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The Impact of Place-Based Tax Credits on Internal Migration Patterns

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Adi Finkelstein

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The Impact of Place-Based Tax Credits on Internal Migration Patterns*

Adi Finkelstein[1]

Abstract

This paper evaluates the effectiveness of place-based income tax credits in shaping household residential location decisions in Israel. Exploiting a 2016 policy reform that expanded eligibility to 321 localities – doubling the number of eligible workers – and using administrative panel data on employees from 2000 until 2019, I estimate the effects of the reform on migration patterns among medium- to high-income households, measured by the number of migrants and their composition. The reform significantly increased inflows to newly eligible localities, while outflows did not decline, resulting in a substantial improvement in net migration rates. The composition of migrants shifted toward higher-income households, while the mean distance from the origin locality remained unchanged. Effects were heterogeneous: small Jewish localities with low benefit rates and those in the Southern and Judea and Samaria regions experienced stronger positive impacts, whereas large and Arab localities saw no significant net change. No evidence of adverse spillover effects on neighboring localities was found. Fiscal analysis indicates that the reform incurred substantial costs per additional net migrant, distributed across newcomers and incumbents. These findings suggest that while the policy achieved its stated goal of attracting higher-income households to remote localities, its cost-effectiveness remains limited, underscoring the need for more targeted interventions.

JEL classification: H24, R28, H31

Keywords: Place-based policies; Income tax credits; Regional Migration Policy; Residential location choice; Fiscal incentives; Tax-induced mobility

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1 Bank of Israel, Research Department and Hebrew University of Jerusalem, Department of Economics. Email: adi.finkelstein@boi.org.il

השפעת הטבות המס ליישובים על דפוסי ההגירה הפנימית

עדי פינקלשטיין[1]

תקציר

מאמר זה בוחן את יעילותן של הטבות מס ליישובים בהשפעה על החלטות משקי בית לגבי מקום מגוריהם בישראל. באמצעות נתוני פאנל מנהליים על שכירים בישראל בשנים 2000 – 2019 ורפורמת מדיניות משנת 2016 שהרחיבה את הזכאות ל-321 יישובים נוספים – מהלך שהכפיל את מספר העובדים הזכאים – אני מעריכה את השפעת ההטבות על דפוסי ההגירה של משקי בית בעלי הכנסה בינונית-גבוהה, הן במונחי היקף המהגרים והן במונחי הרכבם. הרפורמה הגדילה באופן ניכר את זרמי ההגירה הנכנסת ליישובים שהפכו למוטבים, בעוד שזרמי ההגירה היוצאת לא פחתו. הטבות המס גם שינו את הרכב המהגרים, בכך שמשכו משקי בית בעלי הכנסה גבוהה יותר אך מרחק יישוב המוצא נותר דומה. ההשפעות היו שונות בין סוגי ישובים: יישובים יהודיים קטנים עם שיעורי הטבה נמוכים וישובים במחוזות הדרום ויהודה ושומרון נהנו מהשפעות חיוביות יותר, בעוד שביישובים גדולים ובישובים ערביים לא נרשמה השפעה מובהקת. לא נמצאו השפעות שליליות על יישובים סמוכים שלא קיבלו הטבות. הניתוח הפיסקלי מצביע על כך שהרחבת ההטבות גררה עלות גבוהה לכל תושב נטו נוסף, עלות שחולקה בין תושבים חדשים לוותיקים. הממצאים מצביעים על כך שבעוד שהמדיניות השיגה את מטרתה המוצהרת – משיכת משקי בית אמידים ליישובים המוטבים – יעילותה התקציבית מוגבלת, ויש מקום לשקול התערבויות ממוקדות יותר.

סיווג JEL: H24, R28, H31

מילות מפתח: מדיניות מבוססת-מקום; זיכוי מס הכנסה; מדיניות הגירה אזורית; בחירת מקום מגורים; תמריצים פיסקליים; הגירה כתוצאה ממיסוי

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* אני מודה לנעמי פלדמן על הדרכתה ולדוד ג'נסוב, דניאל פלזנשטיין, דריו סרנו-פואנטה, שי צור ואיל ארגוב על הערותיהם הבונות. אני גם מודה למשתתפים בסימפוזיון המחקר של מכון אלרוב של מחקר חדש בכלכלת נדל"ן, אזורית ועירונית, בסמינר המחקר של בנק ישראל, בכנס האגודה הישראלית לכלכלה, בסדנת המיסוי והניידות של ה-ZEW, בכנס המיסוי השנתי של Manneheim taxation ולחברי חטיבת תכנון וכלכלה ברשות המסים על הערותיהם החשובות. תודה נוספת למרינה שפייכלר מרשות המסים על העמדת הנתונים לרשותי, לחגי אטקס ונעם זוסמן על הנגשת נתוני הטבות המס ההיסטוריים, ולאלעד דה-מלאך על שיתוף מסד הנתונים המעובד של הרשויות המקומיות.

[1] עדי פינקלשטיין – בנק ישראל, חטיבת המחקר והאוניברסיטה העברית בירושלים, מחלקה לכלכלה adi.finkelstein@boi.org.il

1. Introduction

The use of place-based policy tools to stimulate economic activity and improve welfare in underdeveloped regions has become increasingly common in various countries. This approach contrasts with supplementing transfer payments to economically disadvantaged households, which are often over-represented in these areas. In many developed countries, these policies take the form of tax incentives for businesses, including corporate tax credits and payroll tax reductions, contingent on the firm's geographic location. In Israel, similar strategies are implemented through government programs targeting the geographic periphery, such as land subsidies in "National Priority Areas", lower corporate tax rates for exporting firms, wage subsidies targeting populations with low employment rates, and personal income tax benefits for residents of rural or peripheral localities.

This research investigates the impact of the latter, i.e., locality-based tax credits. The policy's stated goal is to attract high-earning individuals to peripheral localities. My primary focus is thus to analyze how these tax incentives influence inflows and outflows of the targeted population, the composition of movers by taxable income, and their impact on neighboring localities.

The economic literature offers mixed perspectives on the effectiveness of place-based programs. The classical spatial equilibrium model predicts that such incentives will distort the allocation of production factors, shifting them from more productive to less productive areas and potentially neutralizing the intended welfare effects through higher land values in these regions (Glaeser & Gottlieb, 2008). Conversely, empirical evidence indicates that economic barriers, rather than mere personal preferences, constrain individuals from leaving economically disadvantaged areas, thereby impeding their migration to regions with more robust economic opportunities (Bergman et al., 2019).

Proponents of place-based policies argue that, given the growing polarization between economically resilient and weak regions, these measures can justify their economic costs by increasing labor demand in areas plagued by chronic underemployment (Austin et al., 2018). Studies from various countries have documented a pronounced behavioral response to tax rate differentials across municipalities, districts, states, and countries (Schmidheiny, 2006; Liebig et al., 2007; Agrawal & Forenmy, 2019; Agrawal, Forenmy & Martínez-Toledano, 2025; Moretti & Wilson, 2017; Akcigit et al., 2016; Kleven et al., 2013; Rubolino & Giommoni, 2023).

Nonetheless, the effectiveness of place-based tax incentives in achieving their intended policy objectives – particularly regarding fiscal cost – remains a subject of debate. Previous research on Israel's tax benefit system for residents of peripheral areas has found limited impact on internal migration to targeted localities and has shown that fiscal expenditure has benefited mainly incumbents rather than new entrants (Ben Na'im, 2010; Etkes, 2015). Despite these findings, locality-based tax credits remain a popular policy tool among elected officials in Israel.

This paper's contribution to the existing literature is twofold. First, while previous studies on the elasticity of workers' migration to tax rates have primarily focused on top earners (Kleven et al., 2020), this research broadens the scope to examine taxation-induced mobility across a broader segment of the income distribution. The policy analyzed in this study is effective for any household with an income above the tax threshold before taking into account locality-based credits. In Israel, this refers to the upper half of the income distribution. This broader focus has significant implications for economic policy beyond Israel, as it provides insights into how tax incentives can influence location choices for a larger proportion of households, not just the highest earners. By examining the mobility responses of a more diverse income group, the findings can inform policymakers globally about the potential effects of tax-based incentives on population distribution and regional development across various socioeconomic strata. To the best of my knowledge, only Blumkin et al. (2020) and Rubolino and Giommoni (2023) have so far attempted to study a broader segment of the population. The former by estimating the effects of nationwide permanent tax cuts on international emigration from Israel, and the latter by analyzing inter-municipal tax competition in Italy.

Second, changes in individual tax rates examined so far have occurred amid tax competition among states, regions, or other jurisdictions. This competition can affect local government budgets and, therefore, the local public services they offer to their residents. This is consistent with the classic Tiebout model (1956), according to which individuals sort themselves across jurisdictions based on their preferences for taxation and public goods. In the Israeli case, the tax system is nationwide, and the differential in tax rates is determined at the central government level. This does not have a first-order effect on local authorities'

revenues,[1] thereby strengthening the ability to identify the effects of tax changes on households' location decisions.

In this study, I leverage administrative panel data covering a large sample of Israeli employees and a major policy reform that more than doubled the number of eligible localities. This setting enables a comprehensive, up-to-date analysis of the effects of locality-based tax credits on migration patterns among medium- to high-income households. The goal is to provide new empirical evidence on the effectiveness of such policies in shaping residential decisions and to inform future debates on their design and fiscal sustainability.

I use a Difference-in-Differences approach, comparing changes in outcomes for localities that became eligible for locality-based tax credits with those for other localities in the same regions that remained ineligible. An ideal setting for identification would include randomly selected localities across the country, grant them such benefits, and compare changes in outcomes with those of similar untreated localities, assuming potential outcomes are the same.

The actual setting, however, is more complex. In 2014, the government of Israel introduced a new formal scoring system that determined eligibility and the amount of locality-based personal tax credits. The score was based on the locality's socioeconomic status index and geographic location. My identifying assumption is that selection into treatment was not correlated with expected changes in migration flows, and that, in the absence of these changes, migration trends in both treated and untreated localities would have evolved the same way, conditional on observable covariates. The selection method also resulted in geographic clusters of treated localities in the Northern and Southern regions, which may have caused spillover effects on nearby untreated localities, thereby making them contaminated control groups. I therefore consider untreated neighboring localities a semi-treated group and estimate the effect of exposure, using more distant localities as my control units, while limiting their distance from treated units to strengthen the parallel trends assumption.[2]

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[1] This may have a secondary effect on local revenues from residential municipal taxes, although these taxes constitute a relatively minor source of income in the budgets of local authorities.

[2] Central cities such as Tel Aviv are not part of the study, though migrants to and from Tel Aviv are counted if they are observed in the participating localities.

My primary dataset is a panel of localities spanning 2006 – 2019. The panel is constructed by aggregating tax records for Israeli salaried workers, including their registered place of residence and annual income. This allows me to focus on workers for whom the tax credits were effective and to identify movers and their origin. The workers' data are augmented with local authority data, such as local spending on public goods, increases in housing supply, and demographic information, to control for other local factors that might have changed during the post-treatment period.

The analysis suggests that introducing tax credits led to a 32 percent increase in inflows of eligible employees and their spouses and a 15 percent increase in outflows. This effect sounds tremendous at first, but it is important to note that annual migration flows are small to begin with – only 8 percent of the relevant population relocates between localities annually, and a smaller share moves to the targeted regions. The mean household income of workers moving into treated localities increased by 9 percent, and total income inflows surged by 46 percent. Net migration rates for policy-targeted individuals in treated localities increased by an average of 1.6 percentage points, accounting for about half of the mean net migration rate in the treated localities during the post-treatment period.

I also find that the policy's effects varied across regions and ethnic groups. Jewish localities experienced positive net migration effects in both the number of immigrants and their average income. In contrast, Arab and Large localities did not experience any improvement in net migration rates. The Southern region benefited more than the Northern region from the expansion of eligibility, possibly due to fewer housing market frictions. Notably, the Southern region also showed positive effects in nearby non-eligible localities, suggesting spillovers. The impact was more pronounced in small localities and in those that became eligible in earlier years, compared with larger localities (with over 10,000 residents) and those that gained eligibility since 2016.

Due to the small migration flows of above-average-earning households into newly eligible localities, even substantial percent increases have a limited impact on the medium-term number of high-income residents. A back-of-the-envelope analysis indicates that the population eligible for a tax credit increased by 3.2 percent from 2016 to 2019. As a result, the tax expenditure – which primarily benefits incumbent households – is relatively expensive. The fiscal analysis indicates that the expansion of tax benefits resulted in substantial costs, with the annual value of tax benefits growing by approximately NIS 0.7 billion by 2019 due to

the doubling of eligible workers. Dividing the estimated cumulative tax expenditure during 2016 – 2019 (NIS 1.86 billion) by the cumulative net inflows during the same period (5,485 workers) yields an estimated fiscal cost of at least NIS 339,300 per additional net migrant during the first four years of eligibility expansion.

The next section of this paper outlines the institutional background of benefits for residents of geographically peripheral areas and the 2016 expansion of eligibility. Section 3 describes the data and the study population. Section 4 outlines the empirical strategy. Section 5 presents the results; Section 6 discusses the policy's economic significance, potential anticipation, and fiscal costs; and Section 7 concludes.

2. Policy Background

Locality-based personal income tax credits were first introduced in the Income Tax Ordinance in 1955 for residents of the city of Eilat. This policy tool was later used to encourage migration to newly established localities. Over the years, many localities were added to and removed from the list of eligible localities without clear criteria. The benefits were later codified in legislation as permanent benefits for anyone whose life center is in an eligible locality for at least 12 months.

