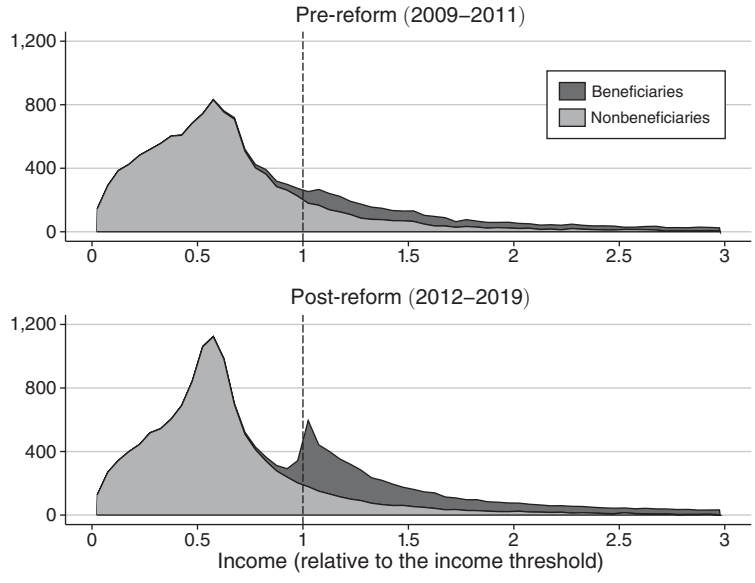

FIGURE 5. YEARLY COUNTS OF MIGRANTS BY INCOME AND BENEFICIARY STATUS

Notes: The figure shows the annual average number of arriving migrants for each level of annual taxable income (in the first full year of employment), normalized relative to the 30 percent rule's income threshold, in bins of 5 percent relative to the income threshold. The sample is restricted to migrants who start working within six months and stay for at least one full year and excludes migrants from Belgium and Luxembourg, employees in academia, and migrants under the age of 30 at the time of migration (see Table 1). Footnote 17 explains why we observe some beneficiaries below the threshold after 2012.

a difference-in-differences (DiD) framework.[29] To do so, we exploit the fact that some parts of the income distribution were unaffected by the reform.

A. *Affected Parts of the Income Distribution*

In Section II we showed how the fraction of beneficiaries relative to all migrants across the income distribution changed after the reform (see Figure 1). The pre-reform fraction of beneficiaries increased smoothly 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. 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 percent points. After 2012, around 65 percent of those above the threshold benefit, and this share increases only

[29] The reform and especially the income threshold were discussed in a written exchange between the Senate and the House of Representatives throughout the year 2011 (starting from the financial agenda published in April; see Kamerstuk 2010). In theory this may cause anticipation where migrants might strategically delay their starting date to have their eligibility assessed with the income threshold. However, we find no discontinuously higher level of arriving migrants in the early months of 2012, suggesting that this is not a major concern.

marginally with income.[30] The reform had little impact on eligibility for migrants either with income below the threshold (where it was close to zero even before 2012) or with high income, say exceeding the threshold by more than 50 percent.

Comparing the fractions of beneficiaries before and after the reform, we expect the strongest effect of the reform on the number of arriving migrants with income closely above the threshold. We define three groups of interest: individuals with taxable labor income at most 1 percent, 5 percent, or 10 percent above the threshold. In addition we consider the number of arriving migrants with taxable labor income more than 10 percent above the threshold. Finally, we also examine the number of arriving migrants with taxable labor income closely below the threshold (90–95 percent or 95–100 percent of the threshold),[31] although we have shown that the probability of benefiting hardly changed after the reform for this income range. Nevertheless, with sufficient wage bargaining, migrants with income just below the threshold might have been able to bargain up their wage across the threshold after the reform. Migrants with an income further below the threshold are certainly unaffected by the reform, as they never benefited before 2012, and are unlikely to be 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 nonbeneficiaries. As a result, one might interpret all estimates as intention-to-treat, meaning the effect of the reform on total inflow per income bin. While the impact of the reform on the eligibility probability is strongest closely above the threshold (see Figure 1), all three groups that we study (100–101 percent, 100–105 percent, and 100–110 percent) are close enough to the threshold such that we consider them as roughly equally affected. What is more important for interpreting magnitudes is that these bins vary substantially in size. To make estimates comparable, we include percent effects relative to the baseline (pre-reform) number of arriving migrants for all our estimates.

B. *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 5). By doing so, we calculate the change in the number of arriving migrants in the specific income ranges defined above. An advantage of the policy under study is that the pre-reform income distribution captures the scenario without an income threshold. Therefore, it is not necessary to extrapolate a distribution to calculate a counterfactual scenario, as with the bunching estimator (e.g., Saez 2010 and Kleven 2016). The density comparison approach hinges on the

[30] Reasons 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, lack of paperwork (e.g., proving previous residence or an employment contract), and unwillingness to apply on the side of the employer or employee.

[31] We consider the 90–95 percent group because, as discussed in footnote 17, there is some nonzero fraction of beneficiaries in 95–100 percent group, casting some doubt on conclusions drawn from this group.

identifying assumption that the income distribution of arriving migrants is constant across years absent the reform.