The mechanism of tax credits affects workers' choices through the average tax rate, not the marginal rate. Nevertheless, unlike other tax credits in Israel, which are lump-sum[3], Locality-based tax credits are calculated as a percentage of the individual's annual labor income. Because tax credits are nonrefundable in Israel, the benefit applies only to workers with annual income above the tax threshold, which depends on personal deductibles and other personal tax credits.[4] This mechanism leads to a semi-regressive benefit scheme: higher statutory benefit rates apply only to higher-earning workers.

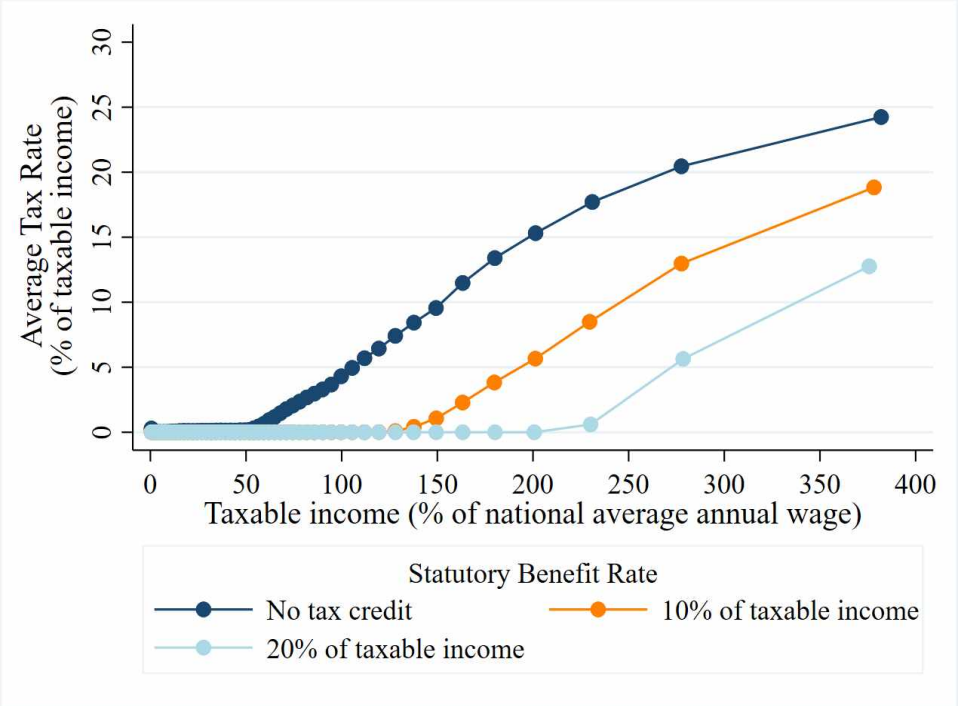
Figure 1 illustrates the differential tax schedule created by locality-based credits. It presents average tax rates under different statutory benefit levels for different income levels. The figure, based on administrative data (described in the next section), shows, for example,

[3] For example, child tax credits are a lump-sum amount per child based on age, with more credits given to parents with younger children.

[4] The tax threshold varies based on the employee's characteristics (number of children, special credit points, tax deduction for pension fund contributions, etc.).

that the average worker earning an average wage pays 5 percent of gross taxable income in income tax. For such a worker, there is no difference between a 10 percent and a 20 percent benefit rate because any rate above 5 percent is high enough to reduce post-credits tax liability to zero. To fully realize the maximum 20 percent statutory benefit rate, one must earn an annual income exceeding three times the average wage. The credit tool is only semi-regressive because the sum of the credits has a nominal cap: above a certain income ceiling, the benefit amount becomes a lump sum, and the benefit rate as a share of income decreases with income for those earning more than the cap.

Figure 1: Average Tax Rate by Benefit Level, 2015



Notes: The figure presents a binned scatter plot of actual average tax rates in localities whose residents were eligible for a 0, 10, or 20 percent statutory benefit rate in 2015. Workers are divided into 50 equally sized bins according to their taxable labor income in 2015, i.e. 2 percent of workers in each bin. Income is normalized to the annual average wage per employee post.

Since the early 2000s, several petitions have been filed with the Supreme Court challenging the unequal and arbitrary allocation of benefits between localities in Israel's geographic periphery. The petitioners argued that eligibility was politically motivated and unconstitutional.[5] In 2012, the Supreme Court ruled that the lack of clear and equal objective

[5] HCJ 8300/02 Nasser vs. The Government of Israel, (May 22, 2012) (hereinafter, the "Nasser Case").

criteria for distributing the benefits discriminated against certain localities and ordered the government to correct the mechanism for determining localities' eligibility.

Following the ruling, the government decided in February 2014 to amend the Income Tax Ordinance.[6] By the end of 2015, the Israeli Parliament (the Knesset) approved the amendment, establishing an annual method for calculating the credit for each locality.[7] Personal eligibility took effect on January 1, 2016. Figure 2 presents the geographic aspect of the amendment. The government's stated goal in granting tax benefits to residents of geographically remote localities was to attract more high-earning individuals to such localities.[8] The policy change gradually increased the number of eligible localities from 196 localities in 2015 to 517 in 2019.

Table 1 presents the localities' characteristics used to determine the benefit rates under the newly legislated formula, grouping the localities into three categories: newly eligible (treated units), not eligible (some of which are control units), and previously eligible (localities that were eligible before 2016). The characteristics in each cell are calculated as weighted averages based on the number of residents in each locality in 2013, the year before the government approved the new set of criteria.[9]

The summary statistics for the localities show that, in the Northern region, the newly eligible (treated) localities did not differ in socioeconomic status from the non-eligible group. However, they were more remote than the untreated localities, as measured by the peripheral index and proximity to the nearest border.[10] Their average population size is 20,000 residents, and the average statutory benefit rate by the end of the period was 9.2 percent. In the Southern region, the difference between treated and control localities is much greater because of the high share of Bedouin localities in the south that became eligible

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6 Government Resolution No. 1340 (2014, February 16). Retrieved from https://www.gov.il/he/departments/policies/2014_dec1340 (in Hebrew).

7 See the criteria for inclusion on the list and the scoring method for allocating the benefit rate in Appendix A.

8 From the explanatory notes to the government resolution: 'It is proposed that only a "peripheral locality" be eligible for the benefits established by the ordinance […] The locality must be at a lower socioeconomic level than cluster 8 in the Central Bureau of Statistics socioeconomic index, as localities at a higher socioeconomic level do not need tax benefits to attract strong residents.'

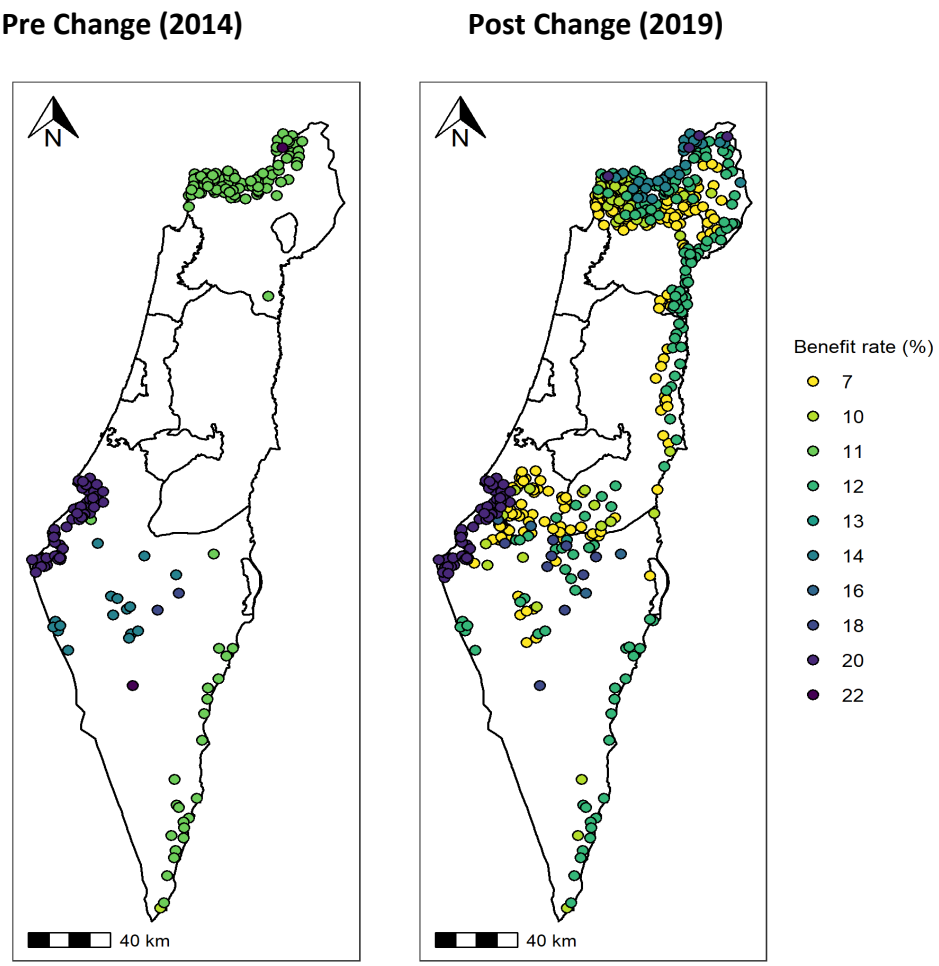
9 Very small or unique localities were dropped from the sample, including 6 previously eligible and 19 newly eligible localities.

10 The Israeli CBS calculates the Peripherality Index based on the travel distance from all other localities, weighted by their population and distance to Tel Aviv. For more details, see: https://www.cbs.gov.il/en/subjects/Pages/Peripherality-Index-of-Local-Authorities.aspx.

following the reform. For this reason, the average statutory benefit rate stood at 13.8 percent, much closer to the "previously eligible" localities in the same region. In Judea and Samaria, there were no locality-based benefits since the early 2000s. Following the change in the criteria, many socioeconomically strong localities became eligible. However, their characteristics resemble those of non-eligible localities in other regions, except for their peripherality index, which is low due to the small size of the surrounding localities.

Figure 3 shows the dynamic aspects of the policy change across three dimensions: locality treatment status, personal income level, and time. The figures illustrate that the benefit rate granted to the newly eligible localities was lower on average than in previously eligible localities, reducing average tax rates by only 5 – 7 percentage points. This was mainly because they are less geographically peripheral. It is also evident that the benefits were most effective for workers earning 100 – 300 percent of the national average salary.

Figure 2: Statutory Benefit Rates 2014 – 2019



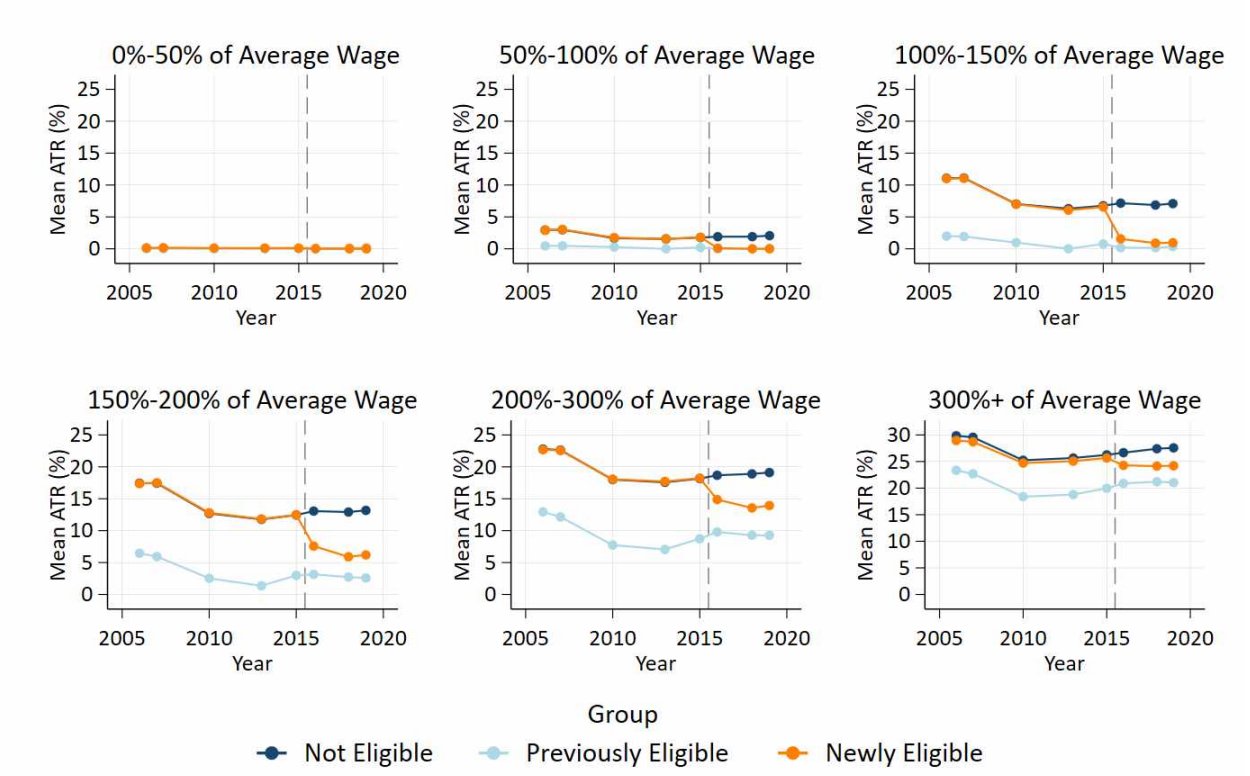
Notes: The figures present the administrative regions in Israel and the statutory benefit rates before and after the adoption of the new criteria system. Darker dots represent localities whose residents are eligible to higher statutory benefit rates.

Table 1: Locality Characteristics According to Benefit Criteria

Region	Group	N	Socioeconomic Cluster 2013	Peripherality Cluster 2013	Average Distance to Lebanon/ Syria/ Gaza (km)	Average Distance to Tel Aviv (km)	Average Population in Locality (Thousands) 2013	Benefit Rate 2019 (%)
North	Previously Eligible	96	4.8	3.7	10.6	124.3	24.1	11.7
			(1.5)	(0.5)	(8.3)	(21.8)	(20.3)	(2.8)
	Newly Eligible	184	4.1	3.8	18.2	116.7	20.1	9.2
			(2)	(0.5)	(5.9)	(20)	(13.5)	(2.4)
	Not Eligible	134	3.9	4.9	38.8	86.3	25.8	0
			(1.9)	(0.6)	(6.4)	(8.1)	(23.6)	(0)
South	Previously Eligible	92	4.2	3.3	68.0	143.3	23.8	15.5
			(1.5)	(1.4)	(67.6)	(91)	(14.9)	(3.6)
	Newly Eligible	76	3.1	4.5	32.7	84.0	26.7	13.8
			(2.2)	(0.6)	(14.9)	(19.3)	(24.8)	(5.2)
	Not Eligible	65	5.1	6.0	25.8	53.2	153.4	0
			(0.9)	(0.8)	(9)	(26.2)	(76.6)	(0)
Judea and Samaria	Newly Eligible	42	5.0	4.0	64.7	83.5	3.2	10.2
			(1.4)	(0.1)	(14)	(9.6)	(2.6)	(2.2)
	Not Eligible	84	4.1	5.9	70.8	48.9	28.5	0
			(2.3)	(0.5)	(10.1)	(15.6)	(23.7)	(0)
Jerusalem	Not Eligible	62	4.2	8.4	63.4	50.3	663.2	0
			(1.1)	(1.2)	(7.4)	(5.9)	(322)	(0)
Haifa	Not Eligible	85	5.4	6.0	47.9	67.5	103.6	0
			(2)	(0.5)	(18)	(19.5)	(110.5)	(0)
Central	Not Eligible	218	6.1	7.8	64.0	11.0	105.5	0
			(1.7)	(0.8)	(15.4)	(7.2)	(86.7)	(0)
Tel Aviv	Not Eligible	12	6.5	9.5	60.4	0	221.4	0
			(1.9)	(0.7)	(5.2)	(0)	(146.9)	(0)
Total		1,149	5.2	6.8	51.2	48.8	175.3	2.0
			(2)	(2.1)	(24.4)	(47)	(242)	(4.7)

Notes: The table presents summary statistics of localities' attributes before the government decision to expand locality-based credits. Localities are grouped by treatment status in 2016 – 2019. Standard deviations are included in parentheses to indicate the variance within each group. The benefit rate for 2019 is expressed as a percentage, reflecting the statutory tax benefit as a percentage of income. Socioeconomic and peripherality indices are published by the Israeli Central Bureau of Statistics on a scale of 1 – 10, where 1 represents a low socioeconomic status and being more remote, respectively. For details on the Peripherality Index, see: https://www.cbs.gov.il/en/subjects/Pages/Peripherality-Index-of-Local-Authorities.aspx.

Figure 3: Average Tax Rate by Income Segment and Locality Group



Notes: The figures present the average tax rate across three dimensions: time (2006-2019), income segment, expressed as a percentage of national wage per employee post; and locality eligibility group ("Newly eligible" are the treated localities, "Not eligible" are the control/exposed groups, and previously eligible are excluded from the estimations). It shows that new tax credits were effectively the largest (7% of gross income) for those earning 100–200 percent of the average wage.

3. Data

The unit of observation in the analysis is at the locality-year level. Locality-level data are constructed by aggregating confidential administrative individual records from the Israel Tax Authority and the Population Registry. The full sample includes all salaried workers who lived in localities where locality-based tax credits were in effect during 2016 – 2019, and a 10-percent random sample of employees from the rest of the country. For each individual, a panel is constructed for the years they were employed, including their past and concurrent income and residential location.[11]

[11] In years when there is no employer report for the worker, we lack economic or demographic details about the individual, but I can impute the worker's residential location using past and future observations and/or the spouse's location. In such cases, I assume the annual income was zero, with the caveat that the person may have had another source of income as a self-employed worker. This caveat also applies to workers with

The dataset matches individual income tax records (from employer reports to the Tax Authority) with demographic characteristics from the Population Registry and similar details for spouses, if the worker is registered as married. The study period spans from 2000 to 2019, although data availability is limited to selected years.[12] The sample excludes career soldiers, kibbutz members, non-Israeli citizens, and workers under the age of 18. I also exclude households with a pre-benefit tax liability of zero, because they are not the target audience of this policy, and households in the 99th percentile, whose income may distort average income patterns in cells with few households.

The location of residence is determined by cross-referencing two sources: the Population Registry and the employer's report. Both sources have limitations that may introduce measurement errors. The Population Registry, while administrative, historically required bureaucratic effort to update addresses, which may have delayed or prevented updates following a change of residence (though this process has since been digitized and simplified). Conversely, the employer-reported address is a textual variable that required extensive data cleaning to convert locality names into codes for database merging. An additional limitation may arise from employees' incentives not to report address changes to employers, particularly if leaving benefited localities, in order to continue receiving tax credits. However, since the 2015 amendment, in order to receive the credit, a worker must provide the employer or the Tax Authority with proof of residence using a form signed by the local authority.[13]

I define an "in-migrant" as someone who has moved to a different locality from where they resided in the previous period. This is determined by comparing their current locality with their previous one. Similarly, an "out-migrant" is someone who will move to a different locality in the next period, identified by comparing their current locality with their future one. These definitions help us track migration patterns over time, even when data are missing.

Each locality is matched with ancillary data on the statutory benefit rate from Tax Authority documents, as well as a range of social and economic characteristics of local

recorded salary income who may also work as self-employed, but such earnings are not part of the data provided to us by the Tax Authority.

[12] The available years are: 2000, 2001, 2003, 2004, 2006, 2007, 2010, 2013, 2015, 2016, 2018, and 2019.

[13] Sensitivity tests confirmed that the results are robust to the source used to define the worker's place of residence.

authorities. I restrict my population of localities to those within a 60-kilometer travel distance of a locality in the treatment group. Below this cutoff, most excluded localities are in the Central and Tel Aviv regions, and all localities in the Northern and Southern regions remain in the sample. It should be stressed that although certain localities are not part of the estimation, individual movers from the excluded localities and their spouses are still included in the migration flows if they moved to or from any locality that is treated or located at most 60 kilometers from a treated locality.[14]

Tax Authority data do not explicitly indicate the actual amount of locality-based benefits granted to taxpayers. For employees who claimed the benefit through their employer, there is an indication of the statutory benefit rate to which they are entitled, but not the actual credit, namely, the effective benefit rate.[15] Additionally, the benefit can only be claimed after 12 consecutive months of residence in the locality, and eligibility is retroactive. Since tax filing is not mandatory for most salaried workers in Israel, a tax refund application must be submitted to the Tax Authority, which is not recorded in the current database. To calculate pre-credit tax liability and the effective benefit amount, I built a tax calculator that computes each employee's tax liability as a function of their annual income, national tax credits, and place of residence.

Table 2 provides descriptive statistics for the individual-level data. The data include 3.65 million observations over eight nonconsecutive years from 2006 to 2019. These observations involve 866,037 unique individuals, representing the entire targeted population of salaried workers in Israel with household labor earnings high enough for locality-based credits to be effective.[16] Residents of previously and newly eligible localities account for 12 percent of the sample; residents of untreated localities in the peripheral regions (South, North, and Judea and Samaria) account for another 19 percent of the data; and the rest live in the central regions of Israel.

[14] In 2004, a comprehensive reform was implemented for the lists of localities eligible for tax benefits, canceling benefits for many localities. I therefore refer to localities by their benefit eligibility between 2006 and 2019.

[15] See, for example, the average worker in Figure 1, whose average tax rate is 5 percent and who lives in a locality with a 20 percent benefit rate. Their employer's report to the Tax authority will show a 20 percent benefit rate, but the effective benefit rate will be only 5 percent.

[16] Individuals from the eligible localities are sampled with probability 1, and individuals from other localities are sampled with a 10 percent probability. Weights are based on the inverse probability of being sampled.

Using changes in residential locations, I document roughly 271,000 representative moves over the years, with an average travel distance between origin and destination of 44 kilometers and a median distance of 21 kilometers. The periodic moving rate is about 8 percent, slightly higher than the actual moving rate because some years represent a cumulative flow of two or three years. Appendix B presents an identical table for the full sample of households (including those with total household earnings below the tax threshold). The comparison of Table 2 and Table B.1 indicates that both groups are similar, except for their income and tax liability levels.

Table 2: Summary Statistics

	Mean	Sd	Min	Max	N
Age	43.9	13.8	18	99	3,641,855
Married	0.80	0.40	0	1	3,651,918
# kids (0-17 years)	1.2	1.4	0	6	3,651,918
Female	0.46	0.50	0	1	3,651,918
Household Monthly taxable income (NIS, 2019 prices)	20,342	15,255	0	117,829	3,645,321
Household monthly tax liability (pre locality-based credits, NIS, 2019 prices)	2,903	4,558	0	48,685	3,645,321
Worker's effective average tax rate (% of taxable income)	9.5	7.7	0	42	3,645,321
Household average tax rate (pre locality-based credits, % of taxable income)	9.0	7.9	0	42	3,645,321
Household effective average tax rate (% of taxable income)	7.6	8.2	0	42	3,638,475
Resident of an Arab locality	0.10	0.30	0	1	3,651,918
Moved into a locality	0.08	0.27	0	1	3,561,536
Moved out of a locality	0.07	0.25	0	1	3,031,871
Origin-destination locality travelling distance of movers (km)	43.8	58.5	0.4	485	271,032
Resident of previously eligible localities	0.06	0.24	0	1	3,651,918
Resident of newly eligible localities	0.06	0.24	0	1	3,651,918
Resident of non-eligible localities in North/South/Judea and Samaria	0.19	0.39	0	1	3,651,918
Resident of Central regions (TLV, Center, Haifa, Jerusalem)	0.69	0.46	0	1	3,651,918

Notes: The table reports summary statistics for individual salaried workers included in the sample. The full sample comprises all salaried workers who resided in localities where locality-based tax credits were in effect during 2016–2019, along with a 10 percent random sample of employees from other areas. Statistics are presented only for workers whose household tax liability without locality-based credits was positive.

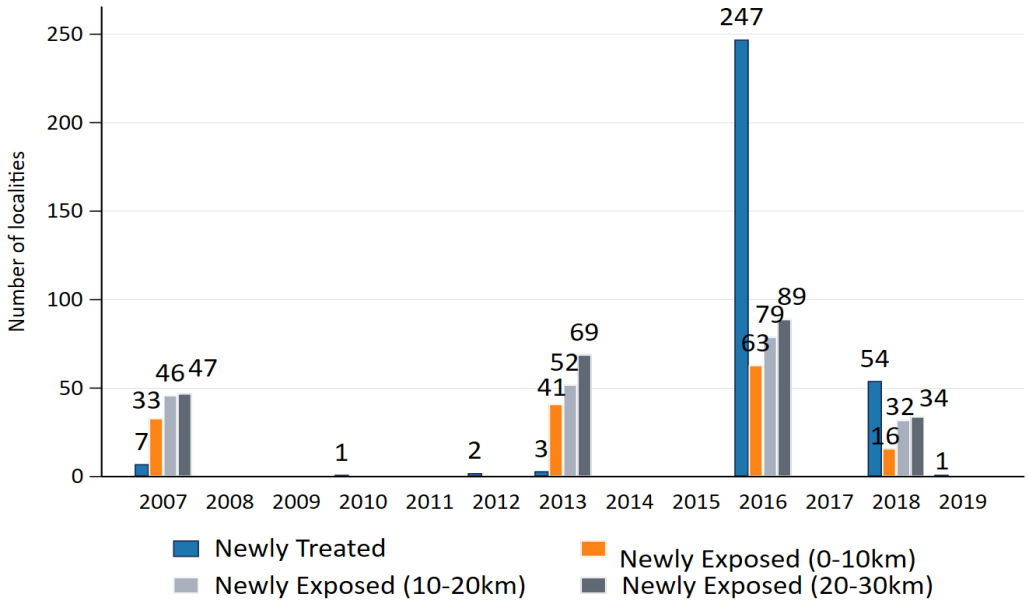
4. Empirical Strategy

To estimate changes in internal migration patterns, this section will assess the overall changes in migration flows of salaried workers and their spouses to and from localities that became eligible for locality-based tax credits.

Two econometrically challenging issues are common across place-based policy evaluation studies. First, the spatial character of the benefits, clearly visible in Figure 2, may lead to spillover effects of the treatment onto nearby control units – making them "exposed" to nearby treatment, even though they are not treated. Such spillovers may be harmful if the benefits are sufficiently high to shift the equilibrium and attract high-earning residents from neighboring untreated localities into the treated localities.

Second, while the latest reform marks a significant change in the distribution of local tax credits, there have been several minor changes to the list of eligible localities throughout the research period (Figure 4), which I account for using a staggered difference-in-differences framework.

Figure 4: The Rollout of Tax Credits to Localities



Notes: The figure presents the annual number of localities in the estimation sample that changed their status in terms of treatment (becoming eligible for locality-based tax credits) or exposure to spillovers (being within a 10-, 20-, or 30-km travel range of a treated locality).

To address potential bias in estimating the treatment effect, I adopt the identification strategy proposed by Butts (2023). To capture spatial heterogeneity in spillover effects, I construct three concentric rings around each treated locality, with travel distances of 0–10

km, 10–20 km, and 20–30 km, representing decreasing levels of spillover exposure, from strongest to weakest. I implement this using the packages *did2s* (Butts, 2021) and *did_imputation* (Borusyak et al., 2024) to perform both DiD and event-study estimations that apply the "imputation-based" methodology developed by Gardner (2021) and Borusyak et al. (2024).