To formalize this idea, define the number of arriving migrants with an income 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 income distribution, we can estimate the total change in inflow for a particular income 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 income bin $b = [100\%, 105\%)$, which we denote by $+5\,\%$ for brevity:

$$\Delta^{+5\%} = \left(\frac{N_{post}^{+5\%}}{N_{post}} - \frac{N_{pre}^{+5\%}}{N_{pre}}\right) \cdot N_{post} = N_{post}^{+5\%} - N_{pre}^{+5\%}\left(\frac{N_{post}}{N_{pre}}\right). \tag{1}$$

Put differently, the impact of the reform is the difference in the number of arriving migrants in a certain income bin between the pre and post year, where the post year is scaled using the relative change in the total number of arriving migrants.[32]

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. The goal is to calculate the total change in inflow of migrants for the affected bins (as defined in the previous section).

Figure 6 shows the (yearly) estimates for the (affected) groups above and below the threshold compared to the pre-reform period. The comparison exposes a large and increasing inflow of migrants above the threshold regardless of the choice of affected bin. For the range 100–110 percent of the threshold, the estimates imply an additional inflow of up to 1,000 migrants. The difference in the width of the bins complicates a comparison of magnitudes, therefore we provide percentage effects in the next section. Changes in the below-threshold bins are either zero (95–100 percent bin) or small and negative (90–95 percent bin).

This comparison of densities provides 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 select a control group that is unaffected by the reform.

C. *Difference-in-Differences*

With the DiD 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

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

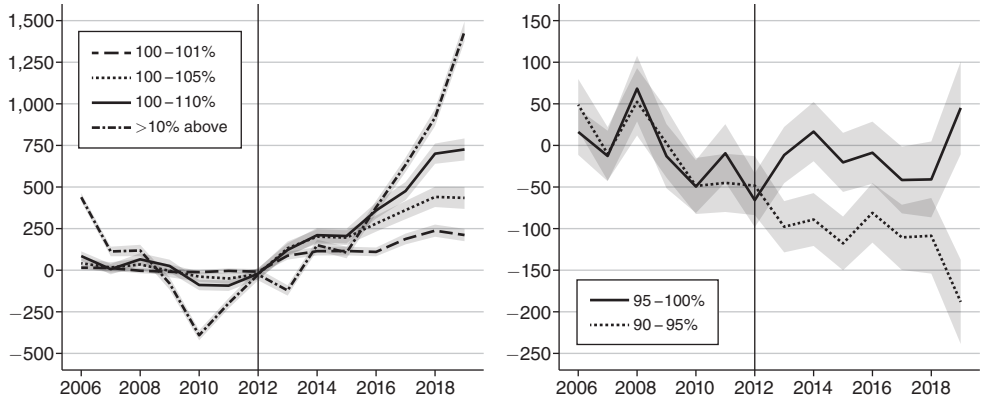

FIGURE 6. ESTIMATED ADDITIONAL INFLOW OF MIGRANTS BY INCOME RANGE BASED ON DENSITY COMPARISON

Notes: The figure shows the estimated additional number of migrants per year in income bins above the threshold, based on the density comparison. The left-hand side figure shows the estimates for income bins above the threshold, the right-hand side shows the estimates for income bins below the threshold. For every year and income range we compare the density with the pre-reform density averaged over the period 2006–2011 (see equation (1)). Ninety percent confidence intervals in grey. Standard errors are calculated based on the variation of the pre- and post-densities, assuming that the periods are uncorrelated.

control group as the inflow of migrants with income well below the the threshold (50 percent–90 percent) 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 nonbeneficiaries in both the pre- and post-reform periods (see Figure 1). 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.

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, as shown by Donald and Lang (2007). 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. In our setting the control and affected groups 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 percent 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 affected group in 2011. Depending on the size of the affected group, this procedure yields a large(r) amount of control groups.[33] We provide visual evidence that the

[33] We deviate slightly from this partitioning approach for treatment groups that are so large that only one control bin could be selected that has the same size in 2011. In those cases (for example the $>$ 110 percent group), we split the entire 50–90 percent range into two equally sized control groups which are only slightly smaller than the treatment group. See for example the lower-right figure in Figure 7.

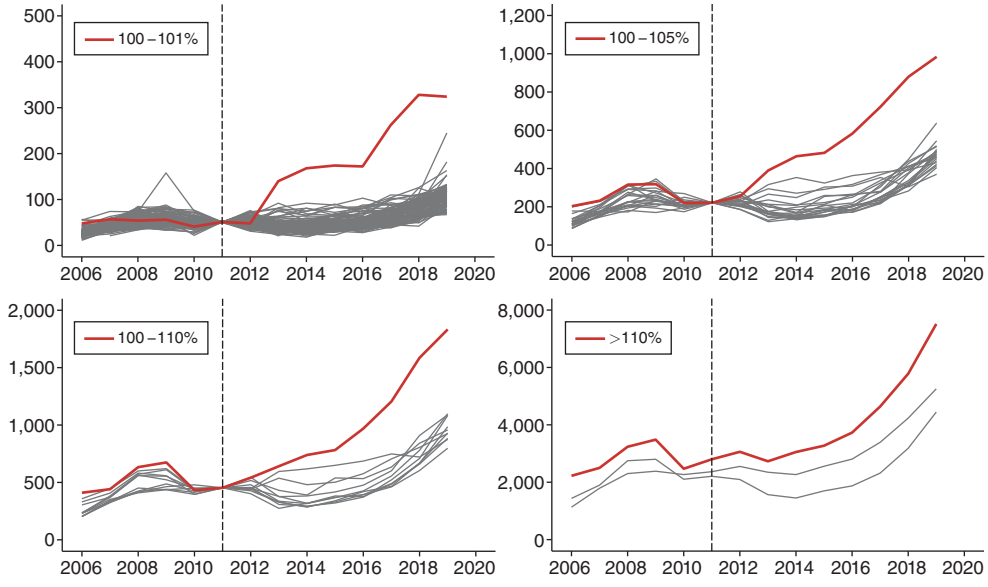

FIGURE 7. NUMBER OF ANNUAL ARRIVALS IN AFFECTED AND UNAFFECTED INCOME BINS

Notes: The charts show the annual arrival of migrants in the sample, per income group relative to the threshold. The control groups (grey) are the number of arrivals in the range corresponding to 50–90 percent of the threshold, partitioned such that the groups have the same size as the affected group in 2011, the year before the reform. In the lower-right figure (the $>$ 110 percent group), we partition the control group into two groups that are slightly smaller than the treatment group in 2011.