The baseline estimation of both treatment and spillover effects uses a two-stage procedure that addresses the staggered nature of the studied policy:

First, the dependent variable is regressed on time and unit fixed effects, as well as other covariates, using only observations that are either never or not yet treated or exposed to spillovers:

$$Y_{it}(\mathrm{D_{it}} = 0, \mathrm{Sarea_{it}} = 0) = \theta_i + \theta_t + X_{it}\beta + \varepsilon_{it} \quad (1)$$

where Y_{it} are various outcome variables for locality i, $\theta_i \; and \; \theta_t$ are unit and year fixed effects, and X_{it} is a vector of time-varying covariates at the municipality/locality level.[17] Second, potential outcome $Y_{it}(0)$ is imputed for all observations using the estimated parameters:

$$\hat{Y}_{it}(0) = \hat{\theta}_i + \hat{\theta}_t + X_{it}\hat{\beta} \quad (2)$$

Then the residuals are generated for the entire sample (including the post-treatment period), creating an independent variable that represents the deviation of the outcome from its counterfactual prediction, which is considered the potential outcome absent treatment:

$$\widehat{\delta}_{it} = Y_{it} - \hat{Y}_{it}(0) \quad (3)$$

In the second stage, the residual $\widehat{\delta}_{it}$ is regressed on treatment and spillover dummy variables:

$$, \widehat{\delta}_{it} = \tau \mathrm{D_{it}} + \textstyle\sum_r \gamma_r \mathrm{Sarea}^r_{\mathrm{it}} + \epsilon_{it} \quad (4)$$

where D_{it} is a binary treatment dummy that switches on once the locality becomes eligible for local tax credits, and $\mathrm{Sarea}^{\mathrm{r}}_{\mathrm{it}}$ is a binary spillover dummy that takes the value 1 if a locality has at least one treated locality within a specified distance range r = 1, 2, or 3. Specifically, r = 1 corresponds to 0–10 kilometers, r = 2 to 10–20 kilometers, and r = 3 to 20–

[17] Locality ethnicity–time trend, region-specific time trend, locality size group- trend, lagged proportion of children under 18, lagged socioeconomic index cluster, local municipality tax revenue per capita, local spending on education per capita, log number of residential building completions, and distance to nearest train station.

30 kilometers.[18] This maps four levels of geographic proximity to the treated units: 3 within the exposure distance and one outside the spillover range, at 30–60 kilometers.[19] This division into rings around the treated unit approximates the exposure intensity of control units to the treatment. The treatment and spillover dummy variables are mutually exclusive, i.e., a locality can either have $\mathrm{D_{it}} = 1, \mathrm{Sarea_{it}^{k}} = 1,$ or zero. This way, τ can be interpreted as identifying the total effect, equal to the direct treatment effect on the treated units as well as the spillover effect on the treated units from nearby treated localities, γ_1 is the indirect effect on untreated localities that are in the nearest ring (r=1, in the case of 0-10 kilometers) to the treated localities, and γ_2 represents the effect on localities in r2 (10–20 kilometers), etc. The hypothesis is that $|\gamma_3| < |\gamma_2| < |\gamma_1|$, since these units are more distant. ϵ_{ist} are standard errors clustered at the natural region level,[20] and are calculated taking into account that this is a two-stage process as proposed by Gardner (2021). I limit the post-treatment period to the first four years following the onset of treatment or exposure to capture medium-term effects. This focus is motivated by the fact that the years after 2020 were marked by numerous economic and geopolitical shocks, which may have temporarily influenced households' relocation decisions.

In a dynamic difference-in-differences specification with one ring, the estimation takes the following form:

$$, \hat{\delta}_{it} = \sum_{j=-10}^{-2}(\tau_{\mathrm{j}} D_{\mathrm{it}}^{\mathrm{j}} + \gamma_{\mathrm{j}}\mathrm{Sarea_{it}^{j}}) + \sum_{k=0}^{3}(\tau_{\mathrm{k}}\mathrm{D_{it}^{k}} + \gamma_{\mathrm{k}}\mathrm{Sarea_{it}^{k}}) + \epsilon_{it} \quad (5)$$

where the first two groups of estimated coefficients ($\tau_{\mathrm{j}}\ and\ \gamma_{\mathrm{j}}$) represent the pre-treatment trends, and the last two groups of coefficients ($\tau_{\mathrm{k}}\ and\ \gamma_{\mathrm{k}}$) represent the estimated effect of being treated or exposed to spillovers for k periods.

Since there is a big group of localities that were "always treated", there is also a big group of localities that were "always exposed", many of which switched into treatment in the studied reform. In such a case, both always treated and always exposed should not be part of the estimation.

[18] Distance is calculated using the shortest route between the centroids of two localities, excluding dirt paths, toll roads, and routes in the Palestinian territories (zones A and B).

[19] I limit the control group to 60 kilometers to strengthen the parallel trends assumption, excluding the most central localities in Israel, which may have very different growth trajectories.

[20] CBS uses Natural region codes as 3-digit identifiers in its geographic coding system. It is bigger than a locality but smaller than sub-district.

In all specifications, I exclude the "always treated" localities from the estimation. Nevertheless, excluding the "always exposed" localities from the estimation narrows the sample of newly treated units as of 2016 by about 2/3 because many of them were in proximity to a few eligible localities. These localities are also concentrated in specific areas, such as the Golan Heights or the eastern part of the country (Negev and southern Mt. Hebron). Although this estimation is more consistent in the econometric sense, it raises questions about its internal validity.

Because exposure to spillovers was relatively stable and low for an extended period in many of these localities, the 2016 eligibility expansion marked a significant change in their exposure to treated localities. To retain a larger part of the sample, I use a narrower definition of exposure, following the procedure below:

a. For each locality i within a 60-kilometer travel range of any actively eligible locality, I create an annual exposure index: $Exposure_{it} = \sum_{j \neq i} \frac{D_{it}}{d_{ij}}$, which is the number of eligible localities at least 60 kilometers away, each weighted by the inverse of the distance from locality i.
b. I define the minimal exposure level below which exposure is ineffective, based on the maximal exposure level in localities more than 30 kilometers from any treated locality.
c. I define the minimal change in exposure level above which exposure begins, based on the minimal change in exposure in 2016 ($\Delta Exposure_{it} = 0.4$, 0.3, and 0.1 for localities within 10, 20, and 30 km, respectively).

The result of the procedure is illustrated in the following example. Suppose there is a rural locality in the Beit She'an area. This locality has been exposed to a treated locality since at least 2006, but there were no other treated localities within a 30 km range, making exposure relatively weak. According to the narrower definition of exposure, for $Sarea_{it}^{1}$ to be turned "ON", three conditions must be met:

(1) There is a treated locality within a 10-kilometer travel distance.
(2) Exposure index $Exposure_{it}$ is higher than the maximal exposure index of control units (30–60 kilometer ring) for that year.
(3) (3) There has been a change in exposure (due to expanded benefits) that is greater than 0.4.

The above procedure ensures that exposure is effective for most neighboring localities in 2016.

5. Results

5.1. Number of Movers

The first analyzed outcome is the number of eligible employees and their spouses.[21] This is measured using $n_{it} = \ln(N_{it})$: the log of the number of workers entering or leaving locality *i* between years *t-h* and *t*.[22] Workers and their spouses are weighted by the inverse of their sampling probability.

Table 3 presents the results of Equation (4), comparing flows between treated and nearby localities with more distant untreated localities serving as control units. The first column in each part (1 and 5) is a naïve DiD estimate that assumes the SUTVA condition, i.e., no spillover effects on the control units. Under this assumption, the estimated treatment effect is a 23 percent (0.21 log-point) increase in the annual inflow of employees and a slight increase in the number of out-movers, which is insignificantly different from zero.

In the following columns, I gradually add "spillover rings", leaving only more distant localities in the control group.[23] Adding dummies for the 0–10, 10–20, and 20–30 rings gradually increases the magnitude of the estimated treatment effect on flows. The estimated effect on outflows from treated localities is small. However, it rises to 14 percent under the two-ring specification (column 7), suggesting a possible effect on composition: given supply constraints, some eligible households leave, and others enter and replace them. Estimated spillover effects on neighboring localities are minor and not statistically different from zero for localities within a 0–20 kilometer travel distance.

The extended specification presented in columns 4 and 8 yields larger estimated inflows and insignificant outflows in treated and exposed localities – a result that does not align with the previous columns. While expanding the estimation to include more distant rings may improve the estimator's consistency, it also introduces trade-offs. Specifically, it excludes a greater number of localities from the analysis because they are classified as "always exposed",

[21] Defined as individuals who are part of a household with a positive tax liability before the application of locality-based tax credits.

[22] Some years capture cumulative flows over three years, while others cover one or two years. This is accounted for in the year fixed effects. h = 1, 2, or 3, depending on t and the gap in the data.

[23] In Column 2, localities more than 10 km away are considered controls. In Column 3, this applies to localities more than 20 km away, and in Column 4, it applies only to localities between 30 and 60 km from any treated locality.

and it relies on comparisons between treated localities and more distant control units. This may undermine the plausibility of the parallel trends assumption.

Table 3: Effect on Flows of Workers: Treated and Neighboring Localities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Inflows				Outflows			
	log of Movers (Sampled Workers + Spouses)							
Treated	0.206***	0.225***	0.275***	0.366**	0.040	0.050	0.140**	-0.224
X Post	(0.040)	(0.051)	(0.066)	(0.164)	(0.049)	(0.050)	(0.065)	(0.154)
0-10km		0.056	0.088	0.055		-0.001	0.038	-0.123
X Post		(0.076)	(0.088)	(0.094)		(0.060)	(0.084)	(0.093)
10-20km			0.045	0.006			0.118	-0.120
X Post			(0.078)	(0.106)			(0.085)	(0.076)
20-30km				0.018				-0.162
X Post				(0.077)				(0.106)
Counterfactual of treated units (level)	24.7	26.4	24.3	24.0	20.7	23.1	20.4	28.5
observations	6,279	6,078	6,042	5,844	6,151	5,954	5,920	5,728
Localities	850	850	846	841	848	848	844	836

Notes: The table presents the results of estimating Equation (4), where the dependent variable is either the inflow or the outflow measured as the log number of movers to/from locality i. All estimations include locality and year fixed effects in the first stage, as well as locality ethnicity – time trend, region-specific time trend, locality size group- trend, lagged proportion of children under 18, lagged socioeconomic index cluster, local municipality tax revenue per capita, local spending on education per capita, log number of residential building completions, and distance to nearest train station. The counterfactual mean is computed using the first-stage regression presented in Equation (2). Standard errors in parentheses are clustered at the natural region level.
* p<0.1 ** p<0.05 *** p<0.01.

Given my objective of assessing potential spillover effects and considering that the median distance of moves is 21 km, I adopt the two-ring specification (Columns 3 and 6) as the baseline. In what follows, localities located 20–60 km from treated units serve as control units, while those within 0-20 km are pooled together to estimate the average spillover effect.

According to this baseline specification, the average treatment effect on treated localities is a 32 percent increase (0.275 log points) in inflows. The estimated effect on outflows is a statistically insignificant 5 percent increase, and under the 20-km ring specification, a 15 percent increase (0.14 log points). The mean counterfactual annual inflows and outflows in the first stage are 24 and 20 individuals, respectively.[24] Multiplying the estimated effect on inflows and outflows by their respective counterfactual means implies an annual average increase of 7.7 and 3.3 eligible individuals and spouses entering and leaving the average

[24] The counterfactual mean is calculated using the first-stage regression presented in Equation (2).

locality, respectively. This results in an estimated average net annual addition of 4.4 eligible workers and spouses during the first four years in which the locality qualifies for tax credits.

5.2. Composition of Movers

The policy's stated goals are not only to attract more households to the treated localities but also to attract high earners from the central regions of Israel to relocate there and to prevent high earners from leaving. I thus estimate the impact on the composition of movers by examining the mean taxable income of migrating households and the total income flow – measured as the natural log of total taxable income entering (or leaving) locality i between t-h and t. In other words, this measure weighs households by their total income. Table 4 presents income-related outcomes. The number of workers (top panel) represents changes in the number of individuals, while the mean income outcome (middle panel) represents changes in their composition. Total income (bottom panel) combines both effects.

Stayers grew by 9 percent, but the mean income of stayers did not change in response to the new credit eligibility (Column 1). A possible explanation for the increase in staying rates – despite no decline in exit rates – and the absence of a significant change in the average income of stayers may reflect three offsetting processes. First, some households with earnings below the tax threshold may have increased their income and entered the targeted group. This addition of relatively lower earners would mechanically reduce the average income of the observed group due to a composition effect, reflecting a positive change on the intensive margin. Second, households already above the threshold may have responded to the new credit eligibility by increasing their earnings, thereby raising the average income. Third, rising migration gradually added medium- to high-earning households to the stock of stayers. However, the net inflow earns less than the incumbent households on average, a channel that may lower the stayers' average income over time. The net result of these opposing dynamics is no significant change in average income or total income.

Looking at **inflows** (column 2), I find that both quantities and composition were affected in the same direction: larger inflows are also associated with higher income per migrating household, so overall income flows are amplified. I estimate that the mean household

income[25] of incoming workers in the treated localities increased by 9 percent after the localities became eligible for local tax credits, and total income flows surged by 46 percent (0.378 log points). Out-movers' mean income did not change following the eligibility change; thus, total income outflow growth was smaller than the growth in the number of exiting workers.