common trend in the pre-reform years holds for all of these smaller control groups in 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. Additionally, we conduct a permutation exercise (see Supplemental Appendix D), where the results support that the DiD model only yields positive and significant results for the actual affected group, but for none of the other groups.

With the outcome y (the number of arriving migrants) for group g and period p (pre-reform / post-reform period), the standard DiD model is

$$y_{gp} = \lambda_g + \theta_p + \beta D_{gp} + \varepsilon_{gp}, \tag{2}$$

where λ_g and θ_p denote group and period dummies, respectively. The term $D_{gp} = \mathbf{1}_{\{g=treatment\ group\}} \times \mathbf{1}_{\{t\geq 2012\}}$ indicates assignment to the treatment group in the post-reform period. In our setting the treatment group is the affected income bin. 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 4), the simple pre / post specification may fall short to capture the evolution of the 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

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

where θ_t is a set of dummy variables for each year t and D_{gj} captures the affected group in year j. The specification includes m leads and q lags, allowing the treatment effect to vary by year.[34, 35]

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 with Labor Income 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 50 percent − 90 percent 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 groups 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 percent and 5 percent 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.

Table 3 presents the estimates for the DiD specification (equation (2)), which compares averages for affected and control groups in the pre- and post-reform periods. The estimated treatment effects for the affected 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 ($>$ 110 percent) is positive and sizeable, but not statistically significant. The DiD estimates amount to an average additional inflow of 137 and 280 migrants per year in the 1 percent and 5 percent groups above the threshold, respectively, which correspond to a 291 percent and 137 percent increase relative to the pretreatment average in the treatment group. The estimate for the group at most 10 percent above the threshold (96 percent increase) is also sizeable and significant. Although the estimate for the group more than 10 percent above the threshold is positive, it is smaller in relative terms and not statistically different from 0.

[34] A growing recent literature has pointed out biases in two-way fixed effects models when treatment occurs at different points in time for different units, and treatment effects are time-varying (e.g., De Chaisemartin and d'Haultfoeuille 2023). Since we evaluate a reform at one single point in time and our control groups are never treated, these concerns do not apply to our setting.

[35] Using heteroskedasticity-robust standard errors leads to substantially smaller confidence intervals. We prefer to be conservative and therefore report nonrobust standard error throughout the analysis.

Table 3—DiD Estimates for Affected Groups with Income above the Threshold

	Dependent variable: Number of arriving migrants				
	Affected groups, defined as income relative to threshold				
	100–101% (1)	100–105% (2)	100–110% (3)	> 110% (4)	> threshold (5)
Affected group × post-reform period	136.7	279.6	402.9	804.3	1,334.2
	(12.6)	(53.2)	(114.2)	(689.3)	(791.6)
Observations (bins × periods)	1,256	294	154	42	42
Included individuals	70,834	73,601	80,215	119,967	131,406
Adjusted R^2	0.29	0.30	0.28	0.29	0.44
Pre-reform yearly average (individ.)	47	204	419	2,120	2,120
Effect relative to pre-reform yearly average	291%	137%	96%	38%	63%
Accumulated effect (2012–2019)	1,031	2,257	3,306	6,141	10,959

Note: The table shows the coefficients for the interaction effects from the DiD specification (β in equation (2)). Standard errors in parentheses. "Observations (bins × periods)" provides the number 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 groups 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 in the range of 50−90 percent of the threshold. "Accumulated effect (2012–2019)" is calculated as the sum of the coefficients for the interaction effects from the event-study specification.

To allow for dynamically evolving treatment effects, we present event-study estimates (equation (3)) in Figure 8. For the affected groups close to and above the threshold (1 percent, 5 percent, and 10 percent above the threshold), the effect is positive and significant from 2012 onward, and increasing over time. The dynamic estimates show that the impact by the end of our observation window is even substantially larger. For example, almost 500 additional migrants arrived in 2019 with income at most 5 percent above the threshold, relative to a pre-reform average of 204 migrants per year. The treatment effects for those more than 10 percent above the threshold are positive, but imprecisely estimated and become statistically significant only after 2017. Notably, for all groups, 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 with Labor Income 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 V. While eligibility for those closely below the threshold hardly changed in 2012 (it was close to zero both before and after, see Figure 1), the reform created a strong incentive for those below the threshold to bargain up their wage across the threshold. As a result, the arrival of migrants below the threshold might have decreased. Table 4 shows estimates from the DiD model for an affected group consisting of arriving migrants with income in the range of 95–100 percent and 90–95 percent of the threshold. The DiD estimates for both groups are small in magnitude and not statistically significant. The event-study estimates (Figure 9) show a