Net migration cannot be calculated simply by subtracting outflows from inflows because their bases differ. I also cannot use the natural log of net inflows because some are negative. I therefore use a different scale and measure changes in percentage points rather than growth rates. The net migration rate equals the total number of in-movers minus out-movers, divided by the lagged targeted population in each locality. All measures are normalized to annual terms. The net migration rate for policy-targeted individuals (Column 4) is estimated to be 1.6 percentage points higher in the treated localities, where the counterfactual mean stood at 1.9 percent during the post-treatment period. Alongside an improvement in the composition of incoming households relative to outgoing ones, this led to a 1.3 percentage point increase in the net income inflow rate – measured as total net income added to the locality divided by the total lagged income of the existing population – compared to an average net income inflow rate of 1.5 percent during those years. Neighboring localities did not experience improvements in either their net migration rates or their net income flow rates.

[25] The income measure is based on the previous period's income to avoid endogeneity of wages due to relocation. It is the recorded household income in year t-h. If the household is missing in that specific year but the location can be imputed, the household's lagged income is zero.

Table 4: Treatment and Spillover Effects on Composition of Migration

	(1) Stayers	(2) Incoming	(3) Exiting	(4) Net migration rate
Log Number				
Treated × Post	0.084**	0.275***	0.140**	1.60***
	(0.032)	(0.066)	(0.065)	(0.60)
0-20 km Neighbors × Post	0.016	0.061	0.089	0.45
	(0.021)	(0.066)	(0.077)	(0.46)
Counterfactual of treated units (level)	675	24.3	20.4	1.86
Observations	6,371	6,042	5,920	6,379
Localities	850	846	844	850
Log Mean Income				
Treated × Post	-0.012	0.084***	-0.005	
	(0.024)	(0.031)	(0.040)	
0-20 km Neighbors × Post	-0.024*	0.004	-0.002	
	(0.015)	(0.023)	(0.027)	
Counterfactual of treated units (level)	226,299	166,848	192,508	
Observations	6,370	6,025	5,920	
Localities	850	846	844	
Log Total Income				
Treated × Post	0.060	0.378***	0.103	1.32**
	(0.041)	(0.084)	(0.068)	(0.62)
Neighboring 0-20km × Post	-0.011	0.075	0.043	0.37
	(0.028)	(0.066)	(0.096)	(0.47)
Counterfactual of treated units (level)	66,279,740	2,078,524	2,007,656	1.45
Observations	6,366	5,915	5,743	6,376
Localities	850	844	837	850

Notes: The table presents the results of estimating Equation (4) using the narrow definition of spillover areas as those within 20 km of the newly treated locality. In column 1, the dependent variable is the log of stayers. In columns 2 and 3, the dependent variables are the logs of the incoming and outgoing numbers of workers, representing the extensive margin effect on migration; the log of average household income (measured as the sum of the sampled mover's and spouse's incomes if both are sampled and registered as married to each other), representing the composition effect; and the total income inflows or outflows (the log of the number of movers weighted by their income), representing the total effect. In column 4, the coefficients represent the effect measured in percentage points on the net migration rate and the net income flow into locality i (net addition divided by lagged total population or total income). All estimations include locality and year fixed effects in the first stage, as well as locality ethnicity – time trend, region-specific time trend, locality size group-trend, lagged proportion of children under 18, lagged socioeconomic index cluster, local municipality tax revenue per capita, local spending on education per capita, log number of residential building completions, and distance to nearest train station. The counterfactual mean is computed using the first-stage regression presented in Equation (2). Standard errors in parentheses are clustered at the natural region level.
* p<0.1 ** p<0.05 *** p<0.01.

In Table 5, I extend the same estimation strategy to examine migrants' origin and destination patterns. The average distance traveled by in-movers did not change significantly in treated localities (Column 1). Inflows from central regions and from ineligible localities in the peripheral regions, which together constitute about half of total inflows to newly eligible localities, increased more slowly than the overall rise in inflows (Columns 2 and 3). In contrast, inflows from previously eligible localities[26] and from other newly eligible localities grew at a much faster rate (columns 4 and 5).[27] These patterns suggest that a larger share of additional migrants originated from other peripheral, credit-qualifying localities. Taken together, the results imply that although the policy increased the volume of migration into treated localities, it did not expand their geographic reach.

A similar pattern emerged for out-movers to central regions. The number of movers to the Center, as well as to other groups of peripheral localities, did not change, even though total outflows increased, and the average distance to destination remained largely unchanged. Overall, there is no evidence of changes in the composition of outflows.

These results suggest that the policy change had mainly a local effect – attracting high-earning households to treated localities within the same regions. Untreated neighboring localities also saw faster growth in workers from the central areas, likely due to positive spillover effects. However, because overall inflows did not increase, this implies that fewer households from the peripheral areas entered these untreated localities, as they were diverted to credit-qualifying localities. I also tested whether demographic characteristics of migrants changed because of the benefits, but found no effects for outcomes such as the main earner's age, marital status and the number of children in the migrating household.

[26] Localities that were eligible for benefits prior to 2016, including those omitted from the estimation sample due to their "always treated" status.

[27] As noted previously, I include only observations with a positive inflow or outflow – in this case, inflows of workers from central regions – thereby focusing on localities where migration flows are sufficiently large to allow meaningful analysis.

Table 5: Treatment and Spillover Effects on Origin and Destination of Movers

	(1)	(2)	(3)	(4)	(5)
	log Distance (km)	Central Regions	Ineligible localities in the peripheral regions	Previously eligible localities Regions	Newly eligible localities Regions
	From origin		log Inflows from		
Treated × Post	-0.018	0.219***	0.148*	0.387***	0.393***
	(0.047)	(0.067)	(0.085)	(0.122)	(0.109)
0-20 km Neighbors × Post	0.005	0.143**	0.089	0.128	0.014
	(0.033)	(0.066)	(0.085)	(0.118)	(0.065)
Counterfactual of treated units (level)	67.3	9.2	7.3	6.2	10.0
Observations	6,041	4,954	3,613	2,790	3,243
Localities	846	811	765	683	704
	To destination		log Outflows to		
Treated × Post	-0.069	0.085	-0.034	0.072	0.150
	(0.043)	(0.064)	(0.069)	(0.072)	(0.094)
0-20 km Neighbors × Post	0.005	0.053	-0.058	0.015	0.004
	(0.033)	(0.054)	(0.075)	(0.054)	(0.080)
Counterfactual of treated units (level)	56.7	6.8	7.8	8.3	11.7
Observations	5,919	4,279	2,867	2,715	3,405
Localities	844	771	692	699	715

Notes: The table presents the results of estimating Equation (4), with spillover areas defined as those within 20 km of the newly treated locality. Distance is measured in kilometers from the mover's last known locality. Central regions include Tel Aviv, Central, Jerusalem, and Haifa. All estimations include locality and year fixed effects in the first stage, as well as locality ethnicity – time trend, region-specific time trend, locality size group – trend, lagged proportion of children under 18, lagged socioeconomic index cluster, local municipality tax revenue per capita, local spending on education per capita, log number of residential building completions, and distance to the nearest train station. The counterfactual mean is computed using the first-stage regression presented in Equation (2). Standard errors in parentheses are clustered at the natural region level.
* p<0.1 ** p<0.05 *** p<0.01.

5.3. Heterogeneous effects

Figure 5 presents a comparison of the baseline estimates when the treatment dummy is interacted with a group dummy in the second stage, using the following equation:

$$\hat{\delta}_{igt} = \sum_{g=1}^{G} \tau_g \mathrm{D}_{\mathrm{igt}} * \mathrm{I}\{\mathrm{i} \in \mathrm{g}\} + \sum_{g=1}^{G} \gamma_g \mathrm{Sarea}_{\mathrm{igt}} * \mathrm{I}\{\mathrm{i} \in \mathrm{g}\} + \theta_{\mathrm{g}} \sum_{g=2}^{G} I_{ig} + \epsilon_{igt} \qquad (6)$$

where $\mathrm{I}\{\mathrm{i} \in \mathrm{g}\}$ is a group dummy for locality that takes the value one if locality *i* belongs to group *g*. Equation (6) yields the average effect for each group. An alternative estimation

would be to estimate the difference in impact between a baseline group and other groups.[28] Using the latter approach, the effects are not statistically different across groups, but this may also be due to limited power because some groups are relatively small.

Panel A of Figure 5 presents the estimated net migration effects for each group on two outcomes: net migration rates (dark blue) and net income inflow rates (light blue).

When dividing the sample by the primary ethnicity of the locality's residents,[29] the results show that the estimated net effects are larger and exhibit lower variance in Jewish localities. On average, Jewish localities experienced a 1.8 percentage point increase in net migration, compared with a 0.5 percentage point statistically insignificant increase in Arab localities.

Panels B and C show that inflows to and outflows from Jewish localities are very similar to the average estimates. Nevertheless, Arab localities saw a slightly positive increase in inflows, but exit flows from the same localities must have grown, leaving the net migration rate unchanged. The relatively low effect on inflows and the lack of a significant impact on outflows among the Arab population can be attributed to cultural factors and housing-market frictions that reduce the frequency of relocation.[30]

When dividing the sample by region, the effect of net migration rates (Panel A) is largest in The South region and the region of Judea & Samaria, which had no eligible localities during the decade preceding the 2015 amendment, while the Northern region has the smallest net effect which is only marginally significant. The compositional effect – reflected in net income inflow rates – is significantly stronger in Southern localities, indicating a substantial overall impact when accounting for both the size of net inflows and their income levels. Moreover, when comparing inflows and outflows separately (Panels B and C), the Southern region shows a more pronounced increase in inflows in both the treated and neighboring localities, but in the untreated localities this is completely offset by the growth of outflows. This pattern suggests greater mobility and population reshuffling in the Southern region compared to the

[28] This comparison is performed by running a regression of the form: $\hat{\delta}_{igt} = \tau_1 \mathrm{D}_{\mathrm{igt}} + \sum_{g=2}^{G} \tau_g \mathrm{D}_{\mathrm{igt}} * \mathrm{I}\{\mathrm{i} \in \mathrm{g}\} + \gamma_1 \mathrm{Sarea}_{\mathrm{igt}} + \sum_{g=2}^{G} \gamma_g \mathrm{Sarea}_{\mathrm{igt}} * \mathrm{I}\{\mathrm{i} \in \mathrm{g}\} + \epsilon_{igt}$.

[29] The localities are classified as Jewish or Arab localities according to the CBS definition of locality type. Mixed cities are classified as Jewish localities because the majority of residents are Jewish.

[30] See "The Residential Distribution and Socioeconomic Characteristics of Ultra-Orthodox Jews and Israeli Arabs", Bank of Israel *Annual Report* 2016, Chapter 8

Northern region. Analyzing heterogeneity of effects within the subsample of Jewish localities (Appendix C) reveals similar regional differences.

Small localities (with fewer than 10,000 residents) make up the majority of the sample. Among the newly eligible group, there are only three large Jewish localities – Zefat, Karmiel, and Tiberias – and sixteen large Arab localities. The interaction between the treatment and the locality size dummies shows that the average estimates are driven primarily by smaller localities, where the average effect is approximately twice as large as in large localities. Although some large localities also experienced positive effects on the extensive margin, these effects were smaller. These effects exhibit greater variance, rendering the point estimate of inflows statistically significant only at the 90 percent confidence level.

High- and low-benefit localities[31] experienced similar improvements in net migration and income inflows. Low-benefit localities saw a somewhat larger increase in the number of migrants, while high-benefit localities experienced a greater improvement in the income profile of migrants. This difference is more pronounced in the Jewish subsample, where net migration rates did not change in the high-benefit localities but net added income rate did increase (Appendix figure C.1).

This pattern is expected, as the higher benefit rate is designed to compensate households for poorer location and socioeconomic conditions. However, it applies only to a very narrow segment of the income distribution – only about 10 percent of employees nationwide have a pre-credit average tax rate exceeding 12 percent (Figure 1). Consequently, high-benefit localities are not expected to show a more substantial quantitative effect, but they should exhibit a more pronounced effect on composition.