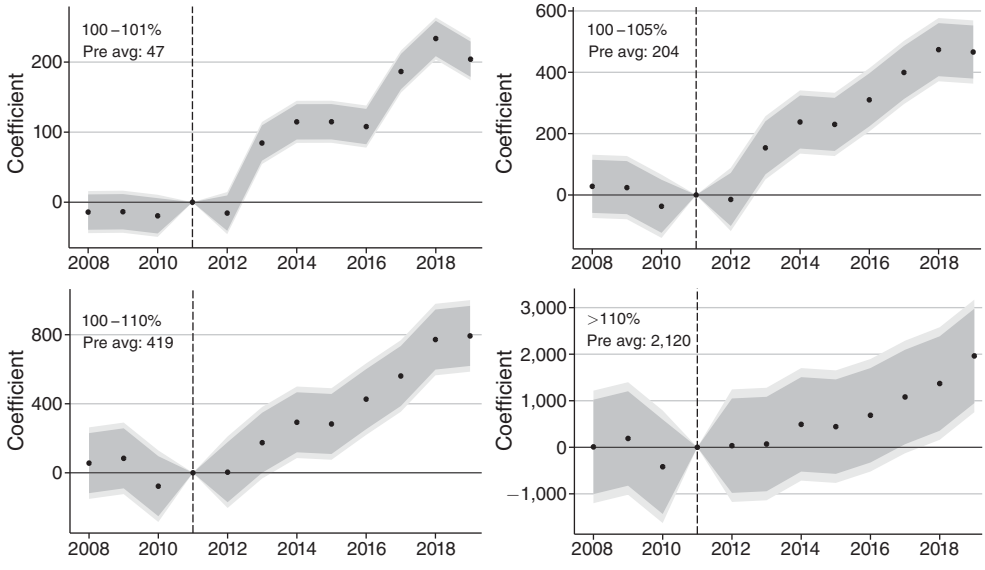

FIGURE 8. EVENT-STUDY ESTIMATES FOR AFFECTED GROUPS WITH INCOME ABOVE THE THRESHOLD

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

similar picture. For the 90–95 percent group, we find negative estimates, although statistically insignificant and small until 2017. For the 95–100 percent group, the estimates are never significantly different from zero.

In summary, the findings indicate that the reform has resulted in a greater influx of migrants with incomes at or above the threshold. This effect has intensified over time, as evidenced by the event-study analysis. This dynamic phenomenon aligns with the idea that employers and employees require time to acclimate to the new regulations and adjust their recruitment strategies to include international hires. Notably, the estimated increase in arrivals with incomes close to or above the threshold is considerably greater than the relatively minor (and insignificant) impact observed for migrants with incomes below the threshold. The estimated change in the inflow of migrants is very similar in both sign and magnitude for the DiD approach (Section IV) and the estimates based on the entire income distribution (Section IVB).[36]

D. *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 neither by theory nor by policy. Nevertheless, Figure 7 shows that

[36] For a comparison of the estimates from both approaches, see Table C.1 in the Supplemental Appendix.

Table 4—DiD Estimates for Affected Groups with Income below the Threshold

	Dependent variable: Number of arriving migrants	
	Affected groups, defined as income relative to threshold	
	95–100%	90–95%
	(1)	(2)
Affected group × post-reform period	−0.7	−74.5
	(61.0)	(59.0)
Observations (bins × periods)	238	252
Included individuals	71,014	73,518
Adjusted R^2	0.08	0.07
Pre-reform yearly average (individ.)	252	249
Effect relative to pre-reform yearly average	0%	−30%
Accumulated effect (2012–2019)	8	−544

Notes: The table shows the coefficients for the interaction effects from the DiD specification (β in equation (2)). Standard errors in parentheses. "Observations (bins × periods)" provides the number 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 groups 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 in the range of 50−90 percent of the threshold. "Accumulated effect (2012–2019)" is calculated as the sum of the coefficients for the interaction effects from the event-study specification.

all control groups that we use (which are all subgroups of the 50–90 percent 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. A permutation exercise confirms this (see Section D in the Supplemental Appendix).

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.[37] 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 percent 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 of Table 5, we show that results remain very similar.

Next, we provide robustness estimates using income measured in the year of arrival, extrapolated into an annual income.[38] The advantage is that this measure reflects earnings from the first employment contract in the Netherlands, but the extrapolation is also prone to measurement error.[39] The estimates (column 4 of Table 5) are again very similar to our baseline results. Only the effect for the 95–100 percent group is now positive and statistically significant. This difference

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

[38] The 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. Note that for the eligibility decision, a similar adjustment is applied: The income threshold is adjusted proportionally based on the share of the year in which the employee was employed.

[39] It may not fully capture changes in compensation, potential bonuses, or systematic variation in monthly salary payments across the year due to holiday allowance payments (mainly in May).