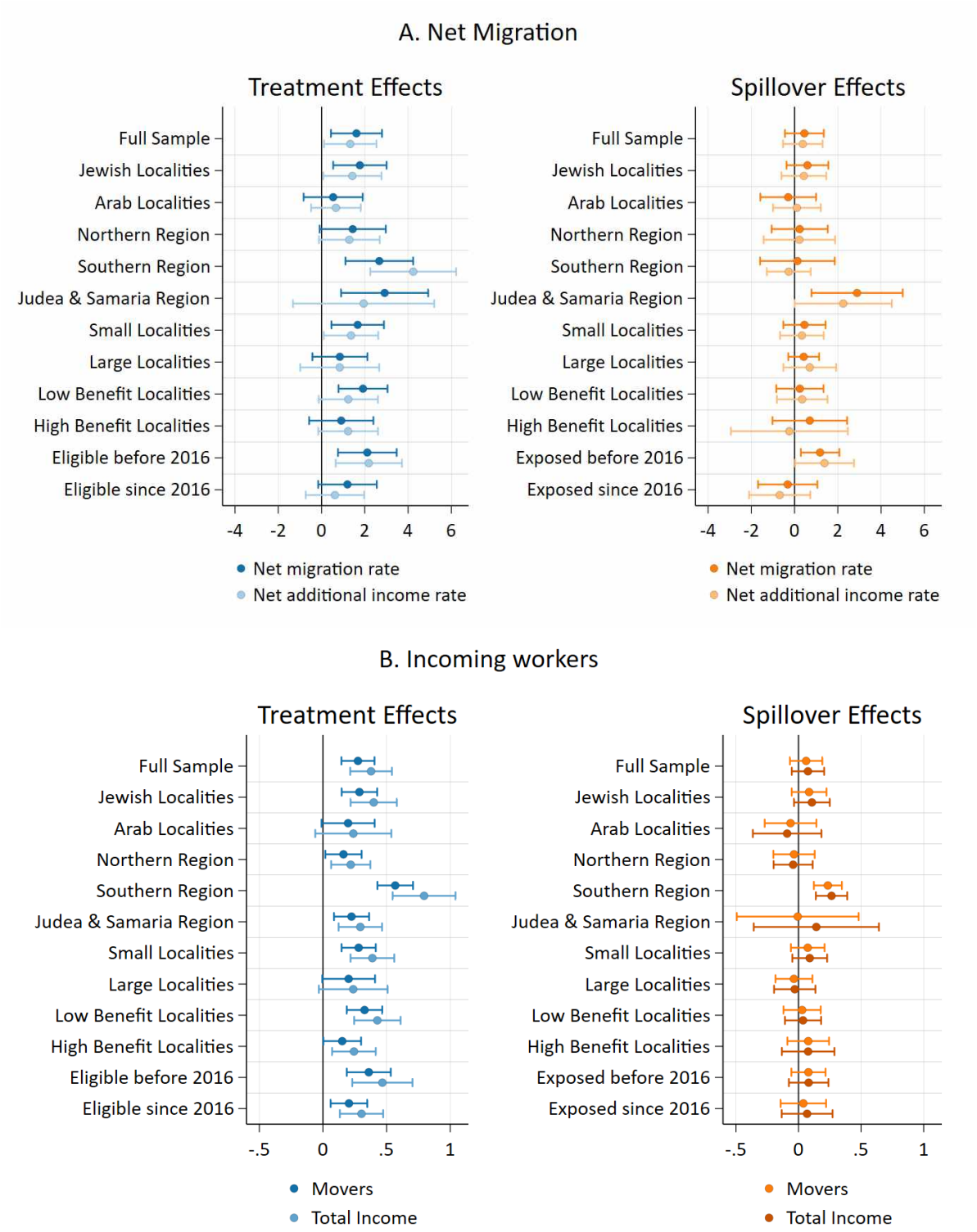
Finally, dividing the sample by the onset of eligibility or exposure – before and after the 2015 amendment – reveals a slightly larger estimated treatment effect in localities that gained eligibility earlier. During the first 4 years of eligibility, early adopters experienced a 2.1 percentage-point increase in the annual net migration rate, compared with a 1.2 percentage-point increase among those who became eligible later.[32] Moreover, the effect on net added income is very small in the later-eligible localities. The benefit's effectiveness may have

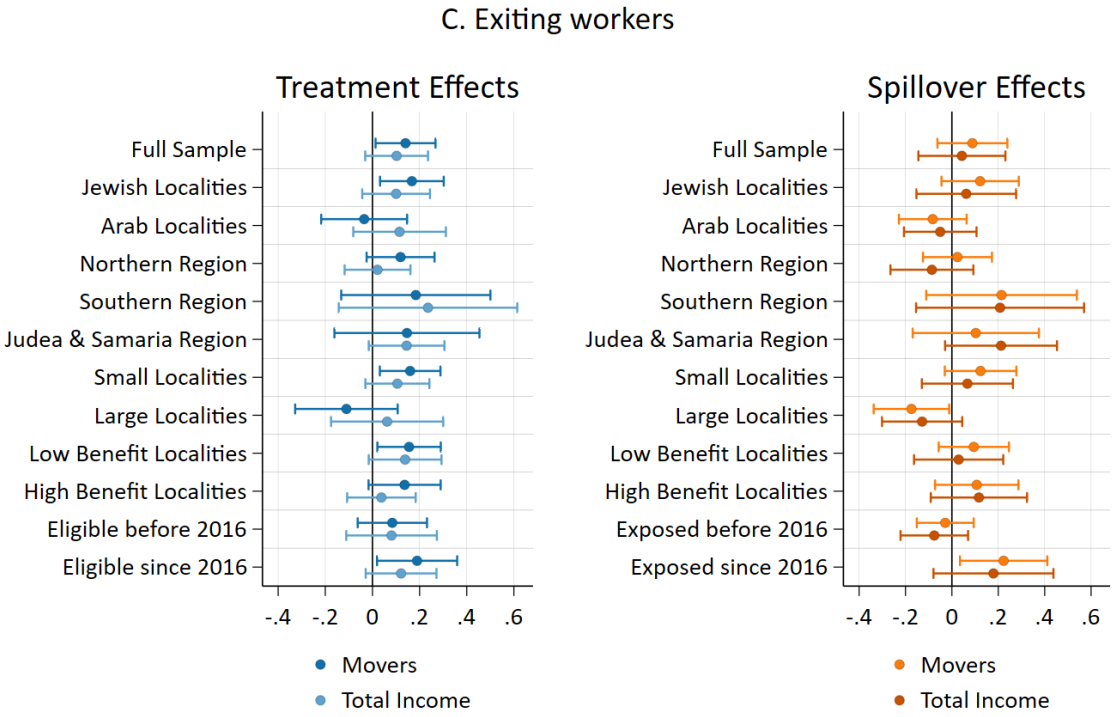
[31] I define low-benefit-rate localities as those that received a statutory benefit rate of 7–12 percent by 2019, and high-benefit localities as those that received 13–20 percent. The 12 percent cutoff is approximately the median locality.

[32] For both groups, the estimates represent the average effect over the first four years of eligibility.

diminished over time, potentially due to a nationwide decline in tax rates across the broader tax system. Additionally, the vast expansion of benefits to many new localities may have diluted the impact for each locality.

Figure 5: Treatment and spillover effects by locality type





Notes: The figures present the estimates of Equation (6) and their 95 percent confidence intervals. Arab localities are those where the majority of residents are Arab. Low-benefit localities have a statutory benefit rate of 7–12 percent, and high-benefit localities have a statutory benefit rate of 13–20 percent. Small localities have fewer than 10,000 residents. In the right panel, low/high-benefit localities refer to those to which neighboring localities are exposed. The same holds for the Northern/Southern region subsamples. Some of the exposed localities are in one of the Central regions, but within a 20 km travel distance from a treated locality.

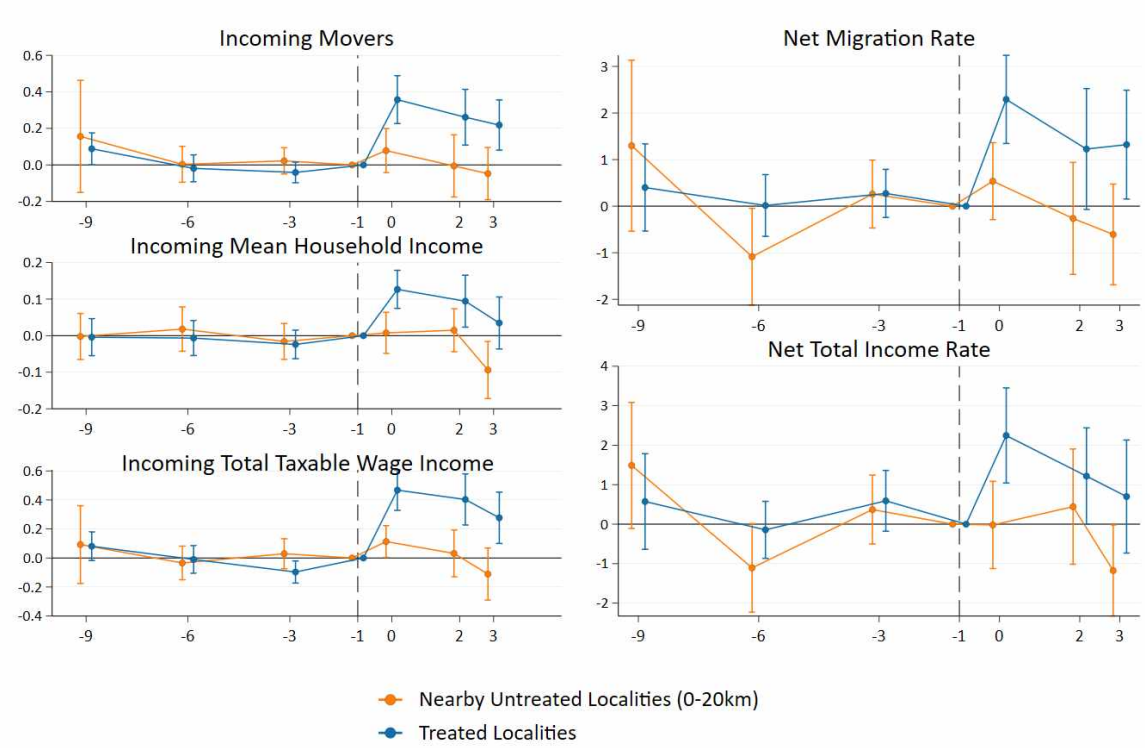
5.4. Dynamics

Figure 6 presents the results of the event study specification in Equation 5 for both incoming migration flows (left-hand side of the figure) and net migration flows (right-hand side). Both sides of the figure show similar dynamics. The blue lines represent the estimated effects on the treated localities, and the orange lines represent the estimated effects on the untreated localities within the 0–20 km range (using localities over 20 km away as controls). The expansion of benefits increased the number of incoming workers in the first four years after the eligibility expansion, with the peak occurring in the first year (t=0). The dynamic estimation shows a smaller effect over time. The increase in incoming household annual taxable income is also short-lived, with insignificant effects after the third year in the full sample and a lasting positive impact in the Jewish subsample (Appendix Figure C.2).

Finally, nearby untreated localities experienced no spillover effects during the first four years of exposure. However, those who remained exposed and did not become eligible by the end of the four years experienced adverse effects on the income profiles of incoming migrants. Specifically, the average income of new arrivals in these localities declined,

suggesting a relative disadvantage compared with newly eligible areas. The combination of a positive treatment effect in eligible localities and a slightly adverse spillover effect in neighboring non-eligible ones resulted in a cumulative gap in total income inflows between the two groups.

Figure 6: Event Study Estimations of Worker Inflows



Notes: The figures present the estimation results of Equation (5) with the same dependent variables as Columns (1) and (3) in Table 4. All displayed confidence intervals represent 90 percent confidence intervals. For further information, see notes of Table 4.

6. Discussion

6.1. Anticipation

As noted previously, the legislative process surrounding the reform drew considerable public attention. Although the benefits were formally implemented in 2016, the original plan, as outlined in the 2014 government resolution, was to adjust the rates starting in January 2015. However, due to an earlier-than-expected government transition, the legislative change was delayed and not approved until December 2015. Additionally, personal eligibility requires at least 12 months of residence in an eligible locality. Therefore, relocating in 2014–2015 was a rational decision for households anticipating the reform.

To account for potential anticipation effects, in Appendix D I conduct a robustness test by treating 2014 as the first year of treatment for localities that met the eligibility criteria outlined in the government resolution, even though they officially became eligible only in January 2016. This shifts treatment timing by two years for 182 localities among the 302 newly eligible localities in the study and for 136 newly exposed untreated localities within a 20 km radius.

The results in Appendix Table D.1 and Appendix Figure D.1 indicate that the estimated effects existed in 2015 (t=1), though to a lesser extent. The peak remains evident in 2016 (t=2), the year in which eligibility was formally in place. The pronounced short-term effect raises concerns that some of the observed moves may not reflect actual household relocations. However, because there is no corresponding adverse effect on outflows, the concern that out-migrants retained their previous address to remain eligible for benefits appears limited. Moreover, as previously noted, the reform was accompanied by a new administrative procedure designed to prevent fictitious address changes.

One remaining measurement concern is that households that had moved but had not formally updated their residential address may have done so in response to the new credit eligibility. This could explain the strong short-term effect, suggesting that the static estimate may represent an upper bound, while medium-term effects are more likely to reflect genuine behavioral changes.

Furthermore, when examining the number of incoming medium- to high-income households in nearby untreated localities, there is evidence of some positive spillover effects during the first two years of exposure. This may be attributed to anticipation of further benefit expansions – some of which materialized in the second round of eligibility expansion in 2017 and later on.

6.2. Economic significance

Although the empirical analysis finds that inflow growth rates were substantial following the introduction of locality-based credits, many of the new residents came from other eligible localities. Additionally, the baseline amounts were initially very small resulting in an annual net addition of 4.4 residents per newly eligible locality.

It is therefore important that any evaluation of the cost-effectiveness of this policy change include further analysis of the actual cumulative number of movers and the total foregone

revenue. Table 6 presents the first part of this analysis, and Table 7 incorporates the fiscal dimension.

Given the substantial heterogeneity across ethnicity and locality size, I estimate treatment effects using a regression model similar to Equation (6), interacting treatment status with locality ethnicity (Jewish or Arab) and size (small or large). I then approximate policy-induced net migration flows for each locality-year observation with a back-of-the-envelope calculation, multiplying the relevant coefficient by the lagged size of the targeted population.

Table 6 reports the aggregated results of this calculation by year. Because the main heterogeneity in the effect is along the ethnicity dimension, the decomposition between Jewish and Arab localities is shown in Panels B and C, respectively. The staggered rollout of the treatment is evident in the increasing number of treated localities between 2016 and 2019. It is also apparent that treated Jewish localities, on average, are smaller than Arab localities, as measured by the total number of eligible workers and their spouses per locality.

Counterfactual prediction suggests that, without the treatment, Arab localities would have stagnated and even experienced a decline in net migration among medium- to high earners. For these localities, the policy change appears to be the primary driver of growth, accounting for most of the increase in the stock of eligible workers and spouses. This result should be interpreted with caution, as the sample of Arab localities is relatively small and may be biased by unobserved factors not accounted for in the imputation procedure.

Using only the localities included in the estimation sample,[33] I document roughly 7,300 net moves of eligible individuals and their spouses into treated localities, of which approximately 5,485 are policy-induced net moves recorded during the four years following the expansion of benefits. This figure represents a 0.8 percent annual increase in medium-to-high-earning households across the 296 newly eligible localities – 1.1 and 0.6 percent in Jewish and Arab localities, respectively. As previously noted, this may represent an upper bound on the number of actual moves. Nevertheless, distinguishing genuine relocations from mere address updates in the data remains an empirical challenge. Nonetheless, the stricter requirement for local authorities' authentication reduces concerns about fraudulent moves.

[33] Out of 321 newly eligible localities, 25 small localities were not included in the estimation because their outcome measures were outliers in some years, and the imputation procedure could not impute their potential outcomes in the post-treatment period.

Table 6: Estimated Number of Movers to Newly Treated Localities due to Benefits

Year	Treated localities in estimation (#)	Total eligible workers and their spouses (lagged)	Net movers		Net moves due to credits	
			N	Annual rate	N	Annual rate
A. Newly eligible localities in the estimation sample						
2016	240	145,455	1,316	0.9%	2,008	1.4%
2017+2018	296	173,779	4,027	1.2%	2,060	0.6%
2019	296	188,071	1,959	1.0%	1,417	0.8%
Total			7,302	1.1%	5,485	0.8%
B. Newly eligible Jewish localities in the estimation sample						
2016	207	61,291	901	1.5%	1,028	1.7%
2017+2018	262	78,607	3,161	2.0%	1,480	0.9%
2019	262	85,201	1,467	1.7%	818	1.0%
Total			5,529	1.8%	3,326	1.1%
C. Newly eligible Arab localities in the estimation sample						
2016	33	84,164	415	0.5%	980	1.2%
2017+2018	34	95,172	866	0.5%	580	0.3%
2019	34	102,870	492	0.5%	600	0.6%
Total			1,773	0.5%	2,159	0.6%

Notes: The table presents the estimated stock of eligible residents in the newly eligible localities, their net flows into this group of localities from other previously eligible or non-eligible localities, and a calculation of net moves attributed to the expansion of locality-based tax credits. Panel A presents the total calculated results for the localities that entered the estimation, Panels B and C decompose the results by locality's primary ethnic group. The "Total" in each panel represents the cumulative sum or average annual rate for the years 2016 – 2019.

6.3.The fiscal cost distribution among residents of previously and newly eligible localities

In the 2019 state budget documents, the Ministry of Finance estimated that the value of tax benefits across all localities would total about NIS 2.3 billion per year, approximately 2.5 percent of income tax revenue from salaried and self-employed workers. It has since grown to an estimated NIS 2.7 billion per year, as recorded in the 2025 budget documents (Ministry of Finance, 2018; Ministry of Finance, 2024).

The formula that calculates the benefit rate places greater weight on peripherality indices and proximity to state borders, favoring previously eligible localities and granting them higher benefit rates. Moreover, in previously eligible localities, pre-benefit tax liability, eligibility rates, and take-up rates were higher than in newly eligible localities. This combination led to the average effective benefit per employee in a previously eligible locality being about 45 percent higher than that in a newly eligible locality. Table 7 presents the increase in the fiscal cost of benefits in previously eligible localities between 2013 and 2019, as well as the differences between veteran and newly eligible localities in 2019.

The table shows that the average benefit per employee remained similar between 2013 and 2019 in current prices. However, the number of eligible employees more than doubled, increasing the fiscal cost from approximately NIS 0.7 billion in 2013 to about NIS 1.85 billion in 2019.[34] In 2019, 63 percent of the fiscal cost benefited residents of previously eligible localities, and 37 percent went to newly eligible localities. Five percent of this was allocated to employees who moved to one of the treated localities between 2016 and 2019, similar to the corresponding rate in previously eligible localities in 2013.

Based on the sample of salaried employees, the fiscal cost of expanding benefits is estimated at NIS 704 million in 2019, with an average annual effective tax credit of NIS 9,400. Cumulatively, residents of newly eligible localities benefited from effective credits worth NIS 1.86 billion during 2016 – 2019.[35] Dividing the total expenditure by the policy-attributed cumulative addition of 5,485 individuals, as calculated in Table 6, yields a result of at least NIS

[34] This estimate is based on actual tax records in which the employer indicated the worker was eligible for the locality-based tax credit and on my calculation of the effective benefit this worker received. The estimate does not include workers who applied for a tax refund directly through the Israeli Tax Authority, an action that can occur within the 6 years following 2019. The remaining estimated forgone revenue (NIS 0.4 billion) is most likely attributable to self-employed workers and retroactive tax refund claims for previous years.

[35] Due to missing data, I impute the 2017 amount using the 2018 amount and the growth rate between 2018 and 2019.

339,300 as the total cost of attracting a targeted individual during the first four years of credit expansion. The effective annual benefit per actual recipient averaged NIS 8,500 during the same period. However, most incumbent households would have stayed even without benefits, given other considerations and idiosyncratic preferences. Since my estimated number of movers is likely an upper bound on the actual effect, this cost per net migrant may have been even higher.

Table 7: Place-Based Tax Credits and Their Fiscal Cost by Category

	Previously eligible localities 2013	Previously eligible localities 2019	Newly eligible localities 2019	All eligible localities 2019
% of workers above tax threshold	43	46	42	44
(excl. locality-based tax credits)	(49)	(50)	(49)	(50)
% of workers above tax threshold	9	11	14	13
(incl. locality-based tax credits)	(29)	(32)	(34)	(33)
Statutory tax credit rate (%)	13.93	13.8	10.6	12.14
	(3.8)	(4.1)	(3.4)	(4.1)
Average Tax Rate	3.4	3.9	3.3	3.6
(pre-locality-based credits)	(5.9)	(6.2)	(5.8)	(6)
Average Tax Rate	0.8	1	1.3	1.2
(post-locality-based credits)	(3.3)	(3.8)	(4.3)	(4.1)
Estimated take-up rate (%)	28	29	24	27
	(45)	(46)	(43)	(44)
Annual effective benefit (NIS thousand)	12	13.7	9.4	11.7
	(10.4)	(11.4)	(7.1)	(9.9)
# of salaried workers (thousand)	224.7	292.2	315.3	607.4
of which: Total benefited workers	62.9	84.7	75.7	160.4
of which: Entered in the last 3 years	7.2	11.4	10.2	21.6
of which: Migrated from ineligible localities	2.3	2.8	2.0	4.8
Estimated forgone revenue (NIS million)	750	1,180	704	1,880
of which: Entered in the last 3 years	79	150	82	230
o*f which*: Migrated from ineligible localities	29	41	19	60
Estimated forgone revenue under full take-up (NIS million)	1,030	1,590	1,020	2,600

Notes: The table is based on individual-level administrative tax records for the studied population. Tax liability is calculated using a tax calculator developed for this study, based on reported labor income, deductions, and regular personal tax credits (e.g., child tax credits). The tax threshold and average tax rates, excluding tax credits, are derived from the calculated tax liability. The tax threshold and average tax rates, including tax credits, are based on actual tax withholding as reported by the employer. The effective benefit is the difference between the pre-credit tax liability and the actual tax liability. Take-up rates represent the share of workers who were eligible for tax credits, reported residing in an eligible locality, meaning they applied for the credits through their employer.

7. Conclusion

This study provides a comprehensive, up-to-date analysis of the effects of locality-based tax credits on migration patterns among medium- to high-income households in Israel, examining both quantities and composition of movers. The evaluated policy change, which increased the number of eligible localities from 196 localities in 2015 to 517 in 2019, led to a significant growth in inflows of targeted workers to these localities, with a corresponding rise in the mean household income of incoming workers. Net migration flows doubled during the first four years of benefits. However, the total effect on the stock of medium- to high- earning residents was modest.

The study also finds that positive impacts were predominantly observed in small Jewish localities and that the effect was larger in the Southern Region and in the first few years, suggesting lower long-term effects. The fiscal analysis indicates that the expansion of tax benefits resulted in substantial costs, as a significant portion of the benefits accrued to existing residents who were not considering relocation. While increasing disposable income in remote localities may be a legitimate policy objective, the emphasis on higher-income households requires stronger justification.

Overall, while the policy achieved some of its objectives, the heterogeneous effects and substantial fiscal costs suggest the need for a more targeted approach to maximize its cost-effectiveness. To determine the optimal allocation of resources, it is crucial to compare the value-for-money of this program with that of alternative policies, such as investing in better educational services, job-creation initiatives, and public transportation improvements. This comparison would help policymakers identify the most effective strategy for achieving regional development goals.

Future research should focus on estimating the indirect economic benefits that non-eligible households may receive from the increased number of eligible households in their locality. Moreover, the data and policy changes enable a richer analysis at the individual and household levels, including estimates of structural parameters such as the elasticity of demand with respect to prices, local amenities, taxes, and job opportunities.

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Appendix A

The criteria for locality-based tax credits as of 2016

"Preferred Locality" – A locality that, as of December 31 before the tax year for which the credit under this section is granted, meets all of the following criteria:

1. It is a locality adjacent to a border or a locality whose residential areas, in whole or in part, are located north of latitude 750, south of latitude 610, or east of longitude 250, except for a locality whose residents are subject to the provisions of Section 11 of the Eilat Free Trade Zone Law (Exemptions and Tax Discounts), 1985-21.
2. Its population is less than 75,000 residents, according to data from the Central Bureau of Statistics.
3. 3. It is classified in cluster 8 or a lower cluster on the socio-economic scale, unless it is a locality adjacent to a border.
4. 4. Its establishment was approved in accordance with all laws.
5. 5. Its total score is at least 25 points.

Total Score is the weighted sum of socio-economic, peripherality, and proximity to international border points.

Socio-economic Cluster Index (1 is the most disadvantaged) *weight: 35%*	**Points**
1	100
2	90
3	80
4	60
5	30
6	15
7+	0
Adjacency to a border *weight: 25%*	**Points**
Yes	100
No	0
Peripherality Cluster Index (1 is the most peripheral) *weight: 40%*	**Points**
1 or 2	100
3	80
4	50
5	30
6	10
7+	0

Appendix B

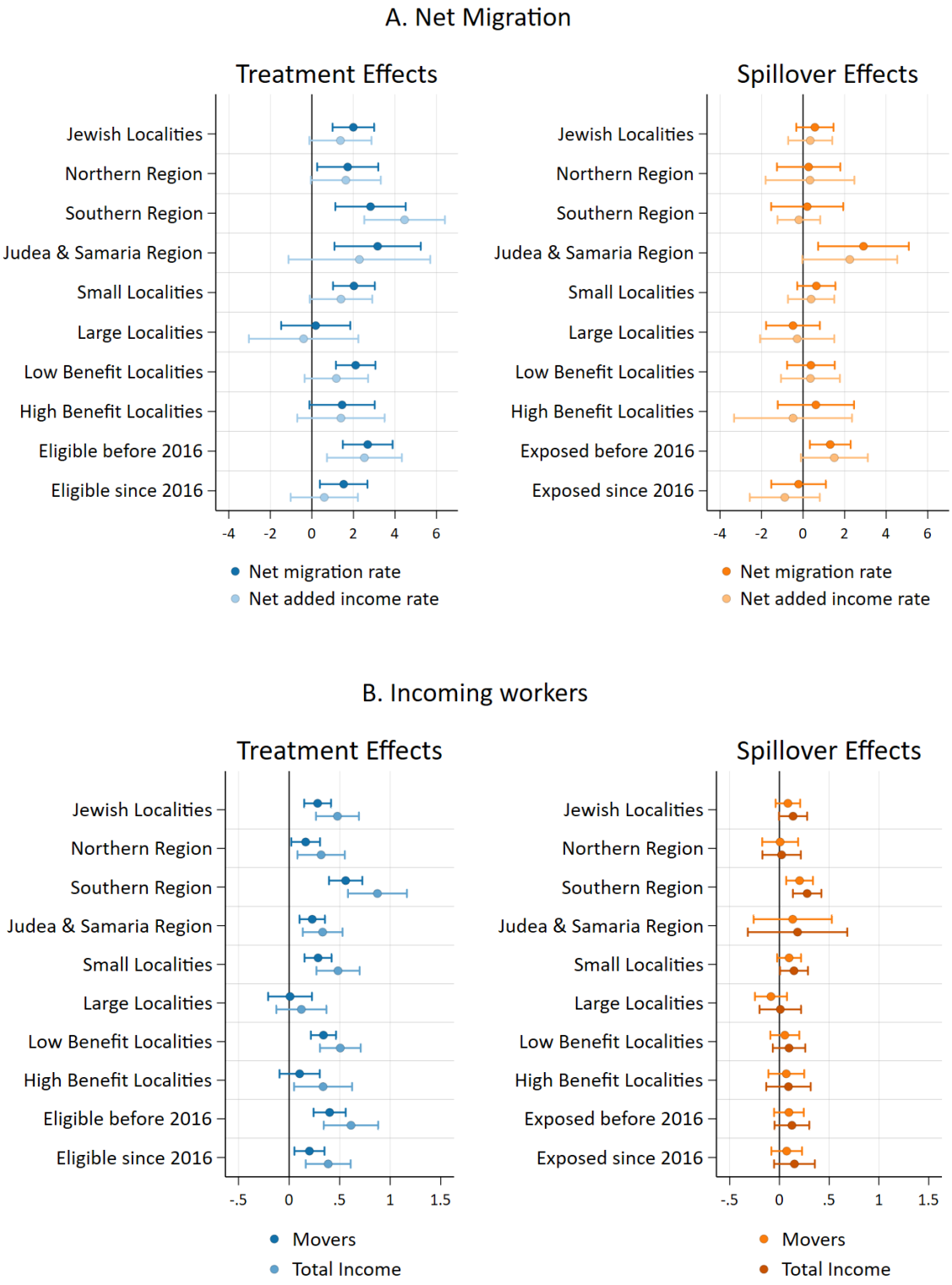
Table B.1 Full Sample Statistics (including lower-earning households)