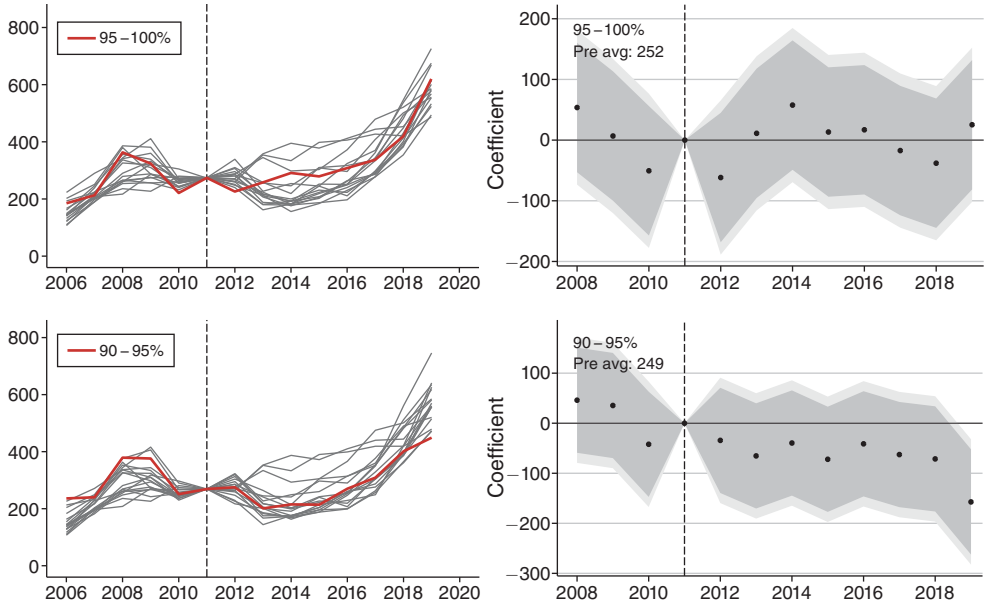

FIGURE 9. NUMBER OF ARRIVALS AND EVENT-STUDY ESTIMATES, MIGRATION BELOW THE THRESHOLD

Notes: The left-hand side charts 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 percent to 90 percent 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 charts show the coefficients for the interaction terms in the event-study specification, along with the 90 percent and 95 percent confidence intervals. "Pre avg" is the average number of arriving migrants in the respective income bin in the pre-reform period.

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.

We provide two more sets of results that speak to the sample construction: One is where we include migrants that stay in the Netherlands for at least 365 days (as opposed to a full calendar year in the baseline), and the other is where we only include a migrant's first spell in the analysis sample (in the baseline sample all spells are taken into account as long as there are at least two years in between). In both cases the resulting estimates are very similar to the baseline results, which underlines that our sample selection approach does not drive our results.

A further concern that warrants consideration pertains to the impact of wage growth on the definition of the affected groups. Specifically, it could be argued that above average wage growth may shift the entire income distribution, thus leading to a spurious effect in the count of migrants per income range. This concern holds little weight in our setting for two reasons. Firstly, we express the income distribution in relation to the indexed income threshold.[40] As depicted in Figure 5, the modes of

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

Table 5—DiD Estimates: Robustness against Alternative Samples and Measurements

	Dependent variable: Number of arriving migrants					
	Baseline	Excl. Germans	Nonchangers	Partial year	365 days	First spell
100–101% group	137 (13)	133 (12)	107 (11)	100 (24)	131 (27)	134 (11)
100–105% group	280 (53)	283 (49)	229 (44)	288 (67)	331 (64)	277 (49)
100–110% group	403 (114)	403 (107)	303 (101)	403 (143)	459 (146)	398 (109)
> 110% group	804 (689)	773 (668)	303 (699)	676 (735)	874 (797)	890 (638)
> threshold	1,334 (792)	1,311 (767)	755 (813)	1,233 (852)	1,506 (908)	1,386 (740)
95–100% group	−0.7 (61)	5 (58)	−4 (44)	227 (63)	−11 (73)	13 (53)
90–95% group	−75 (59)	−73 (57)	−56 (41)	5 (64)	−113 (79)	−66 (53)

Notes: The table shows the DiD treatment effects (coefficient for interaction terms in DiD specification equation (2)). The columns refer to the following specifications. "Baseline" repeats the baseline specification. "Excl. Germans" reports baseline results excluding Germans from the sample. "Nonchangers" reports baseline results excluding workers that switch employer within the year. "Partial year" uses income measured in the (partial) year of arrival instead of the first full calendar year. "365 days" limits the sample to migrants that stay at least 365 days instead of at least a full calendar year. "First spell" reports baseline results including only the first migration spell for each individual. The rows capture the affected groups (each coefficient stems from a separate regression). Standard errors in parentheses. The control group contains migrant inflow in the 50 percent to 90 percent range relative to the threshold.

the income distribution in the pre- and post-reform periods are situated at a similar location. Secondly, above average wage growth would result in a rightward shift of the entire income distribution, but it can not explain the bunching observed at the threshold.

We provide a final reassurance that aggregate labor market developments are not responsible for our results by considering migrants employed in academia. Since the income threshold does not apply to them (they are eligible regardless of their income), we should not observe any bunching closely above the threshold. In Figure C.2 of the Supplemental Appendix, we show the income distribution of arriving migrants before and after the reform and find that indeed there is no bunching at all at the threshold for this specific group.[41]

E. *Explaining the Migration Response*

We have shown that there is a substantial increase in the inflow of migrants in the wage bins 1 percent, 5 percent, and 10 percent above the threshold. Also the number of arriving migrants with income more than 10 percent 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 II, to investigate what drove these changes. Firstly, we compare working hours of beneficiaries before and after the reform to examine whether migrants have increased their

[41] Note that we do not perform an actual placebo DiD estimation exercise because also for academics the eligibility criteria changed slightly in 2012. While this would invalidate the placebo interpretation of a DiD estimate for academics, the lack of bunching at the threshold in Figure C.2 of the Supplemental Appendix is still supportive evidence for the validity of our main results.

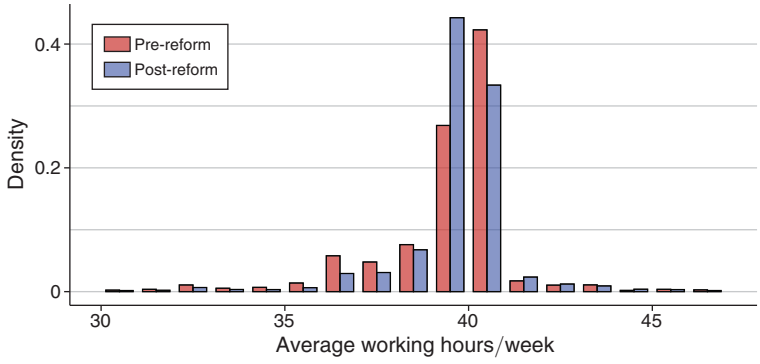

FIGURE 10. DISTRIBUTION OF AVERAGE WEEKLY WORKING HOURS

Notes: 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.

labor 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 migrants to increase their income to meet the threshold is to increase working hours (labor supply). To examine to what extent this effect 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 before the reform and remains very similar after. If anything, we find that weekly working hours decreased slightly with 38 hours becoming more common than 40 hours after the reform.[42] 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 II and in Section B of the Supplemental 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 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

[42] Also, for nonbeneficiaries the distribution of average weekly working hours hardly changes, indicating the absence of a time trend (see Figure C in the Supplemental Appendix).

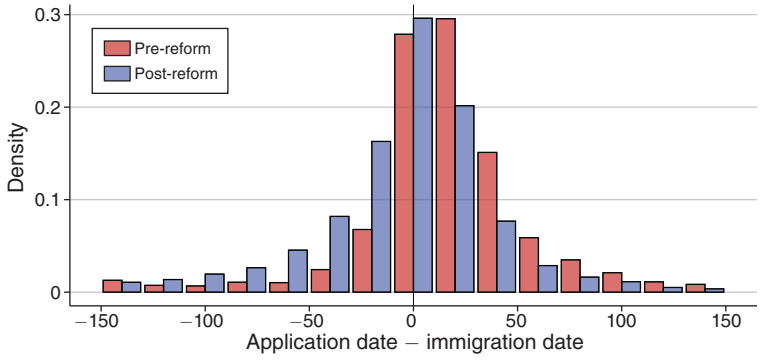

FIGURE 11. DISTRIBUTION OF TIME BETWEEN APPLICATION AND IMMIGRATION

Notes: The chart show the distribution of the difference between application date and immigration date. Positive values mean that the application was filed after the immigration date. The figure refers to the subsample of 30 percent rule beneficiaries.

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. In fact, our estimate of the increase in the total number of migrants above the threshold (last column in Table 3, 1,334 additional migrants) can be interpreted as the 'migration response'. Bunching from above may have affected the precise income within this income range, but not the total count.

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.

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. Figure 11 shows the 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.[43] 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.

[43] Figure C.4 in the Supplemental Appendix also shows that, relative to the start of employment, the application is filed earlier.

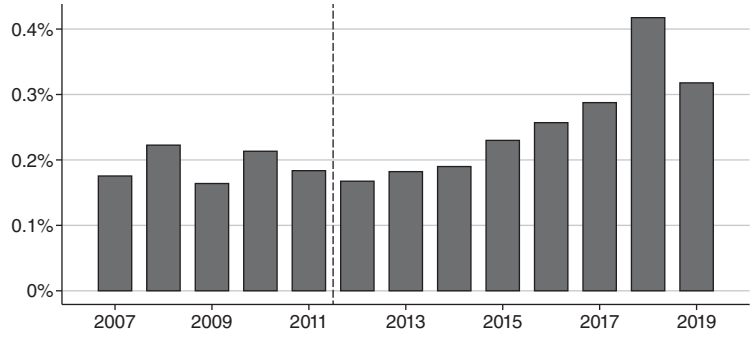

Figure 12. Percentage of Firms That Hire a Beneficiary for the First Time

Notes: The chart shows the percentage of firms that hire a migrant benefiting from the 30 percent 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 dashed vertical line marks the year 2012, when the reform came into effect.

Improved transparency and predictability may also affect the pool of firms that make use of the 30 percent rule. Consider a 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 and predictability of the rule may have incentivized a broader range of firms to apply. Figure 12 shows that the share of firms that hire a beneficiary for the first time increases steadily after 2012. This finding suggests that the reform simplified access to the tax rule for a larger range of firms.[44]

F. *Heterogeneity by Region of Origin and Sector of Employment*

The increased inflow of migrants with incomes closely above the threshold raises the question which types of migrants were activated most by the increased availability of the tax reduction. There are two salient dimensions to consider when contemplating the heterogeneous effects: the country of origin and the sector in which the migrants commence their work. Given that roughly half of all migrants originate from within the EU, we evaluate the reform's impact on EU and non-EU migrants. Additionally, we divide the sample into two groups based on the primary sector of employment–the largest sector being business services (accounting for approximately 40 percent of all eligible migrants) and all other sectors combined. Unfortunately, more granular breakdowns of sectors or countries of origin are

[44] In addition, we compute the hiring concentration of firms (Herfindahl-Hirschmann-Index, HHI) per year (sum of the squared share of beneficiaries hired per firm). The HHI for hiring beneficiaries increases by approximately 65 percent in the post-reform period, while it only increases moderately by 17 percent for all hirings (see Table C.2 in the Supplemental Appendix). 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.