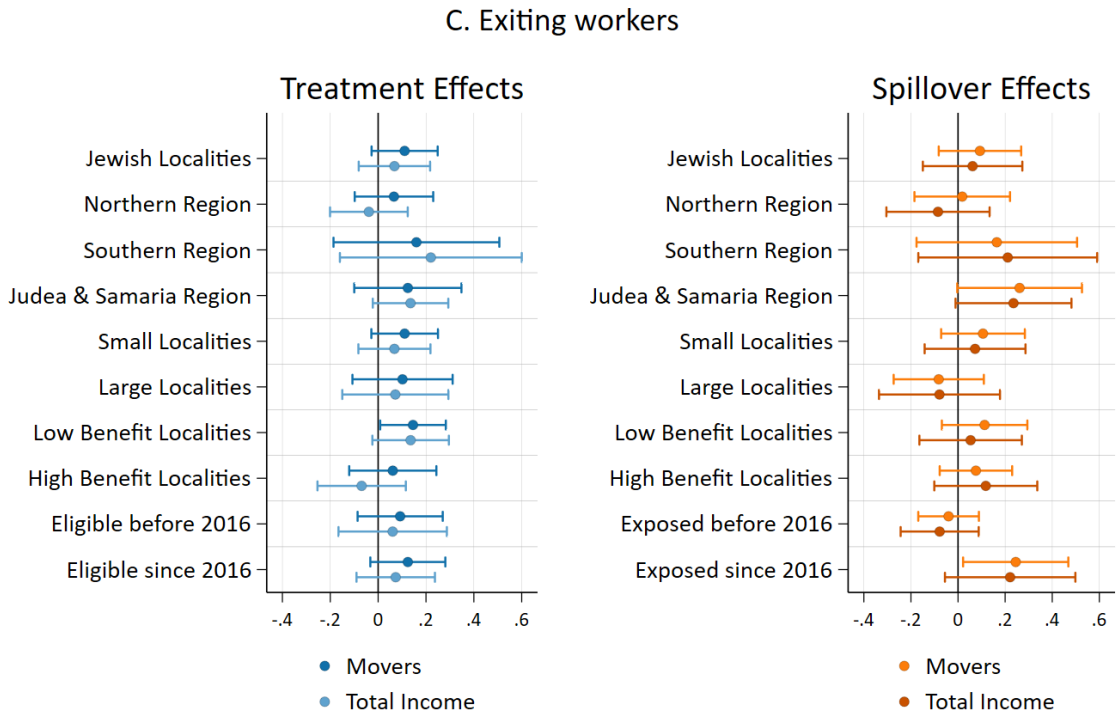
	Mean	Sd	Min	Max	N
Age	41.1	16.0	18	99	6,576,669
Married	0.70	0.46	0	1	6,598,921
# kids (0–17-year-olds)	1.03	1.44	0	6	6,598,921
Female	0.50	0.50	0	1	6,598,921
Household Monthly taxable income (NIS, 2019 prices)	13,457	14,306	0	117,829	6,592,324
Household monthly tax liability (pre locality-based credits, NIS, 2019 prices)	1,677	3,749	0	48,685	6,592,324
Worker's effective average tax rate (% of taxable income)	4.4	7.3	0	42	6,504,713
Household average tax rate (pre locality-based credits, % of taxable income)	5.6	7.5	0	42	6,519,342
Household effective average tax rate (% of taxable income)	5.3	7.5	0	42	6,519,342
Resident of an Arab locality	0.11	0.31	0	1	6,598,921
Moved into a locality	0.09	0.28	0	1	6,007,504
Moved out of a locality	0.08	0.27	0	1	5,386,111
Origin-destination locality travelling distance of movers (km)	14.3	41.8	0	485	1,842,798
Resident of previously eligible localities	0.07	0.25	0	1	6,598,921
Resident of newly eligible localities	0.07	0.25	0	1	6,598,921
Resident of non-eligible localities in North/South/JS	0.19	0.40	0	1	6,598,921
Resident of Central regions (TLV, Center, Haifa, Jerusalem)	0.67	0.47	0	1	6,598,921

Notes: The table reports summary statistics for individual salaried workers in the sample. The full sample includes all salaried workers who resided in localities where locality-based tax credits were in effect during 2016-2019, along with a 10 percent random sample of employees from other areas.

Appendix C

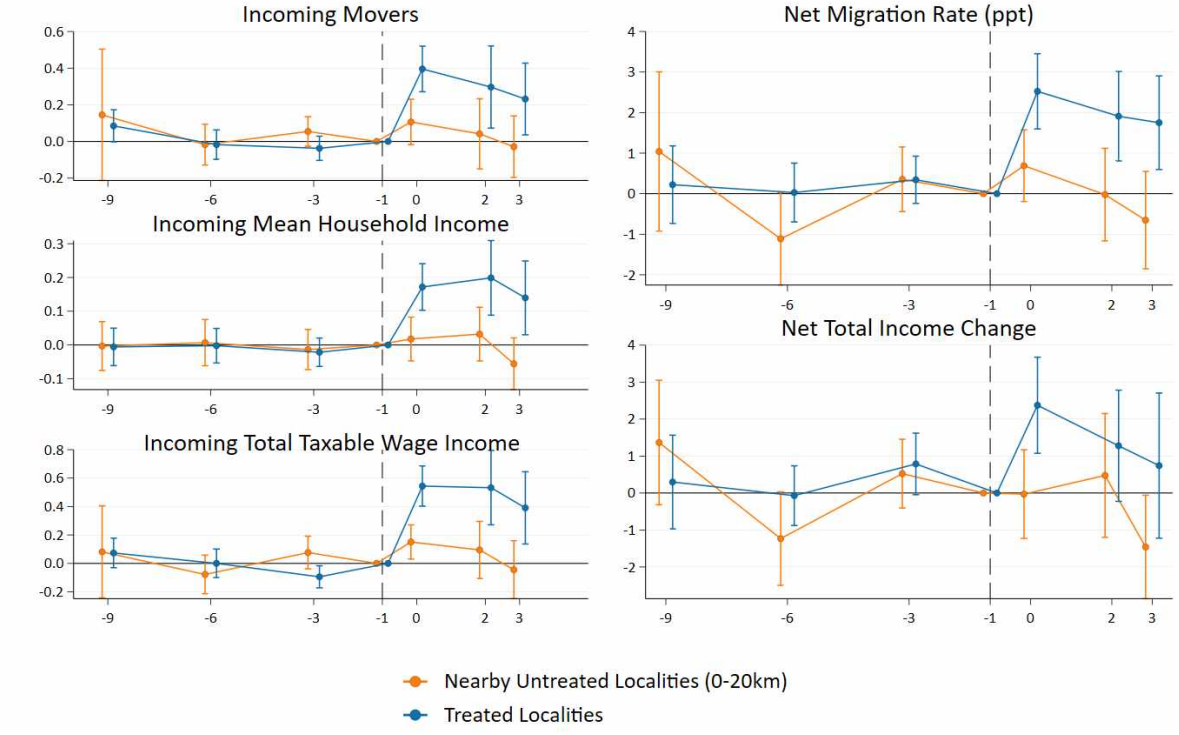
Figure C.1: Heterogeneous treatment and spillover effects among Jewish Localities





Notes: The figures present the estimates from Equation (6) and their 95 percent confidence intervals for the subsample of Jewish localities. I divide localities into low-benefit localities, with a statutory benefit rate of 7–12 percent, and high-benefit localities, with a statutory benefit rate of 13–20 percent. Small localities are those with fewer than 10,000 residents. In the right panel, low/high-benefit localities refer to the localities to which neighboring localities are exposed. The same holds for the Northern/Southern region subsamples. Some of the exposed localities are in one of the Central regions, but within a 60 km travel distance from a treated locality in the Northern or Southern administrative regions.

Figure C.2: Event Study Estimations of Worker Inflows in Jewish Localities



Notes: The figures present the estimation results for Equation (5) using the same dependent variables as Columns (1) and (3) in Table 4. All displayed confidence intervals are 90 percent confidence intervals. For further information, see the notes in Table 4.

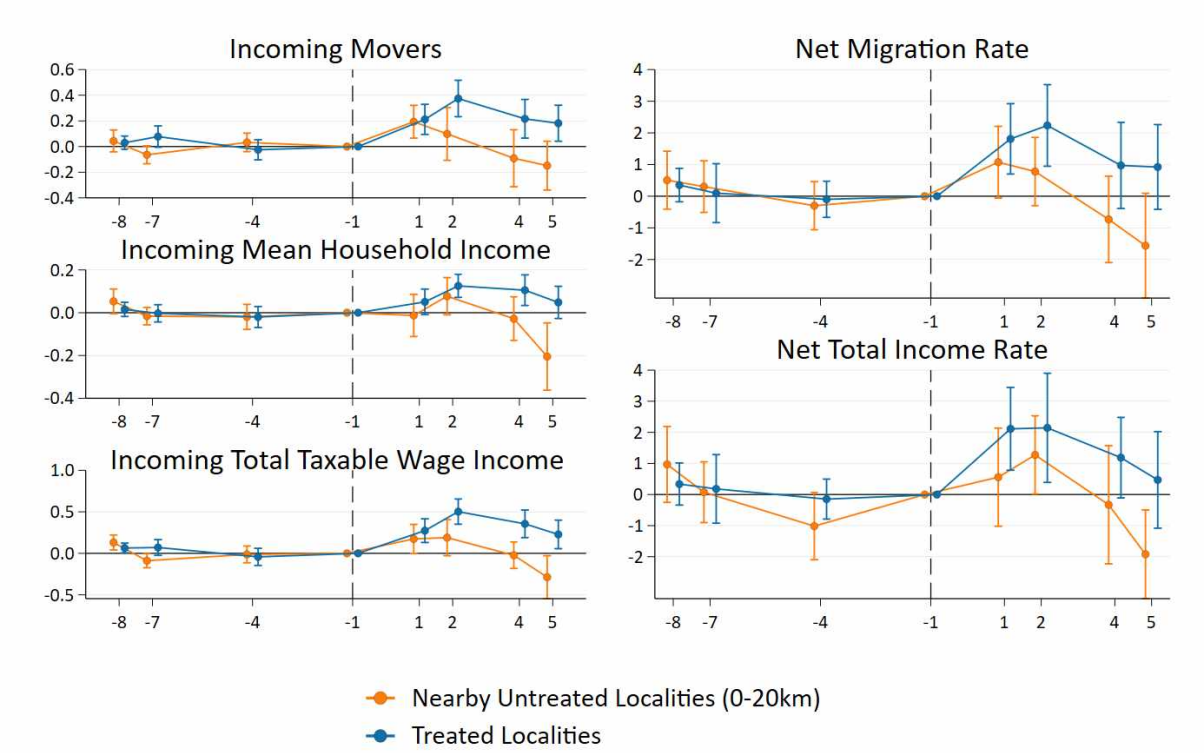
Appendix D

Table D.1: Treatment and Spillover Effects on Composition of Migration (Including anticipation as of 2014)

	(1) Stayers	(2) Incoming	(3) Exiting	(4) Net migration rate
	Log Number			
Treated × Post	0.067**	0.290***	0.140*	1.72***
	(0.033)	(0.059)	(0.058)	(0.58)
0-20 km Neighbors × Post	0.013	0.072	0.081	0.64
	(0.021)	(0.065)	(0.067)	(0.39)
Counterfactual of treated units	726	26.0	22.6	1.66
Observations	6384	6055	5933	6392
Localities	850	846	844	850
	Log Mean Income			
Treated × Post	-0.011	0.089**	0.001	
	(0.027)	(0.027)	(0.035)	
0-20 km Neighbors × Post	-0.021	-0.008	-0.013	
	(0.019)	(0.028)	(0.027)	
Counterfactual of treated units	220,440	166,848	186,904	
Observations	6,383	6,038	5,933	
Localities	850	846	844	
	Log Total Income			
Treated × Post	0.046	0.398***	0.103	1.61
	(0.047)	(0.073)	(0.063)	(0.74)
Neighboring 0-20km × Post	-0.008	0.074	0.024	0.57
	(0.030)	(0.068)	(0.063)	(0.43)
Counterfactual of treated units	69,574,961	2,161,201	2,127,786	1.12
Observations	6,379	5,928	5,752	6,389
Localities	850	844	836	850

Notes: The table presents the results of estimating Equation (4) for the alternative specification that includes anticipation as of 2014. In column 1, the dependent variable is the log of stayers. In columns 2 and 3, the dependent variables are the logs of the incoming and outgoing numbers of workers, representing the extensive margin effect on migration; the log of average household income (measured as the sum of the sampled mover's and spouse's incomes if both are sampled and registered as married to each other), representing the composition effect; and the total income inflows or outflows (the log of the number of movers weighted by their income), representing the total effect. In column 4, the coefficients represent the effect measured in percentage points on the net migration rate and the net income flow into locality i (net addition divided by lagged total population or total income). All estimations include locality and year fixed effects in the first stage, as well as locality ethnicity – time trend, region-specific time trend, locality size group- trend, lagged proportion of children under 18, lagged socioeconomic index cluster, local municipality tax revenue per capita, local spending on education per capita, log number of residential building completions, and distance to nearest train station. The counterfactual mean is computed using the first-stage regression presented in Equation (2). Standard errors in parentheses are clustered at the natural region level. * p<0.1 ** p<0.05 *** p<0.01.

Figure D.1: Event Study Estimations of Worker Inflows with Anticipation



Notes: The figures present the estimation results for Equation (5), using the same dependent variables as Columns (1) and (3) in Table 4. The bars represent 90 percent confidence intervals. For further information, see the notes in Figure 6.