Table 6—DiD Estimates: Heterogeneity by Region of Origin and Sector

	Dependent variable: Number of arriving migrants				
	Baseline	EU	Non-EU	Business services	Nonbusiness services
100–105% group	280 (53)	103 (33)	178 (22)	148 (24)	133 (37)
Pre-refom avg.	204	100	104	62	146
% effect	137%	103%	172%	239%	91%
100–110% group	403 (114)	95 (67)	313 (54)	253 (70)	160 (76)
Pre-reform avg.	419	198	224	127	299
% effect	96%	48%	140%	199%	53%
> threshold	1,334 (792)	40 (396)	1,299 (438)	1,051 (335)	303 (556)
Pre-reform avg	2,120	1,064	1,056	158	1,962
% effect	63%	4%	123%	665%	15%
90–95% group	−75 (59)	−43 (35)	−19 (26)	9 (13)	−84 (48)
Pre-reform avg.	249	109	146	53	198
% effect	−30%	−39%	−13%	16%	−43%

Notes: The table shows the DiD treatment effects (coefficient for interaction terms in DiD specification equation (2)) for four samples (in columns: baseline as in Table 3, migrants with previous residence in EU countries, migrants with previous residence in non-EU countries, migrants working in "Business services" sector (first employment upon arrival), migrants working in all other sectors except for business services). The estimated coefficients are qualitatively similar when considering migrants from EFTA countries (EU, Switzerland, Norway, and Liechtenstein). The rows capture the affected groups and for each group the estimated DiD coefficient, standard errors in parentheses, pre-reform average migrant inflow in the treatment group, and the percentage effect of the additional migrant inflow relative to the pretreatment period is shown. The control group contains migrant inflow in the 50 percent to 90 percent range relative to the threshold.

unfeasible due to a lack of statistical precision. For brevity, we estimate the DiD model (equation (2)) for each specific subsample, although event-study estimates provide similar results (see Figures C.7–C.9 in the Supplemental Appendix). For all subsamples, the income densities for the pre- and post-reform periods show clear bunching at the threshold (see Figures C.5 and C.6 in the Supplemental Appendix).

Table 6 provides estimates for these subsamples, suggesting that migration from non-EU countries accounts for a larger share of the increase in overall migration. The disparity in effect size between migrants from EU and non-EU countries is especially apparent in higher income ranges. The estimated increase in the number of migrants from non-EU countries in the 100–110 percent income range is 140 percent compared to a 48 percent increase for EU migrants.[45] Explaining the heterogeneous response is not straightforward. One salient dimension is migration costs and visa procedures. Non-EU migrants face generally higher migration costs, both in terms of geographical distance and in costs of obtaining a visa. The increased transparency of the tax benefits may have spilled over to improve awareness and knowledge about the visa applications procedure.[46] At the same time, we also emphasize

[45] We do not find major differences in the duration of stay of migrants from within the EU and from outside the EU.

[46] Importantly, the process of residence and work permits did not change in the years around the 30 percent rule reform. Non-EU citizens can either apply for a joint residence and work permit (*gecombineerde vergunning voor verblijf en arbeid*, GWV), obtain a work permit by complying with the conditions of the highly skilled migrant

that the smaller estimates for the EU migrants may be partly explained by a stronger increase in low-skilled migration within the EU, which constitutes the control group in our DiD estimator (see Figure C.5 in the Supplemental Appendix).

The finding that non-EU migrants displayed a higher level of responsiveness to the tax incentives compared to their EU counterparts is relevant for the earlier mentioned concern about potential tax competition between EU countries. Our results suggest that such concerns are certainly warranted (as migrants from EU countries were increasingly attracted to move to the Netherlands), but the larger effectiveness in attracting non-EU migrants mitigates the concern to some extent.

Turning to sectors, we find that a large part of our positive impacts are driven by an increase in arriving migrants working in the business services sector which contains a range of legal, administrative and financial employers (see Table G.2 in the Supplemental Appendix). Given the sector's relatively small share (40 percent of beneficiaries, but only 11 percent of nonbeneficiaries, see Table 2), the estimated coefficients represent an increase of around 200 percent. Even in absolute terms the coefficient is larger than that for all other sectors combined, emphasizing that workers in this sector especially have been very responsive to the increased availability of the tax incentive.

V. Migration Elasticity and Tax Revenue

Our results indicate 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 (e.g., Kleven et al. 2020). Section E in the Supplemental Appendix provides further details regarding the calculations. In short, the migration elasticity η is defined as

$$\eta = \frac{\%\text{ change in migration inflow}}{\%\text{ change in net-of-tax rate}}.$$

We calculate the migration elasticity for the medium term, 2012–2016, and for the group of migrants with income in the range 100–150 percent. We focus on this group of migrants to capture the range where the probability of benefitting has increased most after the reform (see Figure 1), and hence identification is strongest. To check the sensitivity of the migration elasticity to this choice, we present estimates for the 100–140 percent and 100–160 percent group as well.

The numerator (percent change in migration inflow) follows from our DiD estimates. The denominator (percent change in net-of-tax rate) is $1 -$ average tax rate (we use the average tax rate, as migration is an extensive margin decision). We obtain information for the tax rates and social security payments from the OECD

scheme (*kennismigrantenregeling*) or the European Blue Card. While the two latter work permits have income thresholds attached, the income thresholds are around 36 percent and 60 percent higher than the income threshold for applying to the 30 percent rule. Neither the income threshold for these work permits nor visa regulations changed in the years around the reform.

Table 7—Migration Elasticity Estimates

	Pr(benefitting pre-period)		
	0	0.2	0.45
Inflow with earnings 100–140 percent of the threshold:			
Change inflow	22.3%	22.3%	22.3%
Change net-of-tax rate	14.5%	11.6%	8.2%
Elasticity estimate	1.54	1.92	2.71
Inflow with earnings 100–150% of the threshold:			
Change inflow	22.5%	22.5%	22.5%
Change net-of-tax rate	14.6%	11.7%	8.2%
Elasticity estimate	1.55	1.93	2.73
Inflow with earnings 100–160% of the threshold:			
Change inflow	22.9%	22.9%	22.9%
Change net-of-tax rate	14.4%	11.5%	8.0%
Elasticity estimate	1.60	2.00	2.85

Notes: The table shows the migration elasticity estimates for migrant inflow in the income ranges 100–140 percent, 100–150 percent, and 100–160 percent relative to the income threshold and for the years 2012–2015. As described above and in Supplemental Appendix E, the numerator is the estimated coefficient from the DiD specification 2, the denominator is the population-weighted percentage change in the expected net-of-tax rate per income bin. $\%\Delta$ captures the change in the (expected) population-weighted net-of-tax rate, corresponding with the pre-reform probability of benefitting in the first column. The last row shows the estimate where the pre-reform probability of benefitting is equal to the pre-refom period fraction of beneficiaries in the 100–150 percent income range.

Tax-Benefits Calculator (OECD, n.d.) and calculate a population-weighted mean of the average tax rate for (non)beneficiaries 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. We consider several scenarios (see the three columns in Table 7). Column 1 shows the scenario where 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 a lower bound of the elasticity (1.55). Alternatively, we can assume that prospective migrants formed their expectations about the net-of-tax rate in the pre-reform period using the empirical fraction of beneficiaries (which is 0.45). This scenario is shown in column 3 and yields an upper bound of the migration elasticity (2.73).[47]

This range of estimates of the migration elasticity between 1.33 and 2.36 is somewhat larger than estimates in the literature covering migration and preferential taxation (as reviewed in Kleven et al. 2020). For example, Kleven et al. (2014) find a mobility elasticity of 1.6 for very high earning migrants in Denmark, and Moretti and Wilson (2017) compute a mobility elasticity of 1.8 for top inventors in the United States. We conclude that the elasticity is even larger for medium-level earners.

[47] In related literature, the migration elasticity is sometimes estimated by regressing the logarithm of migrant inflow on the logarithm of the net-of-tax rate, where the latter is instrumented by the policy reform (e.g., Kleven et al. 2014 and Giarola et al. 2023). When we apply this approach, we get a similar range of elasticity estimates between 1.92 and 2.24 for the 100–150 percent group, with a strong first stage (F-stat $>$ 160).

Given the large additional migration inflow and the effective lower taxation for beneficiaries, one may want to consider the implications for tax revenues. Section F in the Supplemental Appendix shows some back-of-the-envelope calculations which are based on a number of simplifying assumptions.[48] We find that the net effect from the additional migration on the government budget is likely to be net positive. Intuitively, this finding is driven by the fact that, although beneficiaries pay less taxes than regular migrants, they still pay taxes on 70 percent of their income. Hence, the tax base increases and this raises tax revenues more than the decrease in tax revenues due to a higher fraction of beneficiaries.[49] The balance becomes even more positive when taking into account that a share of migrants stays beyond the duration of the tax exemption, paying full taxes afterwards.

VI. Conclusion

In this paper we examine the effects of a preferential income tax scheme for foreigners on migration 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 with mid-level earnings.

We find large and statistically significant increases in the arrival of migrants with income closely above the income threshold (approx €35.000 in 2012). The increase amounts to a doubled inflow in the income range of at most 10 percent above the threshold. The largest responses are observed for migrants from outside the EU, and for migrants starting employment in the business services sector. 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.

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

[48] More 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). Based on our DiD estimates, we compute the tax revenue from the additional migrants along with the loss of tax revenue from granting the benefits to more migrants (as reflected by the share of beneficiaries).

[49] Under the assumption of no external effects on natives, the tax revenue maximizing tax rate is the Laffer rate $1/(\eta + 1)$ (see Kleven et al. 2020), which in our case ranges between 0.27–0.39. The top marginal tax rate faced by beneficiaries is substantially larger, implying indeed that the tax exemption positively impacts tax revenues.

the income distribution. As such, we find that tax incentives are at least as effective in attracting international migrants with midrange earnings levels as they are for migrants with very high earnings. Our estimated elasticity (ranging between 1.6 and 2.7) falls in the higher range of findings from previous studies surveyed in Kleven et al. 2020). These findings are important as the number of similar preferential tax schemes within the EU has increased dramatically since the 90's (from 5 to 28; see Flamant, Godar, and Richard 2021). While eligibility criteria vary greatly, several share the feature that also migrants below the absolute top of the income distribution can benefit (e.g., Italy and Finland).

Our study focuses on the direct migration response to tax incentives. Three further considerations are beyond the scope of this paper, yet are essential for policy making and may constitute directions for future research. First, increased migration may generate negative or positive spillovers on domestic employment and wages (e.g., the overview by Dustmann, Schönberg, and Stuhler 2016). Second, it is likely that attracting skilled workers through tax incentives impacts the countries of origin of the migrants. Third, tax exemptions for migrants might trigger strategic responses from neighbouring countries to implement similar tax incentives. Concerns about such a "race-to-the-bottom" might be warranted by the stark increase in the number of such tax rules within the EU over the past decades (Flamant, Godar, and Richard 2021) and require coordination between countries.

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